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ASSISTED BY

GEORGE HENRY MAKINS, F.R.C.S.

Under-Secretary

AND

THE SECRETARIES OF THE SECTIONS

IN FOUR VOLUMES

Vol. II.

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INAUGURAL ADDRESS,

BY THE PRESIDENT,

SIR WILLIAM WITHEY GULL, BART., M.D., D.C.L., LL.D., F.R.S.,

Consulting Physician to Guy's Hospital.

GENTLEMEN, FRIENDS, AND COLLEAGUES—I am deeply sensible of the honour conferred upon me in being called upon to preside over this Section, and I offer you my cordial thanks.

Happily the duties will not be arduous, since the general addresses, which have been arranged and admirably allotted, relieve me of the responsibility of attempting to develop before you the actual position of medicine, or the probable lines of its future progress.

As the International Medical Congress assembles in England for the first time, under the auspices of Queen Victoria, so I am reminded that three hundred years ago, under Queen Elizabeth, Bacon enunciated for the first time, in the simplest terms, the position of the student of Nature in relation to the work before him.

Prone as we mostly are to easy satisfaction on imperfect evidence, and to rest in the *experientia fallax*, it is something that we are all agreed, in aim and means, and admit that there is but one source and test of knowledge, "the observation of the order of Nature." We have no principles, but facts; no eclecticism, but of these; and nothing touching the conditions of humanity is foreign to our consideration.

Anatomy, physiology, and pathology have an impersonal and scientific object. Their aim is wide and general. The facts with which they deal are subject to no deflection from affection. Great or small, they have all an equal value. Pathology even denies its name in the presence of that which is universal, merges into physiology, and sees that "whatever is, is right. But far otherwise is it with clinical medicine, where the welfare of the individual alone has to be considered. We call ourselves physicians, and cannot be too jealous of the title and of all that it includes; but we are *Medici*, or curers of disease. Hence, together with the highest duties which science imposes, there are the various personal claims of humanity, augmented by suffering and charged with every disturbing element that weighs upon the heart of man; but at the same time, for us, with every high and quickening motive. These are warping influences, and to correct them we have often with effort to bring our clinical questions in relation with that which is impersonal and above passion.

It is an agreeable fancy on these occasions to suppose that civilization may have been differentiated into its various nationalities, less for the strife of war, than that each nation might contribute, according to its genius, to the progress of the sciences. It may be Utopian to see it thus. Yet a review of the past two or three centuries would suggest as much. That scientific congresses have met, and that they continue to meet, promise better things to come out of the social chaos; as the imagination realises organization springing up amidst the strife of the elements in an early world.

To Italy and the South we owe the early development of anatomy. The illustrious names of Morgagni, Galvani, Scarpa, and others, many of whom have left their names for ever inscribed on our textures, bear early and continued witness of this. And although we Englishmen will ever be tenacious of vindicating for our Harvey the immortal honour of having first demonstrated the circulation of the blood, we equally admit that Italy was his teacher of anatomy. And no less did Italy lead the way in

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morbid anatomy, as testify the pages of Morgagni, in his treatise, *De Causis et Sedibus Morborum.*

To Germany and the North we are largely indebted for analytical progress. Their profound investigations in chemistry and their exhaustive researches into minute anatomy and histology have gone far to solve the problems of organic composition and organic structure. I will not support my position by citing illustrious names. Happily, many whom I should have to mention are still among us; but biological science will never forget Leeuwenhoek and Ehrenberg, or Berzelius and Liebig, nor the labours of the modern schools.

France, with her rare synthetic faculty, seems specially gifted for promoting the science of physiology: I have but to recall the name of Bichat, and to point to the refined investigations of Bernard, and to those of his successors of to-day. And with France I may join Switzerland, whose Haller gave the earliest and strongest impulse to the study of the laws of living things, as a separate science; though, as in the case of Harvey, his lamp was lighted abroad, in the famous school of Leyden.

The English genius is perhaps more fitted for the historical method, and its obvious lessons. But perhaps I ought not to say obvious, for not rarely the English have been satisfied with records without inferences. There are, however, splendid instances of both; of the one in the Museum of Hunter, and of the other in the works of Darwin.

But here you will be ready to exclaim "*Siste!*" for who in the least acquainted with the progress of the biological sciences in different schools at the present time would venture to claim for either some special fitness over the rest for line of pursuit, and when the spirit of each can say, like Goethe's Natur-Geist:—

"In Lebensfluthen, im Thatensturm
Wall' ich auf und ab,
Wehe hin und her!"

Some have prophesied that the advancement of the biological sciences will leave medicine a barren waste in their midst; but such a result, in the natural course of things, cannot happen. There is an indissoluble union between all the sciences, which, for medicine especially, human interest will ever strengthen. The past history and the present state of our profession give us abundant assurance of this. It is not too much to assert that the study of medicine will for all time attract a large proportion of the best thinkers and workers of the world. It has ever been so; and what has been, doubtless shall be in the time to come. Besides, almost every germ of scientific thought has sprung in some way from medicine; and I have only to remind you that some of the most illustrious physiologists and pathologists of to-day are members of our own profession. And if, from the delicacy, intricacy, and the demands made upon all the powers of the intellect by the extent and character of their investigations, they have as it were turned aside from immediate clinical work, they are still so much in union with us that we daily at the bedside avail ourselves of the result of their labours, and gratefully acknowledge that they are our ministering angels, ascending and descending upon the ladder of science in the furtherance of all good practice.

Clinical medicine, however, of itself, affords opportunities for the study of pathology, which are, in some respects at least, unique. Through it, and through it alone, we become acquainted with the first deviations from normal function. From such early beginnings we may trace the development of pathological processes, until the organism is finally, and in different ways, overwhelmed by them. I need only suggest those chronic lesions which spring up from conditions *ab intra.* In the latter stages of these

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degenerative processes, we are apt, without their history, to be so impressed with the more prominent mechanical results, that these would seem to us the original and essential conditions; as, to the Nile-worshipper, the River is a power in itself.

It is well for the progress of clinical medicine that its lines of investigation are thus intimately interwoven with the more scientific departments. It saves us from the dangers of Separatism, and our colleagues from those of Pharisaism; and it quickens our observations where they might otherwise be thought insignificant. If we cannot weigh and measure the data before us, we may still advance the solution of some of the more difficult problems of our condition by critical and exact records. How much has been done in this field of late, especially in cerebral physiology, need not now be told. Every fact to the clinical physician has its value, though it may be of a different order to the phenomena of gravitation. A tone of the voice, the play of the features, the outline and carriage of the body, are to him as invariably related to the central conditions which they reveal, as are the grosser facts of Nature.

The work of the next few days, so far as it is foreshadowed by the list of promised papers, will raise some important pathological questions. You will be asked to consider peripheral lesions, having their origin in nerve-centres—lesions which have for the most part been hitherto chiefly considered primarily humoral and chemical, but now referred to "trophic changes of nerve-origin."

On this point, it may not be uninteresting to notice how "solidism" is widely reasserting itself in the science of living things; not as an *à priori* system, but through the progress of knowledge. The proximate conditions of pyrexia are no longer vaguely referred to nerve, but to definite, nerve-centres; hyperæmia and inflammatory changes to sympathetic lesions; abnormal chemistry to the great respiratory centres; the strange conditions of Addison's disease, with its characteristic pigment, to the supra-renal bodies, themselves probably but nerve-centres, and related, at least by structure, to the system of the pituitary gland; epilepsy, supposed in Hippocratic times to be due to extraneous maleficent spiritual influences, is traceable to apparently trifling changes in a few grey nerve-cells. The specific fever-processes notoriously owe much of their character and intensity to the nervous system. Their relation to time, their occurrence only in warm-blooded animals, the great mortality they cause through nerve-exhaustion, and the immunity they leave behind them, indicate that, whatever may be the nature or mode of operation of their several poisons, it is by implication of nerve-elements that fever obtains its chief clinical characteristics.

Further, in the advance of "solidism," what can interest us more than the recent investigations on contagia? Perhaps no more important step has been made in practical pathology than the proof that some at least of these contagia are organized solids. This discovery, which it has tried the patience, experimental skill, and scientific criticism of the best observers to establish, has brought us at length within view of that which has hitherto been so mysterious. To have been able to separate, though imperfectly, the contagious particles; to have come to the conclusion that no fever-poisons are soluble—is a hope preliminary towards forcing them to yield up the secret of their nature.

If "solidism," as a theory of organic processes, wanted confirmation, we could point to nothing more striking than the present established views on putrefactive changes, and to the amazing fact that the normal textures and fluids of the body resist decomposition unless invaded by microscopic organisms.

May we not hereafter find that all organic chemistry is the resultant of mechanical changes in organic solids?—all Nature, in fact, as Newton asserted, mechanical, but the Great First Cause? Of this we are admonished on all sides. Histology, physiology,

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pathology, clinical medicine, teach us more and more the supreme importance of form and relation.

Lesions extending from alteration of the blood-vessels will also come under consideration. Of course the more common facts relating to aneurism and valvular disease, or such as are thrombotic or embolic, need not be discussed; but there is a contribution which raises the question how far primary, general, arterial tension may be a starting-point, at least in renal pathology.

The etiology of typhoid fever will be raised at one of our meetings. This cannot but enforce a rigid criticism of the infective processes, and of the differences between the states of simple pyrexia, septicæmia, and the specific fevers.

The pathology and treatment of gout, rheumatoid arthritis, and rheumatism, to which, in one form or another, the English seem rather especially prone, will also come up for discussion. Whether they have humoral sources has of late become more and more doubtful.

Of the pathology of acute rheumatism, we may be said to know but little beyond its clinical records and its symptoms; but, unhappily, this has not always been sufficiently recognized, and too often a dangerous poly-pharmacy has rushed in to the cure where science has not yet advanced her foot.

The forms of renal diseases, for a long time included, with little exception, under the term "Bright's disease," will undergo a further degree of analysis. It was a happy omen of this when they moved from the singular into the plural form, "Bright's diseases;" and we may hope now for a more methodical subdivision of them, making their clinical recognition more easy and their therapeutics more precise.

In the matter of diagnosis, we have invited contributions on the pathognomonic and diagnostic value of the localization of disease in the brain and spinal cord, which will be an occasion for a review of our knowledge of cerebral and spinal mechanism, and for further elucidating the pathology of the different conditions of blood, blood-vessels, and connective tissue concerned in the nutrition and diseases of these great nerve-centres. Brain-texture proper seems but little liable to primary disease. As the nervous lamina takes the lead in embryonic evolution, so it would seem that its equivalents in the adult maintain a degree of resistance to morbid change throughout life.

Time fails me to speak of all that we hope to undertake. Any one paragraph of our programme would more than consume the time at our disposal. It must not, therefore, be inferred that the importance attached to any one of the subjects is in proportion to the prominence given to it in this hasty review. The treatment of disease, for instance, is a subject too large and weighty to speak of in general terms. In some minor points it will come before us, as in a paper on the advantage of high altitudes in the treatment of pulmonary phthisis.

An organization such as our own, which it has taken countless ages to evolve, must reasonably require incalculable time for its scientific analysis; and the same may be said of the infinite and varying conditions by which it is maintained, and upon which its existence constantly and immediately depends. At best we know but a few proximate facts, yet these in judicious hands have afforded a good harvest of practical results: what better fruit we may gather when science has penetrated deeper into the laws of our being, and all that affects it, it is impossible to forecast.

In the spirit of the exhortation given by the President of the Congress in his address to-day, and in the slightly altered words of Bacon, with whom I began, let me conclude by saying: "It were a heaven upon earth, to have the mind illumined by knowledge, to move in charity, and turn upon the poles of truth."

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Epileptiform Convulsions from Cerebral Disease.

Dr. J. Hughlings Jackson, London.

For some years before the brilliant researches of Hitzig and Ferrier, I held, as I do still, that the *whole* nervous system is a sensori-motor mechanism. The cerebrum differs from the rest, I consider, only in being the most complicated part of the nervous system. No one denies now-a-days that all mental phenomena have for their physical side or basis active conditions (discharges) of cells and fibres; this, however, is only a morphological statement as to the composition of the highest nervous centres. I submit that the facts of disease warrant the inference, which is strictly in accord with the doctrine of Evolution, that the cells and fibres of all centres are grouped in sensori-motor arrangements—the very highest centres, those most concerned with mental phenomena, being only the most special and complex of these arrangements.

We should endeavour to study diseases of the nervous system, including so-called "Diseases of the Mind," in a brutally materialistic manner. The attention we, as physicians, give to mental phenomena, is given in order that we may study the nature of their physical basis definitely.

I do not see how it is possible to study diseases of the nervous system materialistically, unless we clearly distinguish betwixt psychical states and physical states. Moreover, it seems to me that the occurrence of such symptoms as convulsions from disease of the "organ of mind" (cerebral hemispheres) is incomprehensible unless that organ is made up of sensori-motor nervous arrangements. The view I take is, briefly, (1) that psychical states and nervous states are utterly different, and yet that (2) nervous states are always concomitant with psychical states; (3) that the nervous states are active conditions of cells and fibres, making up sensori-motor arrangements.

As regards some parts of the brain, it may be said that they are "for mind." The proper expression is that they represent or re-represent parts of the body, and are only the anatomical substrata of mind. Popular psychology ignores the fact that movement enters into the anatomical substrata of ideas, volitions, and emotions, or perhaps conciously repudiates that notion altogether, erroneously supposing the averment to be that movements enter into ideas, emotions, &c. The physical basis of a visual idea (or image) is constituted by central nervous arrangements representing certain retinal impressions and certain ocular movements, but the idea (or image) is not an active state of those nervous arrangements; the idea or image arises during, that is to say, in concomitance with, an active state of such nervous arrangements.

No one is fitted to begin the materialistic study of diseases of the brain unless he has a good knowledge of psychology. It is ignorance of psychology which tends to giving metaphysical explanations of certain symptoms, such as that "A patient does not move his arm, because he has lost volition over it." "A patient does not move after a severe fit of epilepsy proper, because he is unconscious." "A patient does not speak, because he has lost the memory of words." These are explanations which explain nothing.

To say that ideas or sensations produce movements is to use neither psychological nor materialistic language, but a mixture of both. Such expressions have the appearance of clearness, but are really incoherent.

From a physiological point of view, nervous symptoms are divisible into two groups. (1) Negative: those owing to a destructive lesion, or to temporary loss of function of nervous tissues; and (2) Positive: those owing to excess of function of nervous tissues—to over-liberations of energy, or, in other words, to hyper-normal nervous discharges.

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I look on a convulsion from disease of some part of the brain as a sudden, simultaneous, or very rapid development of many movements represented in that part, and in associated parts; or rather, a convulsion is a contention in which many movements are "run up" into spasms. There is a sudden and excessive discharge of many nervous arrangements representing movements, at once or nearly together, because the cells of those arrangements have by some pathological process become highly unstable. The discharges are of nervous arrangements which are not ideas, voluntary actions, and emotions, but are the anatomical substrata for these mental states.

The simplest cases for the study of positive symptoms are the convulsions first described by Bravais (1824). The pathology of this class of seizure has been greatly cleared up by Bright, Todd, Wilks, and other physicians. They have recently been specially elucidated by the physiological researches of Hitzig and Ferrier, and by the clinical researches of Charcot, Lepine, Landouzy, and many other physicians. I speak as much as possible of the clinical side, acknowledging, however, that recent important advances in our knowledge of cerebral localization are due mainly to the physiological researches of Hitzig and Ferrier.

In the class of seizures I speak of in this paper, the spasm begins comparatively deliberately in (1) the hand, or (2) in the face or tongue, or in both these parts, or (3) in the foot. This is the order of frequency of place of onset, the onset being usually always the same in the same patient. The evidence from morbid anatomy, agreeing with that of the physiological experiments alluded to, is that there is in these cases cortical disease, and that the part of the cortex affected is within the mid-region of the brain—of convolutions bordering the fissure of Rolando. We call these seizures epileptiform, reserving the term epileptic for cases of the epilepsy of nosologists—epilepsy proper, as it is sometimes called.

Of necessity, whatever special view may be taken as to localization, each of the three kinds of epileptiform seizures depends on disease of some particular part. We make, then, from the three places of onset, three kinds of epileptiform seizures, owing to disease of different parts of the cortex in the mid-region of the brain.

The term "disease" is vague. Whatever pathological condition is discovered post-mortem in such case, the proximate cause of the paroxysm is an abnormally highly unstable condition of some cells, resulting in occasional discharge. The first outward spasm, although sometimes called a "warning," is, really, the evidence of the starting of the internal hyper-normal discharge. Probably, few cells are abnormally highly unstable; in severe seizures the sudden and excessive discharge of those highly unstable cells overcomes, it is supposed, the resistance of healthy cells in physiological connection with those highly unstable.

The ascertainment of the starting-point of the spasm is the basis for methodical localization; or, speaking more simply, we learn from this what it is which it is most important to localize in any case under observation. If one patient's fits always begin in his thumb, and another patient's always begin in his little finger, the elements of the cortex highly unstable (over dischargeable) in the two cannot be quite the same, however closely they may lie together in the cortex cerebri.

In the study of these cases we have to consider (1) the starting-point of the spasm; (2) the range of the spasm, how far it spreads; (3) the march of the spasm, the order of spreading; (4) the suddenness of onset, rapidity of spreading, and duration of the paroxysm; (5) the post-paroxysmal condition (temporary paralysis); (6) post-epileptic aphasia; (7) affection of consciousness; (8) we have to ascertain the particular part of the cortex diseased; (9) the physiology of the lesion; (10) the pathology of the lesion; (11) treatment.

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1. *Starting-points.*—We have already spoken of three starting-points. Those seizures starting in the hand are clinically analogous to cases of hemiplegia, in which the arm suffers more than the leg; those starting in the side of the face are probably clinically analogous to cases of partial aphasia, with slight and transient hemiplegia. Those starting in the foot are clinically analogous to cases of hemiplegia, in which the leg suffers more than the arm.

The important matter, with a view to localization, being the starting-point of the spasm, our clinical study of it must be minute and precise. When the spasm starts in the hand, it is most frequently in the index finger, or thumb, or both; when in the face, it begins near the mouth, or in the tongue, or in both these parts; when in the leg, in the foot, and the starting is nearly always referred by the patient to the great toe.

In some cases, if not in most, there is premonitory numbness, tingling, or some other abnormal sensation in the parts first seized by spasm; evidence, I think, that the centres engaged are not solely motor. In some cases, almost contemporaneous with the onset of the spasm, there are colours before the eyes; occasionally, but very rarely, there is a stench in the nose. In one case, where the spasm started in one leg, there was a noise in the ear of the same side. For brevity, these important facts are omitted from consideration.

2. *Range.*—It is certain that there are many ranges of the spasm, from, for example, slight spasm, apparently limited to the thumb and index finger, to universal convulsion. It frequently happens that a patient has very limited convulsion, of which he may take no notice, before he has widespread ones. The same patient may have, on different occasions, convulsions of widely different ranges.

A seizure, in which the spasm is seemingly limited to the thumb and index finger, is not a different kind of fit from one so beginning and becoming universal; they are but two vastly differing degrees of one kind of seizure. This, obviously, bears on localization. It means, that a discharge, starting in some cells of a small part of the cortex, may be very limited, or, no doubt, by overcoming the resistance of physiologically associated healthy nervous arrangements, may spread widely. Of necessity, the spasm depends on liberation of energy, and it is supposed that the degree and range of convulsion varies as the quantity liberated by unstable cells, and the rapidity of its liberation.

We may, although the division is of necessity largely arbitrary, speak of ranges thus; (1) Monospasms (of arm, or of face, or of leg). These are clinically analogous to Charcot's monoplegias from cortical destructive lesions. (2) Hemispasm, in two degrees. There is (*a*) convulsion of the face, arm, and leg, analogous clinically to chronic cases of hemiplegia; (*b*) such convulsion with, in addition, turning of the eyes and head to the side convulsed and some spasm of respiratory muscles of both sides, the analogue of cases of hemiplegia described by Vulpian and Prévost, in which the eyes and head turn from the side paralyzed. (3) There is a still further range; part or the whole of the other side of the body is sometimes gained, the convulsion becomes universal. This range, I suppose, is clinically analogous to cases in which, upon a vast one-sided cerebral lesion, such as a large hæmorrhage, the patient moves none of his limbs on either side. It is, however, asserted by some physicians (the patient in these cases of large cerebral hæmorrhage being unconscious or comatose) that he does not move "because he is unconscious." This is only a verbal explanation. It is certain, from the facts of "descending wasting," that both sides of the body are to some extent represented in each side of the brain, and it is a legitimate hypothesis that discharge beginning in part of one-half of the brain, and not reaching the other half, can produce some convulsion of the same as well as of the opposite side of the body. This hypothesis does not say that the opposite half of the brain may be reached: but is, that discharge limited to one half can convulse

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both sides of the body. In this connection Broadbent's well-known views as to representation of unilateral and bilateral movements in the brain should be considered. These views will be referred to again later.

3. *March of Spasm.*—Clinically we have for localization, not only to note the range attained, but to observe the method of spreading over that range. For in localization we have not simply to ascertain the movements chiefly represented in any part of the cortex (presumably those of the part in which the spasm starts), but all the movements represented in it or in other parts in physiological connection with it, those united with the part primarily discharged by lines of least resistance. It is not enough to say of a seizure, for an example, that "the arm was convulsed;" we have to note whereabouts in the limb the spasm starts and how it spreads in it. In one case of convulsion of the arm the spasm spread down the limb from shoulder to finger in every fit I saw (and I saw many). In other cases of spasm limited, or nearly limited, to the arm, it spreads up. We must not use the unqualified expression, of two cases, "in each the right arm was convulsed," when the spasm in one "went up," in the other "went down." Manifestly two different parts of the cortex must have been diseased in these different spreadings of spasm, although in each it was over the same limb; the movements in each must at least have been represented as differently grouped. Whilst the starting-point of the spasm is the clue to the seat of the lesion, and whilst the range of it is a clue to the movements, the diseased centre represents, and to movements represented in nervous arrangements physiologically connected by lines of least resistance with it, the order of spreading is our clue to representation of movements in relations of subordination or sequence one to another—their time relations. Suppose, for the sake of argument, all the parts of one side were equally convulsed in two cases of hemispasm; yet, if the spasm started in the hand in one and in the foot in the other, the parts of the cortex primarily discharged must be different. If in cases of hemispasm the seizure begins in the arm, the rule is that the spasm goes up the arm and down the leg; if in the foot, up the leg and down the arm; to the latter there appear to be numerous exceptions.

As stated, the spasm, in some cases, after affecting what we shall call the "first" side, gains the other side of the body, which we shall call the "second" side. I presume that the spasm of the "first side" is caused in part by intermediation of the decussating cerebro-spinal fibres, which in the cord lie in the lateral column; the "second side" I have suggested is convulsed by the intermediation of the direct fibres in the anterior column. Pitrés and Franck have shown that bilateral convulsions may occur from excessive irritation of one cerebral hemisphere, even when the "motor centres" of the other have been extirpated. On Broadbent's hypothesis it may be said that at least part of the convulsion of the "second" side is owing to active conditions of the decussating fibres putting in action the associated nuclei of both sides of the cord, and thence the bilaterally acting muscles of both sides of the body. On either hypothesis one would infer that the convulsion of the "second side" would not be a facsimile of that of the "first side." However, Ferrier says that the artificially induced bilateral convulsion in a monkey from irritation of a part of the cortex cerebri of one hemisphere is the same on both sides of the animal's body. Apart from any hypothesis, we have, with a view to localization in man, to note how the "second side" is affected. In some cases only a part of the second side is affected, and usually, I think, the leg when the earliest part of the seizure on the "first side" was of the arm. I would ask the following questions, with a view to further observation. Of course we note the particular part of the "first side" which is first in spasm. In regard to the following questions, it is supposed to begin in the right hand; the right is the "first side," and the left the "second side." (1) Does the spasm of the left side begin in the leg? (2) Does it go *down* or *up* these limbs? (3) Does it

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affect the extensors more than the flexors? (4) Is the spasm more tonic than that on the "first side?"

The spreading of spasm in these convulsions has to be considered from another point of view. It is not in simple order; the spasm does not leave one part when the next is attacked. It is in compound order. In cases of slowly spreading spasm, we easily see that the part the seizure begins in is at first a little in spasm, next it is more in spasm, and other parts are in spasm also. There is the double difference, (1) more spasm, and (2) greater range of spasm. This is best seen in convulsions starting in one side of the face. Speaking roughly, there is first spasm of the muscles of the side, so far as their movements are chiefly unilateral; and then, before this is over, there is the compound spasm of the unilaterally acting muscles of one side, and the bilaterally acting of both sides. We may see in this complex struggle that the mouth becomes ovoid; the wider part being to the side first in spasm. Such cases are strictly in accord with Broadbent's hypothesis. The order of spreading in these convulsions may be rudely symbolized as x, then x^2y, then x^3y^2z, and so on.

In some cases, after rapidly recurring convulsions, the part which in the earliest attacks was first seized is not affected; the fit begins at what would be a later stage of the earliest seizures. So far, for a series of rapidly recurring attacks. But in some cases, where the fits occur at long intervals (perhaps from destruction of part of the cortex corresponding to the parts which in early seizures were first in spasm), the parts first convulsed in the patient's earlier fits cease to be convulsed in his later ones.

4. *Suddenness of Onset, Rapidity and Duration of Spasm.*—It is important to note the suddenness with which spasm sets in, the rate of its spreading, and the duration of the paroxysm.

I think the more suddenly the spasm sets in, and the more rapidly it begins to spread, the greater range does the convulsion ultimately attain, and the sooner over is the paroxysm. On the contrary, when the spasm starts deliberately, and spreads slowly, it is more likely to be limited in range, say nearly limited to one arm, and to be lengthy. The bearing of this will be best seen when we come to speak of post-epileptiform paralysis. The duration of seizures varies exceedingly—from a minute or two, to more than two hours. I am not here speaking of repetition of fits at short intervals. General remarks on duration, however, are necessarily indefinite, for we should consider duration of seizure and range of seizure together.

5. *Post-Paroxysmal State* (*Temporary Paralysis*).—With a view to more precise localization, or rather for clearer interpretation of the particular effect any lesion discovered has, we must consider very carefully some relations of convulsion and paralysis.

The fact that after an epileptiform seizure the patient may be temporarily and locally paralyzed is one of extreme importance. There may be complete paralysis, which paralysis is temporary unless the fits recur. Some preliminary remarks are needed before we consider this relation or sequence. Let us put the question we are thus concerned with in a particular way.

When in a case where a cortical lesion is found post mortem there had been any paralysis, it is a matter of great moment to note whether or not that paralysis was after convulsion and temporary; or, if it had been permanent, whether the convulsions were rapidly repeated. For not denying (on the contrary, after Ferrier's and Charcot's researches, affirming) that cortical destructive lesions produce paralysis, and admitting too that such paralysis might be temporary, it still remains that a few hours' complete paralysis after a convulsion belongs to a different category; it cannot be ascribed to the destructive effects of the gross lesion discovered post mortem. A man may be seemingly

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well; he has monospasm, then he is monoplegic, and in a few hours, unless the fit recurs, he is seemingly well again.

In this inquiry we are not concerned with paralysis occurring after convulsion in cases of meningeal or cerebral hæmorrhage, ordinary, or from vascular tumour, nor with cases of so-called uræmic convulsion; we deal with chronic cases only. (*a*) Spasm may begin in parts already partially paralyzed (hemiplegic), or which have seemingly recovered from hemiplegia, the paralysis having set in without convulsion. (There is often a condition for embolism in these cases.) (*b*) It may begin in, and affect severely, a part which, after recent prior convulsion, is partially or even absolutely paralysed. (*c*) Some cases are complicated. We have carefully to distinguish betwixt slowly creeping paralysis from destructive changes and the temporary paralysis after a convulsion. (*d*) There may be the complication of slow paralysis and occasional temporary paralysis after seizures. (*e*) In intra-cranial syphilis we may first have convulsion, followed by temporary paralysis (cortical gumma), and then the very different thing, hemiplegia from blocking of a syphilitically diseased artery. (*f*) There may be indirect evidence of paralysis, as well as direct; an arm may, when the patient tries to use it, move in a way closely like that in disseminated sclerosis. Such erratic movements are to be distinguished from quasi-spontaneous little spasms, jerks, "jumpings," &c., and from increased reflexes (?) on disturbing the parts. (*g*) A patient may cease to be subject to sharp-cut paroxysms beginning in the hand, and become liable to over-movements of the arm and hand for hours or days, movements like, but more gliding than, those of chorea.

Admitting these complications and difficulties, only ordinary temporary paralysis after convulsion, beginning in a patient who, before the seizure, had no obvious paralysis, will be spoken of in this paper. I now make three remarks on such cases. (*a*) The paralysis is, in my experience, always of the parts first and most convulsed. (*b*) It varies in degree, from such as mere inability to pick up a pin, to absolute powerlessness of the hand and arm. (*c*) It varies greatly in range, from paralysis of the hand to hemiplegia, with (Julius Mickle) lateral deviation of the eyes, and probably to greater ranges still.

We have such a sequence as the following to consider: (*a*) A man is seemingly well. (*b*) His leg is convulsed strongly, and the arm slightly, for about ten minutes. (*c*) The leg is much paralysed for a few hours. (*d*) He is seemingly well again. When a cortical lesion is found, we have to note whether any paralysis there had been was after a convulsion or not.

Various Hypotheses as to the Nature of the Paralysis.—(*a*) Post-epileptiform local paralysis has been ascribed to congestion of the brain during asphyxia produced in the prior paroxysm. Besides the difficulty of understanding how uniform brain congestion can produce localized paralysis, there is the fatal objection that absolute local paralysis may remain after a seizure in which there has been no asphyxia. (It may be mentioned, too, that the occurrence of clonic spasm in such cases shows that cerebral congestion is not essential for the production of that kind of spasm.) Thus, during spasm of an arm, leaving the limb paralysed, the patient may stand all the while and talk to bystanders, have no affection of consciousness, and no trouble of respiration. (*b*) Another hypothesis ascribes the temporary paralysis to extravasation of blood. Not denying here that temporary paralysis might, in other kinds of cases, be owing to small clots, there is the objection that in temporary monoplegias after monospasms there is nothing to cause such extravasations. Further, it may be urged that it would be very remarkable were the extravasation in each succeeding paralysis always to affect that part of the nervous system which especially empowers the part seized in the prior convulsion. Besides, if cerebral congestion or extravasation were producible by these paroxysms in

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such a degree as to cause local paralysis, we ought to find local paralysis after the very much severer paroxysms of epilepsy proper, and we do not find it. (*c*) It seems to me that Todd's and Alexander Robertson's hypothesis accounts best for the facts. This hypothesis is that temporary paralysis, after a convulsion, indicates not simply that discharged cells have "run down," but exhaustion in nerve tracts between the cortical lesion and the muscles convulsed. It is manifest that during the convulsion there is an excessive activity from the cortex through some nerve tracts to the muscles convulsed—cortex in so-called motor region, internal capsule, nerve fibres in cord, anterior horns, nerve fibres of nerve trunks. It is reasonable to infer *à priori* that any subsequent and yet temporary local paralysis is owing to exhaustion of nerve fibres produced by that hyper-normal process, especially when the paralysis is found where the spasm had been and is temporary. At any rate, there is some negative condition of the nervous system which, being temporary, cannot be put down to any local disease discovered post mortem.

It has been asserted that there is no proportion betwixt the degree of the seizure and the degree of the post-epileptiform paralysis. I admit that after the severest fits there may be less paralysis than after the slighter ones, if we are to take note only of decided local paralysis, of which so far alone I have spoken. But I think there may be more paralysis after the severest seizures, if we take count of what goes by the name of "weakness." There are many ranges of post-epileptiform paralysis. We have spoken of monoplegias. Long ago Todd described what he called "Epileptic hemiplegia."

If spasm begins deliberately, and spreads slowly, it is more limited in range and continues for a longer time. After such paroxysms, the paralysis also is limited in range—there is obvious local paralysis. If spasm begins suddenly, and spreads rapidly, it may become universal; and then there may be, after the paroxysm, no, or but little, obvious local paralysis but yet widespread slight paralysis—a condition spoken of as one of "weakness." Let us for one moment consider a patient's condition after a severe seizure of epilepsy proper.

I submit that there is often paralysis after even severe seizures of this kind, although there is no decided local paralysis. Since the patient is, after such fits, unconscious, it may be asserted that he does not move, "because he is unconscious." This is a verbal explanation, identical in kind with the explanation that a man monoplegic, after monospasm, does not move the part affected, "because he has lost volition over it;" these explanations explain nothing. Our explanation of materialistic conditions must be brutally materialistic. Whenever a man is unconscious, there is some abnormal physical condition of some part of his nervous system, and it seems to me more reasonable to put his immobility down to this. Besides, I think paralysis is admitted to exist after paroxysms of epilepsy proper, by the statement that the patient on recovery from loss of consciousness is "prostrated," &c. If, so to speak, as much paralysis as constitutes immobility of one arm for four hours could be "spread out thin" all over the body, it would not then be called paralysis, but "prostration," &c.

On Reflexes after Epileptiform Seizure.—There is a further field of investigation as to the nature of post-epileptiform paralysis. Observations on the condition of reflexes after epileptic seizures have been made by Westphal. After one convulsion I saw, which began in the foot and affected the left leg chiefly, and the left arm slightly, there was very great paralysis of the leg, and slight paralysis of the arm. There was greatly exaggerated knee-jerk, and there was foot clonus on the paralysed side during the paralytic stage. These conditions were not found on recovery. In some other cases of post-epileptiform paralysis observed for me by Mr. Neatby, the conditions were too complicated for brief analysis. For the most part they were in harmony with the observation just

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cited. We should consider the condition of reflexes after epileptic paroxysms. In some cases, immediately after epileptic attacks, there is, Dr. Beevor has ascertained for me, exaggerated knee-phenomenon, and also foot-clonus. But Westphal has found the knee-jerk sometimes absent after epileptic seizures, and so has Gowers in one case he has briefly reported; in that case it was absent for a minute or less. Hence there are discrepancies.

The cases of post-epileptiform paralysis, besides differing in being temporary, differ from paralysis in ordinary cases of hemiplegia from clot and local softening, in that in the former there has been some active process through the cord during the paroxysm, whereas in the latter the cord becomes affected after a time, and slowly. On Todd and Robertson's hypothesis, we might say that a temporary negative condition in the cord was established when the paralysis appeared, and explain the absence or presence of the deep reflexes on the degree or range of that negative condition. Gowers has suggested that, in the case he mentions, there may have been temporary exhaustion of the lumbar nuclei; in my patient it may be that the knee-jerk would have been found absent had I tested for it instantly when the spasm ceased. The subject is a complicated one; in my patient's case, as in others, little spasms came on now and then "spontaneously," and in some cases touching a limb after a fit will make it jerk. There are other complications. We have particularly to bear in mind that there may be some persisting paralysis from permanent destructive lesion of the cortex, as well as, in addition, some temporary paralysis after a seizure; and thus there might be increase of the reflex mentioned at all times, although perhaps they would be more marked after a paroxysm. I have found foot clonus in one case several weeks after a patient's last seizure, and at a time when there was no obvious paralysis; the patient walked well and felt well. I would suggest that we should most carefully note, not only the region and range of post-epileptiform paralysis, but also the condition of reflexes instantly on the cessation, and at various times up to and after recovery.

6. *Post-Epileptiform Aphasia.*—In some cases (all right-sided, so far as I have yet personally observed) there is, after the seizure, temporary partial aphasia. I have but once known this so-called complication to occur after fits beginning in the foot. Perhaps that case was exceptional, as there were found post mortem, months later, large blood cysts, which had compressed each hemisphere from front to back. Aphasia is certainly most frequently found after fits beginning in the hand, or in the side of the face, or in the tongue. It may occur when the spasm is limited to the cheek and is trifling in degree. These cases must not be confounded with cases of so-called emotional aphasia. There may be temporary, partial, or even complete aphasia without any spasm, for example, in patients with valvular disease of the heart; these are probably cases of very limited softening from embolism, I, however, limit myself to cases in which there was clear evidence of precursory local (right-sided) spasm. In some cases the aphasia is discovered after recovery from loss of consciousness following severe epileptiform seizures; in others there is no loss of consciousness in the prior seizure.* My belief is, that when the fit begins in the face or tongue, or both, the defect after it is rather ataxy of articulation, a jumbling of syllables; and that when it follows a seizure starting in the hand, it is more often one in which words are well uttered, but either mistakenly, or only in short, well-organized sentences, such as "very well." It must be borne in mind that in some cases of post-epileptiform aphasia, as in some cases of aphasia otherwise caused, there is

* It is worth noting, that in many such cases, when the spasm affects the face, the patient will say that his arm on the same side "falls dead." Gowers suggests that epileptic discharges sometimes inhibit.

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inability to put out the tongue when told, although the patient moves it well in eating, swallowing, and puts it out "by accident."

Temporary aphasia, after epileptiform seizures, may exist with temporary paralysis of the arm, or of one side of the body. The presumption is, that the internal negative local condition is the same both for the aphasia and the paralysis.

In considering the explanation of post-epileptiform paralysis we ought to consider it along with post-epileptiform aphasia, with which, as said, it sometimes occurs, and also with post-epileptic conditions. It seems to me that Todd and Robertson's hypothesis gives the simplest explanation of post-epileptiform paralysis, of the physical condition of the negative element of post-epileptiform aphasia, and of the physical condition in post-epileptic loss of consciousness. Of course the erroneous and limited utterances of the aphasic are not supposed to be owing to local nervous exhaustion—that is, supposed to account for his inability to say anything else or better. And in a case of post-epileptic mania, local exhaustion is only supposed to be the physical condition for the loss of consciousness accompanying the maniacal action. It is impossible that a negative condition can cause the positive manifestations in a case of aphasia, or those of mania after an epileptic seizure.

7. *On Affection of Consciousness.*—So far we have said nothing, excepting incidentally, about cessation of consciousness. In some epileptiform seizures it is lost and in some not. It is lost in severe cases; it is not possible for me to use a more precise expression. There may be spasm limited to the arm or to the leg, or affecting one arm and leg, and even the face also, without any evidence of even defect of consciousness; the patient converses with us about his condition. Roughly speaking, consciousness usually ceases when the head and eyes begin to turn after the limbs of one side have been affected. I suppose that the more rapidly the spasm sets in and spreads, the less is the range attained before consciousness is lost.

It is worth while to speak of the difference betwixt epileptiform seizures and epileptic seizures, with regard to negative affection of consciousness.

The difference is not that in the epileptiform paroxysm consciousness is not lost, and that in the epileptic it is lost; but that in the former it is lost late in the paroxysm, whilst in the epileptic it is lost, or is defective, first thing, or nearly first thing.

8. *Localization of Lesion.*—The observations made, recording local lesions found after death in different cases of epileptiform seizures, are now very numerous. The cases I have recorded have for the most part been cases of tumour, and the disease has been too often so wide that most of them are not of much beyond clinical value. These cases have been many times quoted, and I need not refer to them. Nor need I give an account of other observations; they are very numerous, and many of them have been already collated by Ferrier and others.

9. *The Physiology of the Lesion.*—It is a truism to say that a healthy movement implies a liberation of energy or nervous discharge—initially by cerebral cells, at any rate if the movement be a voluntary one. A convulsion—that is to say, a sudden, excessive, rapid, and temporary development of movements—many movements "run up" into spasm—implies of necessity a corresponding sudden, &c., discharge. A local convulsion, if only local at the onset, implies that the excessive discharge begins locally. I say begins locally, for I suppose the "discharging lesion" to be, so to speak, "fulminate," which, by the rapidity of its "decomposition" discharges healthy associated (collateral and lower) nervous arrangements. When fits (always of the same style) recur, although often in different degrees and ranges, the inference is that there is persistent change in some cells in one locality, such that they occasionally attain high instability and occasionally discharge excessively.

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10. *The Pathology of the Lesion.*—The question in pathology is, What is the process by which cells become highly unstable? Without pretending to determine the steps of it, we may say that the primary change is frequently tumour, and very often a syphilitic tumour. In some cases I have failed to discover, have no doubt overlooked, a local lesion. It is not necessary to go into the evidence as to the pathology of these cases here. The diagnosis of the nature of the lesion is a general question in diagnosis, not one about these seizures in particular. I refer to but two things. From my earliest studies (*London Hospital Reports*, vol. i., 1864) I have been struck by the occurrence of epileptiform seizures in some cases of complete or partial recovery from hemiplegia, presumably due to embolism (sometimes occurring in young patients who have valvular disease of the heart), and in some patients with valvular disease without previous hemiplegia. But in no such case have I had a post-mortem examination. It is then but an hypothesis that plugging of vessels may lead to instability of cells. The empirical association is, I think, too frequent to be a mere coincidence. So far as the process produces softening, so far, of course, it produces a condition from which spasm, or any other positive symptom, is impossible.

Sometimes a patient who has had an injury to the head, followed by general brain symptoms (loss of consciousness, &c.), recovers, except that he remains liable to convulsive seizures. And sometimes the seizures are of the class we are considering.

I am convinced that in some syphilized patients a blow on the head is followed by cortical syphilitic disease. Dr. Clifford Allbutt long since asserted this. It ought to be borne in mind when we consider the propriety of trephining in cases of epilepsy or of epileptiform seizures after injuries to the head.

In one case already referred to of seizure beginning in the right foot there were found post mortem two very large blood cysts, one over the left, the other over the right cerebral hemisphere.

11. *Treatment.*—It is not necessary to go into the question of treatment with regard to these cases for the obvious reason that the question of their treatment is but part of the question of treatment of various morbid conditions of brain. There are certain valuable empirical methods of treatment by drugs too well known to need pointing out. Everyone knows that a ligature and certain other procedures allied in effect will sometimes stop epileptiform seizures. No one fails to conceive the possibility of syphilis in such cases, and to treat the patient anti-syphilitically when necessary. The question of trephining in some cases of epileptiform seizures is very important, but is too large a one for me to consider here; besides, I have had no personal experience of it. In some cases of epileptiform seizures, after injuries to the head, there is depression of bone; there is now in the London Hospital a patient who has such a depression consequent on an old injury in the left postero-frontal region; he is subject to seizures beginning in the right arm.

Zur Jackson'schen Epilepsie und Localisation des Armcentrums (mit Illustrationen eines Falles von isolirter und circumscripter Convexläsion).

Dr. Franz Müller, Graz.

Gestatten Sie mir hier in aller Kürze eines der interessantesten Symptome, welches für die Localisation der Cerebralerkrankungen von höchster Wichtigkeit ist, näher in's Auge zu fassen, ein Symptom, das zuerst von Jackson (dem grossen Neuropathologen

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Englands) aus den mannigfaltigen und höchst verschiedenwerthigen cerebralen Convulsionen herausgeschält und eingehend gewürdigt wurde.

Bei organischen Hirnkrankheiten kommt es bekanntlich unter gewissen Bedingungen zu mehr oder weniger intensiven, in Paroxysmen auftretenden klonischen Zuckungen, die gewöhnlich auf einzelne Muskeln und Muskelgruppen oder Extremitäten beschränkt bleiben, und bei jedem neuen Anfalle mit wahrhaft neckischer Genauigkeit fast stets in denselben Muskeln wiederbeginnen.

Ein Grundzug derselben ist die Unilateralität. Nur höchst selten ergiessen sie sich von der gewöhnlich befallenen Körperhälfte aus, auch auf die andere. In Folge der angegebenen, wesentlichen Merkmale nennt man diese Anfälle Epilepsia unilateralis oder Hemiepilepsie, oder, zu Ehren ihres Entdeckers, Jackson's Epilepsie.

Eine genaue Analyse von achtzehn selbst gemachten, einschlägigen Beobachtungen, sowie der, in der Literatur hinterlegten Fälle zeigt aber, dass die Jackson'sche Epilepsie den Namen "Epilepsie" eigentlich mit Unrecht trägt. Es fehlen ihr nämlich zwei wichtige Kriterien als epileptische Störungen, wobei wir natürlich nur den motorischen epileptischen Krampfanfall im Auge haben.

Erstens die tonischen Krämpfe, und zweitens, was uns besonders wichtig erscheint, die Bewusstseinsstörung.

Der Anfall setzt sich nämlich ausschliesslich nur aus klonischen Zuckungen zusammen; und mögen diese auch noch so intensiv und extensiv sein, das Bewusstsein wird dadurch nicht aufgehoben, wenigstens nicht zu Anfang des Anfalles. Im Gegentheil der Kranke beobachtet selbst das Einsetzen der Zuckungen, verfolgt ihre allmälige Ausbreitung und sucht Hilfe gegen dieselben. Er ist, mit einem Worte, der *spectator sui acti.*

Ferner möchte ich als drittes Differenzirungsmerkmal hervorheben, dass die Jackson'sche Epilepsie niemals plötzlich mit der vollen Intensität, wie die genuine Epilepsie einsetzt, dass sie auch nicht mit einem Male abgeschnitten wird. Wir finden vielmehr ein allmäliges Einschleichen und relativ langsames Anschwellen und Abklingen der motorischen Reizerscheinungen.

Dabei zeigen Anfälle, mögen sie auch in der Cortege der verschiedensten und buntesten Symptome auftreten, eine gewisse Uniformität, um nicht zu sagen, Monotonie.

Eine weitere, klinische Eigenthümlichkeit dieser unilateralen Convulsionen ist die, dass sie stets eine mehr oder weniger prononcirte Schwäche und Parese, ja sogar Paralyse der befallenen Extremität von anfänglich allerdings nur ephemerer Dauer, zurücklassen; andererseits aber schon frühzeitig eine leicht nachweisbare, permanente Steigerung der Sehnenreflexe setzen. Letztere sind das sicherste Signal, dass über kurz oder lang die, bisher stets nur flüchtige Parese und Lähmung in eine bleibende übergehen wird.

Ferner scheinen, wenigstens nach meinen Erfahrungen, oculo-pupilläre Symptome eine sehr häufige Begleiterin der unilateralen Krämpfe zu sein. In 15 unter 18 Beobachtungen war auf dem gleichseitigen Auge: Verengerung der Lidspalte, und zwar in 13 mit gleichzeitiger der Pupille, in 2 mit Erweiterung derselben vorhanden. Auf Beleuchtung und Beschattung reagirte die verengte resp. erweiterte Pupille stets ziemlich prompt, wenn auch nur mit sehr geringer Bewegungsbreite. Die Verengerung der Lidspalte war im Allgemeinen niemals so hochgradig, wie man sie bei gewöhnlicher Lähmung des levator palpebrae zu constatiren Gelegenheit hat. Der Bulbus selbst war in sieben Fällen kenntlich tiefer in der Augenhöhle gelegen (retrudirt), die Prüfung natürlich bei vollkommenem Schluss der Lidspalte vorgenommen. In den übrigen acht Fällen schien keine laterale Verschiedenheit in der Lage vorhanden zu sein, wenigstens konnte eine solche nicht mit Sicherheit constatirt werden. Sonst liess sich in der motorischen Innervation der Augen nichts Pathologisches nachweisen; ebenso war auch der ophthalmoscopische Befund stets ein negativer, und die centrale Sehschärfe, sowie die Grösse

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des Gesichtsfeldes niemals alterirt. Ich betone dies nachdrücklichst, weil Westphal auf Grund einer gemachten Beobachtung von Hemiepilepsie mit Hemianopsie die Vermuthung ausspricht, dass diese Vergesellschaftung öfters vorkommen dürfte. Wiederholt vorgenommene Temperaturmessungen in den Gehörgängen, sowie im Gesichte, waren nicht im Stande, eine Differenz zwischen beiden Gesichtshälften nachzuweisen. Auch zeigte sich in der Gefässfüllung keine laterale Verschiedenheit. Es standen demnach die oculo-pupillären Symptome stets ohne vasomotorische da.

Hingegen waren psychische Aenderungen fast ausnahmslos, in acht Fällen sogar sehr deutlich vorhanden. Sie bewegten sich grössentheils im Rahmen elementarer, namentlich affectiver Anomalien, als: grosse Gemüthsreizbarkeit und Zornmüthigkeit einerseits, Gemüthsweichheit und grosse Neigung zum Weinen andrerseits. Bei den meisten konnte auch Gedächtnissschwäche constatirt werden.

In drei Fällen constatirte ich, bei jedem grösseren Krampfanfalle im rechten Arme, gleichzeitig Aphasie.

Die Pathogenese der hemilateralen Convulsionen ist heute ziemlich klar und allgemein bekannt.

Solange die motorischen Rindenzellen sich im Zustande des Erethismus in Folge des Reizes einer organischen Hirnläsion befinden, werden bei momentaner Steigerung der Reizgrösse contralaterale Convulsionen auftreten. Sind die motorischen Rindenpartien aber im Zustande der Desorganisation, der Zerstörung und der völligen Vernichtung, dann wird Lähmung die eherne Consequenz sein. Dass die psychischen Functionen alterirt werden, nimmt nicht Wunder, da es sich ja um Rindenläsionen handelt. Auch die temporäre Aphasie bei Convulsionen der rechten Extremitäten, namentlich der oberen, erklärt sich ungezwungen aus der Nähe des Sprachcentrums.

Die eigentliche Genese, sowie den Auslösungsort der so häufig von mir beobachteten oculopupillären Störungen klar zu legen, ist man heute, wie ich glaube, noch nicht im Stande, und dürfte es in der That schwer fallen, eine nur irgend zutreffende und einwandsfreie Hypothese dafür aufzustellen. Ich verspüre auch keine Lust, mich in das Reich luftiger und auf schwachen Füssen ruhender Hypothesen zu verlieren. Doch glaube ich aus meinen Beobachtungen den Schluss sicher ziehen zu dürfen, dass es sich nicht um Innervationsstörungen von Seite des Oculomotorius, sondern des Sympathicus handelt. Der Sympathicus innervirt bekanntlich ausser dem dilatator pupillae noch den von H. Müller zuerst entdeckten und von Sappey bestätigten Musculus orbitalis (obturator der fissura orbitalis inferior), sowie die glatten radiär in dem Lidern verlaufenden und die Lidspalte erweiternden Muskelfasern. Lähmung dieser Muskeln erklärt ungezwungen die oculopupillären Symptome.

Ich nehme hier die Gelegenheit wahr, eine in jüngster Zeit zuerst von Landouzy und später von anderen Autoren erwähnte partielle Oculomotoriuslähmung und zwar des levator palpebrae kurz zu tangiren. Bei Convexläsionen ist nämlich in einigen wenigen Fällen isolirte Lähmung des levator palpebrae—ptosis angegeben. Landouzy behauptet, auf Grund seiner Beobachtung, dass das Centrum für den Levatorast im Parietallappen gelegen sei. Ich möchte nun, gestützt auf unsere Beobachtungen, die Frage aufwerfen, ob es sich nicht auch in diesen Fällen um eine Lähmung des glatten Lidmuskels und des orbitalis gehandelt habe. Mich wenigstens will es dünken, soweit ich mir überhaupt aus der Lectüre der diessbezüglichen Casuistik ein Urtheil bilden kann, dass es sich gar nicht um isolirte Oculomotoriuslähmung, sondern um Lähmung der oculären Fasern des Sympathicus gehandelt habe. Lässt sich dies nachträglich auch nicht mehr völlig entscheiden, so möchte ich hiermit doch für die Zukunft auf diese Möglichkeit hingewiesen haben.

Der diagnostische Werth der Hemiepilepsie ist ein enorm grosser. Sie deutet in ihrer

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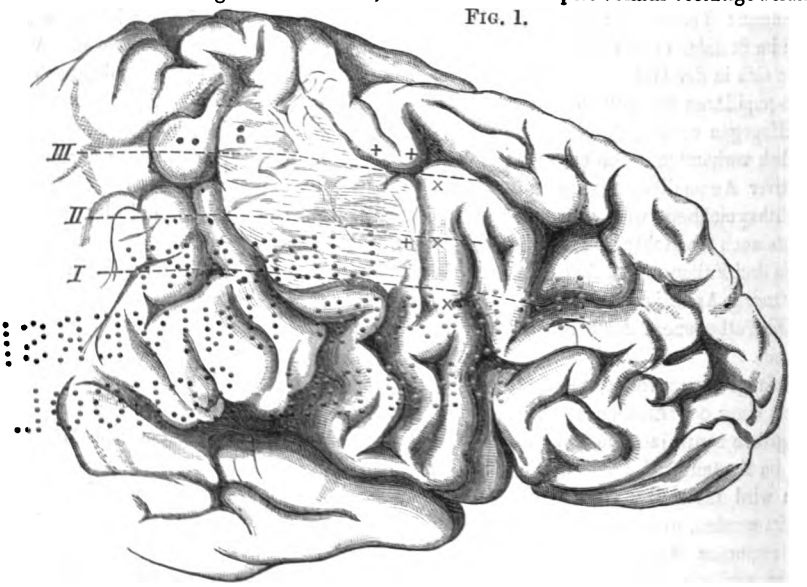
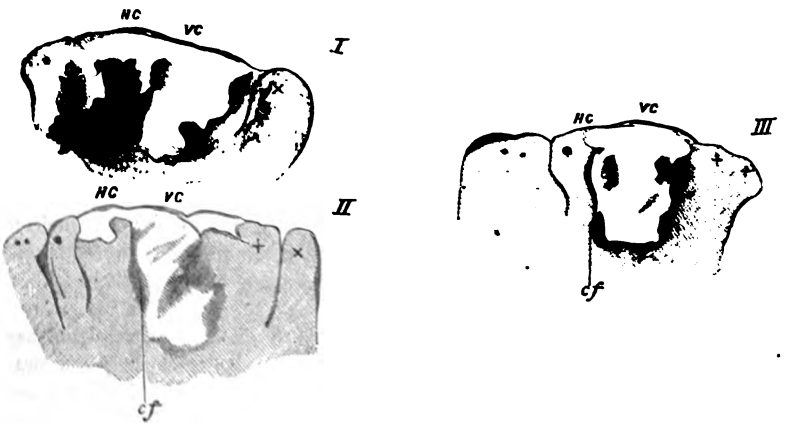
klassischen Form mit konsecutiver Lähmung mit voller Sicherheit auf eine Rindenläsion, u. zwar gewöhnlich auf Tumorenbildung, und gestattet unter Umständen ausserordentlich scharfe Localisationsdiagnosen zu stellen, die durch die Autopsie vollauf bestätigt werden.

Fig. 1.





Rechte Grosshirnhemisphäre mit der Oberflächenansicht des Tumors. Eingezeichnet mit punktirten Strichen die Richtungen der drei Vertikalschnitte, deren Ansicht in den Fig. I., II., und III. gegeben ist. Mit Punkten (·) und Kreuzen (+) die an diesen Durchschnitten in gleicher Weise bezeichneten angrenzen den Windungen.

cf Centralfurche (S. Rolandi).
VC vordere Centralwindung.
HC hintere Centralwindung.

Dunkel gehalten in I., der kleine Blutherd in der H. Centralwindung; in II. und III. die hyperæmische Zone der angrenzenden Hirnsubstanz.

Zur Illustrirung theile ich Ihnen einen einschlägigen Fall aus meiner Beobachtung mit. Bei einem 61jährigen Manne, der bis October, 1879, vollständig gesund war, traten von da ab hie und da eigenthümliche Paralgien in der linken Hand auf, als: Brennen,

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Rieseln und Schmerz, bis endlich im April, 1880, ein starker Krampfanfall ausbrach, der den linken Arm hin und her, theils gegen den Körper, theils gegen die Wand, mit aller Kraft und Macht schleuderte. Nach einigen Minuten kam es auch in den Gesichts- und Augenmuskeln, und endlich auch in der linken unteren Extremität zu klonischen Krämpfen. Nach einer halbstündigen Dauer war der Anfall vorbei, doch der linke Arm bedeutend geschwächt. Diese Anfälle wiederholten sich in der Folgezeit in ganz stereotyper Weise. Im November vorigen Jahres war der letzte Anfall aufgetreten. Bald nachher, December, war aber auch die linke obere Extremität vollständig gelähmt. Ursprünglich bestand nur Lähmung der kleinen Handmuskeln. Mundfacialis, Hypoglossus, die unteren Extremitäten, sowie die Sensibilität, bleiben vollständig intact. Am linken Auge bestanden jedoch Enge der Lidspalte, Myosis und Retrusio bulbi. Zu Ostern (2. 4.) d. J. starb Patient plötzlich an Lungenoedem.

Die von mir gestellte Diagnose: Tumorbildung im mittleren Drittheile der vorderen und hinteren Centralwindung der rechten Hemisphäre wurde durch die Section, vorgenommen von Dr. Paltauf, vollständig bestätigt. Der compacte Tumor—ein Gliom—zeigt im Niveau der erwähnten Corticalisregion seine grösste Flächenausdehnung, durchsetzt die Corticalis und reicht nur an einer Stelle mit seinem konisch zulaufenden Theile bis an die Basis der vorderen Central-Windung. Die übrigen Theile des Gehirns zeigen keine Veränderung auch keine Spuren von absteigender Degeneration im 2. Pedunculus cerebri. Zur besseren Orientirung zeige ich Ihnen hier eine Zeichnung, die Herr Prof. Kundrat von dem gegenwärtig in der pathologischen Sammlung in Graz befindlichen Präparate entwarf.

Ich ziehe aus meiner Mittheilung folgende, allgemeine Schlüsse:

Erstens. Jackson's Epilepsie unterscheidet sich wesentlich von der genuinen. Der Anfall beginnt langsam anschwellend, breitet sich allmälig aus, bleibt dabei aber meistens unilateral, und wird ausschliesslich aus klonischen Zuckungen zusammengesetzt. Das Bewusstsein bleibt während des ganzen oder doch grössten Theiles des Anfalles erhalten.

Zweitens. Sie deutet in ihrer ausgesprochenen Form mit Sicherheit auf eine Convexläsion, und lässt mit Zuhilfenahme der entweder gleichzeitig vorhandenen, oder ihr erst folgenden Lähmungserscheinungen (namentlich der verschiedenen Monoplegien) nicht bloss die Localisationsdiagnose, sondern auch die der Natur der Läsion stellen.

Drittens. Oculopupilläre und zwar vorzüglich Lähmungssymptome, als: Enge der Lidspalte (Ptosis), Verengung der Pupille (Myosis paralytica), wobei keine reflectorische Pupillenstarre besteht, Retrusio bulbi scheinen ein ausserordentlich häufiger und auffälliger Zug im klinischen Bilde derselben zu sein.

Viertens. Das Armcentrum liegt im mittleren Drittheile der vorderen und hinteren Centralwindung.

Fünftens. Reizung dieser Region, resp. allmälig völlige Zerstörung durch Tumorenbildung erzeugt anfänglich Jackson'sche Epilepsie, die stets im Arme beginnt, und schliesslich complete Paralyse der ganzen oberen Extremität.

DISCUSSION.

Dr. Hermann Weber, London: Referring to the President's remark whether aphasia had been observed following left-sided epileptiform convulsions, Dr. Hermann Weber remarked that he had seen this twice in the case of a man, æt. 24, affected with mitral disease. The attacks commenced with convulsions and rigidity in the left arm and leg, followed by momentary unconsciousness and amnesia of what occurred during about two minutes; they were followed by weakness, almost paralysis of the left side, gradually disappearing within two to five days, and aphasia, well marked at first, gradually disappearing in the course of a fortnight. The patient in question had almost equal facility

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in using both hands; he was, however, not what could be called left-handed; he wrote with the right hand. He had had epileptic convulsions in childhood up to the age of four, but had been free from them from that time, until 22, when he was first attacked with the fits described, after having been subjected to great mental excitement and bodily over-exertion and exhaustion. Embolism was thought of as the cause of the first attack.

Dr. Allen Sturge, Nice: I would ask Dr. Jackson if he considers epileptic aphasia to be always due to exhaustion of the speech centres. In my experience aphasia may be one of the early phenomena of an epileptiform attack. In support of this assertion I may refer to three cases that have come under my notice. Two of the cases were women whose fits began with contraction on the right side of the face. In one case the convulsion passed to the right arm. Consciousness was not lost. In the other case it passed to the right arm and right leg, and consciousness was lost. In both cases the patient lost the power of speech simultaneously with the onset of the spasm of the face. When asked, after the fit was over, why they could not speak, both women said that they "did not know, but they could not get their words." The third case was a man whose fits began in the right leg, passed to the right arm, and thence to the right side of the face. There was sometimes loss of consciousness and sometimes not. He said that when the convulsion reached his face he lost his speech. I think that in these cases the aphasia is due to "convulsion," involving the so-called "speech-centres," and was a positive and not a mere negative symptom. There would thus appear to be a positive aphasia due to "convulsion" of the speech centres, and a negative aphasia due to "exhaustion" of them.

Dr. Brown-Séquard, Paris, thanked Dr. Jackson for his very important paper. He had seen some cases in which there was aphasia after unilateral convulsions, localized either in the face or the arm on the right side, or in the same parts on the left side, although less often on this last side than on the right. But he had seen also cases of attacks of aphasia without epilepsy, and he, as well as physicians who have specially studied epilepsy, had ascertained that it is a rule, with only a few exceptions, that some degree of aphasia is observed after attacks of common idiopathic epilepsy. He mentioned a singular case of a gentleman, who for twenty-five years, had had frequent attacks of aphasia, without any other nervous symptom.

Professor Bäumler, Freiburg, thought he could support the proposition of Dr. Sturge that aphasia might be a positive as well as a negative symptom, only he would rather use the term *dysphasia* instead of aphasia, having seen cases in which the derangement of speech consisted in a jumbling together of syllables belonging to various words, as if the cerebral speech centre were in a state of convulsive action. Professor Bäumler further wished that rather more importance among the symptoms of epilepsy should be given to the symptom of amnesia after the attack than to the unconsciousnes.

Dr. Marcovici, Bucharest: May I be permitted to add to the interesting communication by Dr. Jackson, and the no less interesting and confirmatory observations of Dr. Müller, of Graz, the particulars of a case of syphilitic Jacksonian epilepsy which presents many points of interest? It occurred in a young man twenty-six years of age, of good constitution and habitual good health, whom I had seen on account of a hard chancre, and who, in spite of my advice, much neglected himself, and did not follow up my treatment. He met me by chance, and informed me of symptoms which alarmed me, a severe nocturnal headache in the region of the right temple, and of deafness, and sometimes even of pain in the right arm, and especially in the hand. Despite my severe remonstrances, he continued to do nothing, and suddenly, about two o'clock in the morning, he was

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seized with violent Jacksonian convulsions (without tonic contractions, and without loss of consciousness) which lasted nearly two hours, after which he was completely hemiplegic on the left side. This case is very interesting, because it shows first that Jacksonian convulsions may be in certain cases preceded by prodromata of long duration; secondly, that although not lasting more than some minutes, they may repeat themselves again and again in such a fashion as to constitute a severe attack of one or more hours' duration; thirdly, that paralysis may persist indefinitely. Gentlemen, we have known for a long time that severe lesions, and especially compression by tumours of all that part of the encephalon which is considered as forming part of the physiological spinal marrow, may determine epileptic or epileptiform convulsions (Jacksonian), but we did not know the motor centre of the brain, and we did not know that certain parts of the hemispheres could equally determine convulsions. Dr. Jackson in his remarks has given, from a pathological standpoint, full confirmation to the teachings both of anatomy and of physiology, and herein lies their great merit.

Dr. Hughlings Jackson, London: I would first express my very great regret that, from ignorance of foreign languages, I am unable to remark on the observations of Dr. Müller and Dr. Marcovici. With regard to Sir William Gull's comments, the expressions, "going up" and the like, used in speaking of spreading of spasm, are intended simply to imply increasing spasm from increasing development of the discharge in the centres engaged during the seizures. I do not remember a case of temporary aphasia after left-sided spasm. However, having regard to the fact that permanent aphasia sometimes occurs with permanent left hemiplegia (mostly in left-handed people) in common destructive cerebral lesions, such as clot and softening, and having special regard to the interesting remarks of Dr. Hermann Weber, I do not doubt in the least that left-sided seizures are sometimes followed by temporary aphasia. In reference to Dr. Brown-Séquard's important statements, I admit that some aphasia is found after some seizures of epilepsy proper; this paper, however, is limited to epileptiform seizures. Further, I admit that there do occur cases of temporary loss, or of great defect of speech without any spasm; these cases also I have ignored because they do not come within the scope of the paper. I may further remark on Dr. Brown-Séquard's observations, that I have never acceded to the doctrine that the third left frontal convolution (or any convolution or limited region) is "the centre for speech;" my belief is that Broca's convolution is chiefly concerned during speech. I have never believed in abrupt localization of any kind. Referring to Dr. Sturge's most interesting remarks, I do not in the least doubt that the "speech centre" is useless for speech when it is the subject of an excessive discharge any more than I doubt that the arm is of no use for any of its normal operations, when its muscles are engaged in spasm. Of course, exhaustion of nerve elements after a fit is not the cause of a patient saying "very well," &c., but the cause of the diametrically opposite thing, the cause of his inability to say anything else. Indeed, I may quote from the printed abstract of my paper, the expression that exhaustion of nerve elements explains "the negative element only, of course." I believe that all nervous symptomatic conditions are duplex—that in each there is a negative and a positive element. I should suppose that in the important cases Dr. Bäumler mentioned, there was some degree of negative affection of consciousness, since, although the patients spoke, they remembered nothing of what they said on full recovery. I consider that in, and often after, epileptic seizures of different degrees of severity, there are innumerable degrees of negative affection of consciousness from slight mental confusion down to deep coma. I believe that in cases of post-epileptic actions (from simple vagaries to mania) there is negatively the physical condition, exhaustion of some nervous arrange-

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ments of the highest centres, answering to what is, psychically, defect or loss of consciousness. Manifestly the negative condition of exhaustion of nervous elements cannot cause the positive things, the actions. I consider these actions to be the outcome of the diametrically opposite (the positive) condition of over-activity of other nervous arrangements—over-action from "loss of control" or "loss of inhibition." The simplest illustration of this principle is given by cases of drunkenness. In reply to Sir William Gull's further question, I should look on the whole nervous system as a mere sensori-motor mechanism, and my invariable plan is to study all diseases of it (including so-called mental disease) in a "brutally materialistic" manner. But I regard consciousness (only a general name for the artificially separated states of mind called Will, Memory, &c.), not as identical with, but simply as concomitant with, states of this sensori-motor mechanism. As regards the nervous system, no one can be more materialistic than I am: both before and since Hitzig and Ferrier's experiments, I have held that the whole of the brain is like the rest of the nervous system—sensori-motor. My object in speaking of mental states, as I often do, in considering cases of disease of the nervous system, might best be described as an endeavour to put mental states "on one side" in order to study the nervous system in (repeating a former expression) a "a brutally materialistic" manner, and thus without psychological implication. For an example, I never use such expressions as that an aphasic does not speak "because he has lost the memory of words," believing that to be only a verbal explanation. I use the simpler expression, "makes mistakes in words." Further, what I have said does not imply any hypothesis of my own on mind and nervous system; I simply accept the almost universally accepted hypothesis. So, instead of my being bound to give evidence of the validity of this hypothesis ("the hypothesis in possession"), it is rather for those who hold some hypothesis at variance with the accepted hypothesis to bring forward evidence in favour of their novel doctrine. I am not in the least bound to show the nature of the relation betwixt the two different and yet concomitant things—consciousness and sensori-motor activity of the mere mechanism. It is right to add that the view I accept—that psychical states are different from, and yet concomitant with, states of the nervous system—is that taken by Sir William Hamilton, Huxley, Herbert Spencer, Tyndall, Du Bois-Reymond, Laycock, Fiske, J. Stuart Mill, Clifford, Max Müller, Bain, Ferrier, Hermane, and, so far as I know, with one important exception, by all eminent men who have written on the matter. G. H. Lewes believed a psychical state and a nervous state to be one and the same process viewed under different aspects.

On Certain Little-recognized Symptoms of Tabes Dorsalis.

Dr. Thomas Buzzard, London.

In his description of the symptoms of tabes dorsalis, Romberg gives the greatest prominence to the insecurity of gait. "The patient," he writes, "attempts to improve it by making a greater effort of the will; as he does not feel the tread to be firm, he puts down his heels with greater force. From the commencement of the disease the individual keeps his eyes on his feet to prevent his movements from becoming still more unsteady. If he is ordered to close his eyes, while in the erect position, he at once commences to totter and swing from side to side; the insecurity of his gait also exhibits itself more in the dark. . . . . In no case have I found it (this symptom) wanting."*

* Romberg's "Diseases of the Nervous System." Sydenham Society; translation by Dr. Sieveking, 1853, vol. ii. p. 396.

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So also Duchenne (de Boulogne), in the classical description which he gave of the same disease, under the name of Progressive Locomotor Ataxia, allots the foremost place to this motor trouble. "In the first rank," he writes, "I place the troubles of co-ordination of movement, contrasting with the, so to speak, latent integrity of the muscular force, because they constitute the fundamental character of progressive locomotor ataxia."*

The picture which these observers, especially Duchenne, have given of a disease in which this symptom holds a prominent place, is universally known, and I have no intention of reproducing it. I merely wish to point out *in limine* that the ataxia of gait has been so strongly insisted upon by these and other writers, that the symptom has of necessity been hitherto regarded as the dominating one of the disease. There can be little question that the idea never enters into the mind of many, that a patient may be the subject of this disease, unless the inco-ordination of movement be tolerably well marked.

The object of the present communication is to suggest that, in the present condition of the question of tabes dorsalis, we are not quite warranted in saying of a case which not show the symptoms of ataxia that it has not yet arrived at a certain "stage." Our grounds for dividing the course of this disease into different stages, appear to become less and less secure as we observe that various parts of the sensory nervous system may be involved in degrees of intensity which bear no fixed relation to any chronological order. Although the point can only be determined after a considerable lapse of time, it seems most probable that ataxia of gait is no more a necessary symptom than optic atrophy, although it is a much more frequent one. Whilst allowing, then, this highly characteristic symptom when present to retain all its former value for diagnostic purposes, it is well to recognize the fact that not only may the inco-ordination of movement be entirely wanting, but that some other symptom which may not ordinarily be at all a striking one, may come to occupy the most prominent place. In these circumstances it is apt to absorb the attention of the observer to such a degree that, without some considerable care, the general disorder, of which it is only a symptom, is masked, and the symptom itself is liable to be referred to some entirely different pathological condition.

The necessity for extreme brevity obliges me to indulge in a certain dogmatism which I would rather avoid, but which, under the circumstances I trust will be pardoned. Let me say, then, at once, that in my judgment we may take it that Westphal's test, the absence of the knee phenomenon, provided that this is associated with good voluntary power, and ideo-muscular contraction of the vastus internus muscle, a provision which Dr. Erb has most strongly and justly insisted upon, is a nearly certain sign of tabes dorsalis. There is one pathological condition, however, which may possibly give rise to some doubt—indeed, I have met with an instance in point. In diphtheritic paralysis there is often a tottering gait, accompanied by numbness in the extremities. If there should be, as sometimes happens, no difficulty in deglutition, and no failure of the power of accommodating the eyes, a strong superficial resemblance to a case of tabes dorsalis is presented. In such a case the patellar tendon reflex will, in all probability, be absent, and the vastus internus muscle may contract as freely as in health, to direct percussion. The acuteness of the symptoms, however, the absence of characteristic lightning-pains, and the history of preceding sore throat, should be sufficient to distinguish this condition from tabes.

When characteristic lightning-pains are present, Westphal's symptom (with the provisions mentioned) gives, I think, evidence which is, as nearly as possible, complete,

* "De l'Electrisation localisée," Paris : 3me édition, 1872, p. 623.

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of the presence of tabes. I have not the time now to dwell upon the other side of the question—the cases of tabes dorsalis with well-preserved, or even exaggerated, knee phenomenon, of which we occasionally witness examples. Suffice it to say, that the absence of the knee phenomenon in the circumstances described must either be attributable to the presence of tabes, or it must be owing to a natural peculiarity, and examples of this are so exceedingly rare that they may be practically disregarded. I am prepared, indeed, from something which I lately saw, to meet with the absence of knee-phenomenon as possibly representing the only symptom of tabes in a certain case. This test of Westphal's is so delicate, that the accompaniment of any one of the ordinary symptoms of tabes along with it, ought, in my opinion, to enable us, with very little hesitation, to relegate the case in which this association is exhibited to the class of tabes dorsalis.

Let me now refer to some examples in illustration of the statement that a comparatively rare symptom of tabes may assume such proportions as to stand prominently forward, and easily give rise to errors of diagnosis. The first to be mentioned are cases in which the *crises gastriques* dominated over other symptoms.

A gentleman, aged fifty-two, was brought to me by his medical attendant on June 28, 1880, suffering from emaciation and neuralgia in the head. It appeared that for fourteen years he had been subject to so-called rheumatic pains of a flying character, and besides had had what was called sciatica five years before, and sudden shooting pains between the ribs the previous year. For about fifteen years he had been liable to occasional attacks of violent vomiting, accompanied by pain in the stomach, lasting from one to four days, and recurring, perhaps, once in six weeks or two months. I examined carefully for any epigastric tumour, but could find none. The urine contained no albumen, his breathing was laboured, and there were some not very positively marked signs of a cavity in his right lung. His pupils were contracted to less than the size of a pin's head. They were insensitive to light, but contracted during accommodation. The patellar tendon reflex was quite absent in both legs. The vastus internus muscle showed excess of irritability to percussion. He had no ataxia of gait, but, in reply to my inquiry, informed me that he could not get on well in the dark.

I lately learned that this patient died two months after I had seen him, and that his case was diagnosed by a very accomplished physician, who examined him subsequently to me, as one of malignant disease of the stomach. No autopsy had been made.

This man was certainly affected with tabes dorsalis, as evidenced by the absence of knee phenomenon, along with well-preserved, and indeed abnormal, contractility of the muscular fibres of the quadriceps extensor, the presence of the Argyll-Robertson pupil, the history of shooting pains, and of some slight difficulty in the dark. The attacks of vomiting as described were precisely similar to the *crises gastriques* of Charcot, and they had been occurring at intervals of a few weeks for fifteen years. Even without the evidence on the negative side afforded by the fact of my having failed to find any tumour (and, thanks to his emaciation, examination was easy) this history of such long-continued and periodical vomiting is not consistent with malignant disease, but, on the other hand, is precisely what is to be expected in the gastric crises of tabes dorsalis. I have no doubt whatever that this was the explanation of his illness.

I brought before the Pathological Section, at Cambridge, last year, amongst other examples, the case of a man, Henry H., now aged sixty-two, who has suffered from the joint affection described by Charcot, the head of his left humerus having quite disappeared. The loss of the use of this arm had brought the man to the workhouse; but it is an interesting fact that he was there employed as a messenger, and that he could walk as much as ten miles a day. He had no ataxia whatever, but he had lightning-pains,

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Argyll-Robertson pupil, absent patellar tendon reflex, and a history of gastric crises so severe that they are well worth relating in some detail. It seems that he usually had good health till he was about fifty years of age, and then, one day he was seized with a dreadful pain in the epigastrium, and a few hours later retching began and continued. He drank cold water, and when that was vomited the pain was eased for a time, but then returned. For week after week he went on, with almost constant suffering. Perhaps he would doze off to sleep, but in an hour would be woke up by the pain. There was great aching over the chest generally, but the focus of pain was below the ensiform cartilage.

For nine months, he says, he was never a day without pain and retching; not always equally severe, but so bad that he never left his room, and kept his bed for the greater part of the time. He would roll about with the pain, trying all sorts of different positions to get relief. The pain made him sick. It was not due to what he swallowed, as the retching would occur when he had taken nothing. Sometimes some mucus would come up, but it was chiefly empty retching. The only thing that relieved him was drinking large quantities of water; when he had brought that up he would be easy for a time. Sometimes he would contrive to keep down a few spoonfuls of brandy, milk and egg. He tells me that once he was for eight days without taking any nourishment. He became so thin and reduced that he was nearly dying of exhaustion. The bowels were obstinately costive. His case, it would seem, excited curiosity, for his medical attendant brought several doctors to see him at different times, and various remedies were tried to stop the vomiting. Amongst other things, he was cupped, blistered, plastered, and leeched.

After about nine months the pain and retching gradually ceased, not, it would seem, as the result of any special treatment. He then, after a time, returned to work, and remained at his duties, though only in tolerable health, for a year. He was still occasionally liable to relaxation of the bowls, slight pain in the stomach, and retching. At the end of a year he was again laid up for three months, with symptoms like those of the first attack, but on this occasion not quite so severe. Altogether, he has had some half-dozen attacks, all of the same character as the first, but none quite so bad. They would last some six or eight weeks, and during this time he was quite incapacitated, and had to take to his bed.

In this case we have, there can be no doubt, an example of the *crises gastriques* of more than ordinary severity. For the time being, the gastric symptoms dominated everything, and no suspicion of the general disorder with which this man is affected had ever occurred, until an examination, which I made on account of the joint affection, conclusively showed the nature of his case.

In another joint case the patient, a female, had been admitted into a parish infirmary entirely on account of long continued "indigestion." She would have attacks of gastric pain and vomiting, lasting ordinarily two or three weeks at a time, and recurring at intervals, which varied a good deal in length. She has had as many as four attacks in four months, and the longest period of exemption has been eight months.

In the attacks, she says, there is a "bad sensation" in the stomach, and she can keep nothing down. There is great pain in the chest and stomach, which feel as if they were raw, accompanied by much flatulence and a great deal of retching. She will sometimes bring up greenish stuff and food, but she is sick even if no food has been taken. She has never suffered from diarrhœa, but at times there has been a tendency to tenesmus. During the retching there is a feeling of tightness across the chest, and afterwards a soreness.

In her case it was not till six or seven years after the beginning of these gastric attacks that she began to suffer from inco-ordination of movement. At the time when I

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saw her she had Westphal's symptom, Argyll-Robertson pupil, lightning pains, and other symptoms, in addition to her osseous lesions.*

Here is the description of gastric crises in a woman whom I showed to the Pathological Society, along with the last patient:—

She has a pain at the pit of the stomach, going through to the back. There is dreadful griping and belching of wind. For three or four weeks past she has had this every day, but now she is getting better again. The attack usually begins about 4 or 5 A.M. It is not accompanied by diarrhœa, but she gets swollen up like a woman near her confinement. After a bout of pains of this description she may go one, two, or three months without any, and she feels as though she would never have them again. Then she will be suddenly seized with them. The stomach pains began more than ten years ago. She has been married ten years, and previous to that had suffered from the stomach pains, though not so severely as she has done since. At that time, too, she used to have stabbing pains in her knees and in one of her finger-joints, which she thought was rheumatism. Formerly, when the stomach pains attacked her "she would vomit all day and all night" for two or three days together. She threw up "enormous quantities." After a few days this gradually ceased. Between the intervals of the pains she feels quite well. Her appetite is good, and she has nothing (except the crippled state of her legs) to complain of. The plunging pains described attack her in the joints and limbs, but never in the trunk, except in the stomach. She may have gone three months, but never six months, without the stomach pain.

Another woman, for twelve years before she shewed any ataxia of gait, had suffered from attacks of vomiting, which at first would occur about once in a year, and last from ten to twelve days. For the last two years these attacks have been much more frequent, and she will have one as often as every three weeks or so, which lasts a week, or more. At first there is nausea, then vomiting. There is then epigastric pain, which does not go through to the back. It is like cramp, and it makes her twist about. She brings up much wind, and then there is a great deal of empty retching. Neither the presence nor absence of food appears to have any influence upon the retching. This patient also is the subject of the joint affection of Charcot, and presents every symptom of tabes.

Through the kindness of Mr. Herbert Page, I had, quite recently, the opportunity of examining a man aged thirty, who is suffering from joint affection in both feet, which had been diagnosed by Mr. Page as due to tabes. In this young man's case, a gastric attack was part of the history. The case in this respect was especially interesting to me, as I have drawn attention to the remarkable frequency of association of gastric crises with joint affection in tabes.† Out of eight cases of tabetic arthropathy which I have brought before the profession, gastric crises occurred in no less than five. Mr. Page's case adds another to the list.‡ It seemed, on inquiry, that two years ago this young man had an attack of vomiting, which lasted on and off for three months. In the morning, when he got downstairs, he would begin vomiting before he had taken food. Then he would drink a cup of tea, and eat an egg and a slice of bread and butter. He says that he brought up the tea, but not the solid food. The vomiting would continue

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* Cases of Osseous and Articular Lesions in Tabes Dorsalis, by T. Buzzard, M.D.: *Transactions of the Pathological Society of London*, 1880.

† On the Affection of Bones and Joints in Locomotor Ataxy, and its Association with Gastric Crises," by Thomas Buzzard, M.D.: *British Medical Journal*, March 5, 1881.

‡ It is right to say that, since this paper was written, I have seen two cases of tabetic arthropathy in the practice of my friend, Mr. Macnamara, in neither of whom had there been any gastric attack.

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till the middle of the day. He would often vomit as much as a dozen times, and did not pass a day without sickness, for three months at least. There was pain in the stomach and between the shoulders. Much wind was expelled. He became so weak and bad that he could do no work for six months. The vomiting left off suddenly after remedies had proved unsuccessful. He has never drank spirits. In the beginning of his gastric attack he would become dreadfully hungry, and would eat much solid meat towards night. But after three or four weeks he lost his appetite, and became very thin. The gentleman who attended him during this illness tells me that he had no suspicion of the cause of the vomiting. This man has had shooting pains in his legs, the knee phenomenon is absent, his pupils react in accommodation, but not to light. He has never had any ataxia of gait.

These cases are sufficient to show that gastric attacks, often of an extremely severe and obstinate character, may occur in persons who, although they show no sign of inco-ordination of movement, are most certainly examples of tabes dorsalis. There is reason to believe that in none of these cases the nature of the general disorder underlying the gastric attacks had been suspected.

I would suggest that many cases of so-called "gout in the stomach" would be found, if examined by the light of our present knowledge, to be examples of tabes dorsalis with gastric crises. The lightning-pains, which would probably be associated with the sickness and epigastric pain, would be likely in the minds of many to make the diagnosis of "gout in the stomach" complete.

Looking back into the past, I can call to mind cases in which the diagnosis I had made of malignant disease of the stomach was not confirmed by the sequel—long intervals of exemption from the symptoms taking place—which I now think were probably examples of tabes with the gastric crises predominant.

These symptoms may easily give rise also to suspicion of intestinal obstruction, and may possibly also be mistaken for the attacks of vomiting which are apt to occur in uræmia, from Bright's disease.*

This predominance of one symptom, whereby the whole attention tends to become absorbed by it, and it alone, may, there can be no doubt, distinguish other symptoms of tabes. And here it seems to me well to remember the important observation of Pierret, that tabes essentially attacks the sensory side of the nervous system. It is the posterior roots and columns which are changed primarily, not the anterior cornua; though these are sometimes invaded in the course of the malady. Pains (anæsthesia, hyperæsthesia, analgesia) are the most frequent symptom, whilst the absence of the patellar tendon reflex—if this phenomenon be really of a reflex character—can only be explained by a "fault" or break in the sensory or afferent portion of certain spinal nerves. Of

* In a communication brought before the Pathological Society of London, in February, 1880 (*Transactions of the Pathological Society*, 1880), I made the suggestion that the gastric crises depended upon irritation of the nuclei of the vagus by sclerosis. At that time I had no anatomical evidence to offer in support of the hypothesis, which was based upon the paroxysmal character of the attacks, so completely in accord with that characterizing the attacks of lightning pains. It appeared evident that if sclerosis, which, when it attacked nerves of common sensation, produced pain, came to invade the nucleus of the vagus, it might be expected to give rise to symptoms like those of the gastric crises. During the meeting of the International Medical Congress in London, Professor Pierret, of Lyons, has shown me sections of the medulla oblongata which he had made from a case of tabes with gastric crises. I was greatly interested in his demonstration that the funiculus gracilis in immediate relation with the nuclei of the vagus, exhibited distinct sclerosis.

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the cranial nerves, it is those which are sensory in function that are especially liable to be invaded by sclerosis. Just as the lesion in the posterior columns affecting the sensory roots of spinal nerves causes pains as well as anæsthesia, so if it be the nucleus of the trigeminus which is attacked, we get sometimes pains about the head, and sometimes anæsthesia in the district of the nerve. When the palate and pharynx are thus affected the act of deglutition is materially embarrassed. I remember the case of a man who applied principally on account of difficulty in swallowing. He complained that fluids regurgitated through his nostrils. As at the same time he said that he was weak on his legs, a strong *primâ facie* resemblance to diphtheritic paralysis was presented. Subsequent examination showed that, although the difficulty in his palate was so strongly marked as to be the dominant symptom, the case was really one of tabes dorsalis. Again, I have recorded instances in which violent pains in the head accompanied by amaurosis from atrophy of the disc simulated tumours of the brain. The pains were due to sclerosis affecting the roots of the trigeminus, and the patients were examples of tabes with cephalic symptoms dominant.

The association of progressive atrophy of the optic nerve with tabes has long ago been insisted upon, especially by Hughlings Jackson and Charcot. The absence of the knee phenomenon now affords us a most valuable means of confirming this association. In one example of this kind which I have recorded, the patient, besides a progressive atrophy of both discs, presented no other symptom whatever, except that the patellar tendon reflex was absent from the right, and only slightly marked in the left, leg. In the course of a few weeks it had also disappeared entirely from the left leg.

I have seen quite lately a similar case with Mr. Bowman, to whom the patient had applied on account of increasing difficulty of sight.

Duchenne, Topinard, and others have recorded examples of tabes in which deafness occurred, but it is to Pierret that we owe the observation that the auditory nerve is really very often involved. Since his paper appeared ("Sur les Symptômes Céphaliques du Tabes Dorsalis," Paris, 1876) I have noted the occurrence of deafness in a considerable number of cases. My colleague, Dr. Ormerod, has given me notes of thirteen cases of tabes observed by him, in five of which deafness was more or less strongly marked.

I would suggest that in cases of "nervous deafness," as in those of optic atrophy, the examination of the state of the patellar tendon reflex should not be omitted. It is quite probable that we may discover in certain instances absence of the knee phenomenon, and thus be led to place the case in the same category with those of amaurosis from sclerosal atrophy.

It has occurred to me to see one patient in whom the presence of a stone in the bladder led me to examine the state of the patellar tendon reflex. I found that it was absent, and inquiry elicited the fact that the patient suffered from most characteristic lightning pains. It occurs to me to suggest that tabes may be an occasional source of origin of a calculus, of the kind which Sir Henry Thompson calls "local," in contradistinction to the calculus (as, *e.g.*, uric acid) which is of "constitutional" origin. In many cases of tabes, at an advanced stage, there is actual paralysis of the bladder. But a certain amount of difficulty in expelling urine may be a very early symptom, dependent probably upon vesical anæsthesia. In such a case there is always the possibility of cystitis occurring from urine getting retained; and in the mucus of the bladder, in these circumstances, a phosphatic calculus may easily be formed. It is worth while remembering that a bladder trouble of this kind may be as prominent and absorbing a symptom as either the gastric crises or the optic atrophy; and if ataxy of gait be absent, the real nature of the trouble may easily be overlooked. Hemiplegia may mask the symptoms of tabes.

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The following patient (notes of whose case were taken by Dr. Beevor) is now under my care in the hospital:—

Henry H., aged forty-eight, single, a gilder of picture frames; has drunk a good deal, and had a chancre at twenty-two years old, not followed by rash or sore throat. He had felt some numbness around his waist and in the legs for about a fortnight, when, on April 8, 1881, whilst out walking, after his work was over, he was seized with a sudden weakness and numbness in the left leg. Then a pain between the thumb and forefinger, and a little numbness and pins and needles in the left hand, with some weakness in the left arm. He continued much the same till the middle of the next day, when his leg got much worse, and in the evening the arm gradually became powerless. By April 10 he had completely lost power in the left arm and leg. The consciousness had never been impaired. On April 12, the left side of his face began to feel stiff, and he could not move it so well as the right. The sensibility of the skin was never lost. On admission the left side of the mouth was observed to be moved less than the right, and the left eye could not be closed so tightly as the right. The tongue was protruded slightly to the left. He was entirely unable to move the left arm, and could not shrug the left shoulder. There was some stiffness in the joints of the left arm, but no pain was caused by moving them, except a very little in the shoulders, when the arm was abducted from the body and the hand placed on the top of the head. Both legs could be moved freely about, but the right much more strongly than the left, in which there was no power to invert or evert the ankle joint. There was no stiffness in the left leg.

At present the time—June—the patient can just stand without help. With the eyes closed, he reels and threatens to fall. When he walks with help, it is noticed that, whilst the left leg is somewhat dragged, the right is inclined to be thrown about. There is no wasting in his limbs. The knee phenomenon is absent on both sides. There is no ankle clonus. The plantar reflex is obtained on either side, on the right rather better than on the left. The cremasteric reflex not obtained on either side. The abdominal and epigastric reflexes are obtained equally well on either side. No definite loss of sensibility can be made out, but there may be a slight deficiency in the left foot. There is no affection of sight nor of hearing. No diplopia, or hemiopia. The heart's apex cannot be felt; there is occasionally an intermission of a beat. There is no murmur, but the sounds are not clear. The pulse is rather feeble, the brachial not tortuous; urine contains no albumen.

Pupils are of moderate and equal size; they do not contract to light, but contract during accommodation. On inquiry, it seems that the patient has suffered for about ten or twelve years from "rheumatic pains, coming and going" in an instant, and gone before he could cry out. Five years ago he had diplopia, which lasted about a fortnight. He has had no difficulty in walking about a room if he knew the locality; but once or twice he has lost confidence in the dark when he did not know his way about. He never had numbness in the legs, nor loss of sensibility; he has never shown any peculiarity of gait which has been remarked, to his knowledge, by others.

There is a most interesting circumstance to be noted in this case; the man's perfectly helpless left arm shows some rigidity and contraction, as is so often seen as a result of secondary changes in hemiplegia. In such circumstances, as is well known, we confidently expect to find more or less marked exaggeration of the reflex, following blows upon the wrist, as well as upon the patellar tendon. In this case there is an entire absence of any reflex at the wrist, as well as at the knee. The condition peculiar to tabes apparently overpowers that which is apt to occur as as a result of hemiplegia.

In the history of several ataxies I have come across an account of an hemiplegic attack:—

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Charles B., aged forty-one, applied at the hospital in December, 1878, with marked ataxia of gait, inability to stand with eyes shut, lightning-pains, anæsthesia of legs and arms, diplopia, absence of knee phenomenon. Sixteen years previously he had suffered from right hemiplegia.

Matilda H., a typical case of locomotor ataxia, with arthropathy when forty years of age, and five years after she began to get lightning pains, suffered from a slight attack of left hemiplegia, from which she recovered in a week.

Some of these may possibly have been cases in which there was transitory facial paralysis, as in the following instances:—

Charles V., aged thirty-seven, applied at the hospital, in April, 1877, with paralysis of the right side of his face of six days' standing. The eye could not be completely closed, and the mouth was drawn over to the left; the faradaic excitability of the orbicularis palpebrarum, and of the muscles about the chin, was good, but that of the muscles raising the lip was defective. He was found to have all the symptoms of locomotor ataxia. It appeared that the facial paralysis had followed a succession of electric-shock-like pains in the right side of the head, by which he had been severely affected for three weeks. The facial paralysis subsided gradually in the course of a month.

Colonel S. had right hemiplegia four years before symptoms of ataxia were noted; although from the history, as I learned subsequently, the hemiplegia occurred whilst the patient was affected with symptoms of tabes.

Let me pass on now to another condition:—

A gentleman, aged fifty-one, whilst standing up in church, felt a sudden tremor in his legs, which gave way, and he dropped into his seat. This sudden giving way of the legs had been repeated on many occasions, and it was for this, and this alone, that I was consulted. There was nothing to be noticed about his gait. Examination showed that he had no patellar tendon reflex, and inquiry elicited a history of so-called "rheumatic" pains in one of his elbows. He also described a sensation in his legs as though cold water were being gently poured over them from a water-pot, and some numbness of the left hand.

The sudden giving way of the legs, so that in many cases the patient actually drops to the ground, is a symptom which has not, so far as I know, been previously described amongst those characterizing tabes. It is, however, one of very common occurrence. Most frequently it happens in connection with a particularly sharp sort of lightning pain in the lower extremity. But it will often take place without the accompaniment of pain. I do not refer to the insecurity and tendency to fall, which is very often seen in an ataxic who is struggling to preserve his balance, amid the wild throwing about of his limbs. The symptom of a perfectly sudden giving way of the legs often occurs in persons who have no ataxia of gait, and quite independent of a dart of pain.

A very intelligent patient, who is blind from sclerosal atrophy, and who has suffered besides, for twenty years, from typical bouts of lightning pains, but is completely free from ataxia, tells me that he often has this symptom of sudden giving way of the legs. In him it is always associated with pain. He is sure that it is not a spasmodic contraction, but a sudden and temporary failure of muscular power; in fact, a very transitory paralysis.

A letter-sorter, whom I saw in 1871, told me this:—"One day, whilst walking through London, he was suddenly seized with a kind of loss of power in the legs, and would have fallen but that he laid hold of a doorway. The right leg especially seemed to give way." Another letter-carrier, who came to me in 1868, described the first symptoms of his illness thus:—He went for a fortnight's holiday into the country, being then quite well. A week or two after his return, he found his knees giving way

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suddenly under him when running up and down the steps of houses. Soon he found himself tottering, although he could walk fast. He could not run. At that time he had no pains in his legs. He gradually got worse, and walked so much like a drunken person that he feared he should be taken for one. When I saw him he was a typical example of locomotor ataxia.

This sudden failure or giving way of the knees would appear to belong to the same category as the passing diplopia from failure of a muscle of the eye. Pierret* refers the transitory paralysis of muscles to irritation of the posterior roots. The suggestion appears to me to be a very valuable one. On the other hand, paraplegic symptoms may be so strongly marked as to render the diagnosis obscure. A striking example of this has recently occurred in my practice.

X., aged forty-four, tall, and not very muscular, had always been delicate, but fond of walking, and accustomed to walk a great deal. In April, 1881, he had a typical Hunterian chancre, which was open for a month, and was followed by a most profuse roseola, with which he was covered when I saw him on July 1. It seemed that, although there had been complaints of failing power in walking, sense of weariness, and pain in the sacrum and loins for several months, it was not until the attack of syphilis that marked loss of power occurred in the lower extremities. From that time, however, there was a rapidly increasing paresis of the lower extremities, and it was for this that I was consulted. I found him sitting up, able to move about on his legs with difficulty, aided by two sticks, or friendly arms, but not without. There was no throwing out of the feet, which were moved in a laboured fashion, suggestive of paraplegia. The power of lifting either knee against the pressure of the hand was small. There was no muscular atrophy nor loss of cutaneous sensibility. The action of the bladder was imperfect and delayed, and he did not feel any desire to pass urine. His feet were always cold. He had suffered pains in the lower extremities of a neuralgic or rheumatic character.

The superficial reflexes of the foot, sole, cremasteric, abdominal, and epigastric regions were lively on each side. His pupils contracted well to light, and the ophthalmoscope showed no change in the disc. The patellar tendon reflex was absent on each side. Contraction of the vastus internus to percussion and Faradaic currents perfect. A fortnight later there was definite diplopia, the false image being above and to the side of the other.

In this case a rapidly progressive paraplegia following syphilis gave a superficial resemblance to acute myelitis, which was, however, negatived by the state of the reflexes. The occurrence of diplopia along with the absent knee-phenomenon (the normal reaction of the vastus internus being remembered), leaves no doubt, in my judgment, that this case of apparent paraplegia is really one of tabes.

I saw, not long since, by the courtesy of Mr. De Watteville, a man, forty years of age, who had suffered for about twelve years from an occasional feeling as though he might fall down; as if, whilst talking, he needed a support. He had no objective giddiness in the ordinary sense. He had a headache, but he could not tell whether his want of power was in the brain or limbs. This feeling recurred many times a day and grew worse. He saw numerous physicians of eminence, by most of whom the condition was pronounced to be cerebral. It was not until Mr. De Watteville tested his patellar tendon and found the reflex wanting that the case stood out as one of tabes. Only after these sensations had existed twelve years did the patient begin to suffer from lightning pains, and it was still later that a slight ataxia of gait betrayed itself. I found that his vastus internus muscle was over-irritable to percussion, and that his pupils scarcely reacted to

* "Essai sur les Symptômes Céphaliques du Tabes Dorsalis." Paris, 1876.

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light, and reacted in accommodation. There was delay in the reception of painful impressions.

DISCUSSION.

Dr. Noel Gueneau de Mussy, Paris: In his very interesting communication, Dr. Buzzard pointed out some cases of tabes dorsalis in which the chief symptoms were disorders of the digestive organs. It is the more important to dwell on such cases that they are often mistaken. I beg leave to add a few remarks drawn from my own practice. I have observed some cases in which the pain in the stomach had a peculiar character; it was a compressive, belt-like pain. I do not assert that such is always the case, but I can certify that in some cases the pain presented such a character, so expressively connected with the congestive affection of the spinal cord. It is certainly possible that the gastric pains of the tabes dorsalis were sometimes mistaken for an *attaque de goutte dans l'estomac*, but I believe that such is not always the case. There is nothing contrary to analogy and to pathological laws in the supposition that the gouty processes which affects so many nerves can affect directly the nerves of the stomach in some instances. But I quite agree with Dr. Buzzard in the opinion that this very vague denomination of a *goutte dans l'estomac* includes several very different pathological conditions. Thus I have observed some patients, all of gouty family, affected with violent accesses of pain in the stomach, distressing to the utmost degree, sometimes followed by similar pains in the intestines, and lastly ending by the apparition of nettle-rash on the skin.

Ueber die ätiologische Bedeutung der Syphilis für die Tabes dorsalis.

Professor Erb, Leipzig.

Nach einigen einleitenden Bemerkungen, welche die Erörterung des Gegenstandes hier in dieser Versammlung hervorragender Beobachter aus allen civilisirten Ländern theils mit der grossen practischen und principiellen Wichtigkeit des Gegenstandes, theils mit den ausgesprochenen Gegenbehauptungen motiviren, bespricht Redner kurz die seitherige Entwicklung der Frage, nachdem von französischen Autoren (Fournier, Vulpian, Grasset), allerdings unter dem Widerspruch anderer französischer Autoren (Fulliara, Cornil, Lancereaux) zuerst in bestimmter Weise die grosse ätiologische Wichtigkeit der Syphilis für die Tabes ausgesprochen war, hat in England zuerst Gowers (1878–79) angegeben, dass in der Hälfte aller Tabesfälle früher Syphilis vorhanden gewesen sei; und ich selbst habe in Deutschland (1879) zuerst die Frage ernstlicher in Angriff genommen und bin durch eine sorgfältige Statistik zu einer geradezu erschreckenden Häufigkeitszahl der vorausgegangenen syphilitischen Infection bei Tabischen gekommen (1. Mittheilung: 54 per Cent; zweite Mittheilung: 88 per Cent!)

Diese Mittheilungen und die daraus gezogenen Schlüsse haben eine grössere Anzahl von Meinungsäusserungen hervorgerufen: zum grösseren Theil direct widersprechende, aber auch einige bestätigende und zustimmende. Die widersprechenden läugnen den ätiologischen Zusammenhang entweder ganz oder wollen ihn wenigstens nur in beschränktem Masse zugeben (Lehmann, Franz Müller, Westphal, Remak, Bernhardt, Gg. Fischer, Benedikt, M. Rosenthal), die andern finden ähnliche Zahlenverhältnisse, wie ich selbst (Gowers, 70 per Cent Syphilis, und Schanker; Erlenmeyer, 60 per Cent Syphilis; Dowse, "jeder Fall von Tabes, mit wenig Ausnahmen, kann auf Syphilis zurückgeführt werden") ausserdem ist mir noch eine Reihe von privaten Mittheilungen

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guter Beobachter zugegangen, welche meine Angaben durchweg bestätigen, und zwar zum Theil von solchen, welche sich früher meinen Angaben gegenüber ablehnend verhielten.

Alle diese Meinungen und Behauptungen gründen sich auf "Statistiken," auf Zusammenstellungen theils aus alten, nicht speciell auf diese Frage hin controlirten und erhobenen Beobachtungen, theils aus neuen, mit Rücksicht auf diese Frage gesammelten Fällen. Es ist nun sehr bezeichnend, dass die Ansichten unserer Gegner sich durchweg auf Statistiken aus alten Beobachtungen, auf "retrospective" Statistik gründen; sie fanden Alle viel geringere Zahlen, während alle neueren Beobachtungen den meinigen sich annähernde Zahlen ergeben. Dadurch wird der Werth dieser "retrospectiven" Statistik ein sehr zweifelhafter und da meine eignen "älteren" Beobachtungen ebenfalls, wie ich jetzt überzeugt bin, viel zu niedere Zahlen ergeben, glaube ich, sämmtliche Statistiken aus früheren Beobachtungen als unbrauchbar zurückweisen zu müssen; und dies um so mehr, als dabei meist auch viel zu wenig Gewicht auf die Ermittelung eines vorausgegangenen einfachen Schankers gelegt worden ist, und die damit behaftet gewesenen Kranken vielfach—gewiss mit grösstem Unrecht—als Nichtsyphilitische angesehen wurden.

Meine Gegner haben aber noch eine Reihe von Gründen und Einwänden ausgesprochen welche es ihnen—trotz der statistischen Zahlen—für ihre Person unmöglich machen sollen, einen ätiologischen Zusammenhang zwischen Syphilis und Tabes anzunehmen. Diese Gründe und Einwände mögen hier z. Th. berührt und als unhaltbar zurückgewiesen werden.

"Es seien keine sonstigen Zeichen von Syphilis bei den Necropsien von Tabischen gefunden worden."

Dagegen ist zu sagen, dass man sie vielleicht nicht mit hinreichender Sorgfalt gesucht hat; dass das Fehlen anderweitiger Erkrankungsherde durchaus nichts gegen die syphilitische Natur der Hinterstrangsclerose beweist, ebensowenig allerdings, wie die Anwesenheit von solchen viel für dieselben beweisen würde; Wilks und Dowse constatirten, dass in vielen Fällen von Syphilis, wo das Nervensystem besonders stark befallen erschien, Leber, Milz, Herz, &c. in der Regel frei waren und umgekehrt; übrigens hat Westphal unter 11 Autopsien von Tabes 2mal solche anderweitige Localisationen gefunden; in Leipzig* wurde sogar unter 8 Autopsien 3mal dasselbe constatirt.

"Die anatomische Veränderung in Rückenmark trage durchaus keine specifischen Merkmale an sich."

Auch hier ist zunächst zu sagen, dass man ja noch gar nicht darnach gesucht hat; ferner, dass die Syphilis bekanntlich ausser den als "specifisch" bezeichneten Producten auch noch andere Krankheitsvorgänge erzeugen kann, welche sich in nichts von einfachen chronischen Entzündungen, Sclerosen, Cirrhosen unterscheiden und welchen man ihren specifischen Ursprung und Character durchaus nicht ohne Weiteres ansehen kann.

"Die Syphilis könne nur Erkrankungen der Bindesubstanzen erzeugen, die Tabes sei aber eine parenchymatöse Erkrankung, könne folglich nicht von Syphilis herrühren."

Beide sind durchaus willkürliche und unbewiesene Behauptungen; direct gegen dieselben spricht überdiess ein von Westphal publicirter Fall mit exquisit parenchymatöser Erkrankung der Hinterstränge auf syphilitischer Basis.

"Die Syphilis könne, da sie vorwiegend diffuse Gewebserkrankungen bewirke, eine

* Dissertat. von Dr. Pusinelli.

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eigentliche 'Systemerkrankung' nicht bewirken; die Tabes sei aber eine 'Systemerkrankung.'"

Zunächst wäre zu beweisen, dass die Tabes wirklich eine Systemerkrankung ist, aber dies zugegeben, so würde gerade daraus zu folgern sein, dass die Syphilis eine Ursache der Tabes werden könne; ein "System" im Rückenmark ist doch eine Art Organ für sich mit specifischen Besonderheiten und Ernährungsverhältnissen; ein solches System kann auf bestimmte Gifte anders reagiren als andere Systeme; die Syphilis ist aber ein specifisches Gift; folglich nichts natürlicher, als dass sie auf einzelne—vielleicht gerade die bei der Tabes erkrankenden—Systeme mit besonderer Vorliebe einwirkt. Meine Gegner finden es doch gar nicht wunderbar, dass z. B. Erkältung diese sogenannte Systemerkrankung macht; es scheint mir aber viel schwieriger, sich eine solche, streng localisirte Wirkung der Erkältung vorzustellen.

"Das späte Auftreten der Tabesymptome nach der syphil. Infection spreche gegen einen Zusammenhang derselben, mit der lange vorausgegangenen Syphilis."

Dies wird einfach widerlegt durch die überaus zahlreichen Fälle, in welchen ganz sicher syphilitische Producte (Gummata, &c.), nach ebenso langer Zeit, nach ganz symptomfreien Intervallen, bei Männern mit gesunden Kindern p.p. auftreten.

"Es seien keine Zeichen von noch vorhandener Syphilis bei den Tabeskranken nachzuweisen."

Gewiss vielfach nur, weil man nicht genau darnach gesucht hat; das würde überdiess gar nichts gegen die syphilitische Natur der Tabes beweisen; macht etwa irgend Jemand die Diagnose eines Hirngumma von der Anwesenheit von Haut- oder Rachensyphilis abhängig? Ueberdiess findet man gar nicht selten überraschende Zeichen von Syphilis selbst bei älteren Fällen; nur drei Beispiele aus jüngster Zeit zum Beleg:

1.—Mann von 52 Jahren: 1858 secundäre Syphilis (nach einem Tripper!) Seit 1865 typische lancinirende Schmerzen; jetzt verschiedene andere Initialsymptome von Tabes; November 1880 am ganzen Körper keine Spur von Syphilis. April 1881: ganz characteristisches papulös-schuppiges Exanthem am linken Unterschenkel; ein seit mehreren Wochen bestehendes, flaches, scharfrandiges Geschwür am harten Gaumen.

2.—Mann von 40 Jahren: 1867 Syphilis; seit 1871 lancinirende Schmerzen; jetzt—April 1881—ausgebildete typische Tabes. 1879 eine unzweifelhafte syphilitische Affection des harten Gaumens (Ulcus, Caries, Perforation); durch Schmiercur mit grosser straliger Narbe geheilt.

3.—Mann von 47 Jahren: seit 7 Jahren an ausgebildeter Tabes leidend; vor vielen (ca 20) Jahren eine etwas zweifelhafte Syphilis, Angaben des Kranken darüber unsicher. Jetzt—Juli 1881—eine unzweifelhaft luetische Psoriasis palmaris und gleichzeitig eine specifische Hauteruption an der linken Brustseite.

Nach diesen und ähnlichen Erfahrungen ist es mir nicht im Mindesten zweifelhaft, dass man bei genauerer Untersuchung bei vielen Fällen von Tabes noch allerlei Manifestationen von Syphilis finden wird.

"Nur bei gewissen Bevölkerungsclassen und Nationalitäten komme Syphilis an sich häufiger und desshalb auch häufiger in der Vorgeschichte der Tabes vor."

Das Erstere ist zuzugeben; für das Letztere fehlt jeder Beweis; die Landbevölkerung soll viel weniger an Syphilis leiden als die Stadtbevölkerung; dem entsprechend finde ich auch die Tabes ausserordentlich viel seltener beim Landvolk.

"Die Syphilis sei speciell bei den Schichten der Bevölkerung und den Nationalitäten aus welchen sich meine Clientel rekrutirt, sehr häufig, folglich seien meine hohen Zahlen durch Zufall zu erklären."

Gegen diese gänzlich aus der Luft gegriffene Behauptung spricht mit unwiderleglicher Evidenz meine sogleich mitzutheilende Statistik der Nicht-Tabischen meiner Clientel.

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"Der unvollständige oder gänzlich negative Erfolg der antisyphilitischen Behandlung der Tabes spreche gegen deren specifischen Ursprung."

Auch dieser Einwand erscheint durchaus nichtig; Syphilis des centralen Nervensystems gibt überhaupt eine relativ ungünstige Prognose; speciell die syphilitischen Sclerosen können geradezu als unheilbar bezeichnet werden; bei der parenchymatösen Erkrankungsform der Hinterstränge ist wohl eine Restitutio in integrum kaum zu erwarten. Trotzdem existirt doch schon eine ganze Reihe von günstigen Erfolgen von Schmier- und Jodkuren. Vielleicht ist auch die richtigste und beste Methode der Behandlung noch nicht gefunden.

Aus allen den bisher vorgebrachten Einwänden ist somit durchaus kein entscheidender Schluss gegen die Wirksamkeit der Syphilis bei der Entstehung der Tabes zu ziehen. Die Entscheidung bleibt den Thatsachen und zwar zunächst den Thatsachen der Statistik, die hier jedenfalls ein viel grösseres Recht hat, gehört zu werden, als die pathologische Anatomie und die auf vorgefasste Meinungen basirte theoretische Speculation. Natürlich kann aber nur eine gute und sorgfältige Statistik, von zuverlässigen Beobachtern in Betracht gezogen werden. Und die Thatsachen sollen reden.

Zu meinen jüngst (im *Centralbl. f. die med. Wissensch.* 1881, No. 11 und 12) publicirten 100 Fällen, unter welchen sich 88 per Cent Inficirte fanden, sind seitdem 22 weitere Fälle hinzugekommen;* von diesen 122 Fällen zuzammen ergaben sich folgende Procentsätze: Fälle ohne vorausgegangene Infection, 10·6 per Cent; Fälle mit vorausgegangener Infection, 89·3 per Cent; (darunter mit secundär Syphilis, 59 per Cent;) (und mit Schanker ohne secund. Sympt., 30·3 per Cent.

In Bezug auf die Zeit des Auftretens der ersten Tabessymptome der Infectionslehre, 106 Fälle, bei welchen genauere Angaben vorliegen, dass dieselben kamen

Zwischen dem	3 und	5 Jahre	in	22	Fällen.
„	6 „	10	„	40	„
„	11 „	15	„	29	„
„	16 „	20	„	4	„
„	21 „	25	„	8	„
nach dem	25	Jahre	in	3	„

Es tritt somit die Tabes in 58 per Cent aller Fälle innerhalb der ersten 10 Jahre nach stattgehabter Infection, und in 85·6 per Cent der Fälle innerhalb der ersten 15 Jahre nach der Infection auf.

Zur Controle dieser Statistik nun habe ich die Gegenprobe gemacht und alle über 25 Jahre alten Männer meiner Clientel, die an irgend welchen anderen, nicht tabischen und nicht direct syphilitischen Erkrankungen litten (meistens Nervenkranke) genau auf etwaige früher vorhanden gewesene Infection examinirt. Das Resultat unter 500 solchen Personnen war, dass etwas mehr als 77 per Cent niemals inficirt waren, während beinahe 23 per Cent früher inficirt waren, und zwar 11 per Cent nur mit Schanker und beinahe 12 per Cent mit secundärer Syphilis.

(Auch Erlenmeyer hat eine solche Gegenprobe gemacht; er findet bei Tabes, 60 per Cent Syphilis; bei Dementia paralytica, 56 per Cent Syphilis; bei andren Neurosen, 8·5 per Cent., und bei andern Psychosen—13·8 per Cent!)

Die einfache Gegenüberstellung dieser Zahlen ist wohl schlagend genug: hier in gewissen Schichten der Bevölkerung kaum 23 per Cent früher Inficirter; bei den Tabischen aber aus denselben Schichten der Bevölkerung über 89 per Cent.

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* Es sei erlaubt zu bemerken, dass überall sowohl auf die klinische Diagnose wie auf die Ermittelung der anamnestischen Daten die grösste Sorgfalt verwendet wurde.

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Die Logik fordert hier denn doch mit unabweisbarer Nothwendigheit den Schluss, dass zwischen Syphilis und Tabes ein gewisser, ätiologischer Zusammenhang bestehen müsse; und die wissenschaftliche Berechtigung zur Annahme eines solchen Zusammenhangs kann jetzt doch angesichts dieser, aus relativ grossen Beobachtungsreihen gewonnenen Zahlen, gewiss nicht mehr bestritten werden.

Wir sind damit freilich noch sehr weit von der völligen Lösung des Problems entfernt. Die Schwierigkeiten, welche dasselbe umgeben, sind noch immer recht gross, die von mir schon in Baden-Baden erörterten Fragen: ob die Syphilis als wirkliche und alleinige Ursache oder als ein prädisponirendes Moment für die Tabes zu betrachten sei? ob die Tabes eine specifische Manifestation der Syphilis sei oder nicht? sie sind auch heute noch nicht zu beantworten. Die pathologisch-anatomischen und klinischen Thatsachen, welche ich dort als zur Lösung des Problems erforderliche bezeichnete, sind bis heute noch nicht beigebracht. Es muss auf diesem Gebiete noch viel gearbeitet werden; die Nothwendigkeit dieser Arbeit zu zeigen, die Wichtigkeit und Schwierigkeit der Frage immer und immer wieder zu betonen, ist der nächste Zweck dieser Mittheilung.

Ich muss aber noch einige erläuternde, für die Beurtheilung meiner Statistik nicht unwesentliche Bemerkungen hinzufügen; zunächst die, dass meine statistiche Zahl für die Häufigkeit vorausgegangener Infection wahrscheinlich noch zu gering ist, und zwar 1, weil die Kranken sehr viel vergessen von dem, was sie vor 10—20—15 Jahren gehabt haben; dafür liegen mir sehr schlagende Beispiele vor; von der absichtlichen Verheimlichung stattgehabter Infection gar nicht zu reden! 2, weil die Kranken sehr vieles übersehen und nicht an sich beobachten, auch häufig von den Aerzten nicht über die wahre Bedeutung der Affectionen aufgeklärt werden; 3, weil gelegentlich Infectionen ohne bemerkbare Primäraffecte, oder nach übersehenen oder falsch angesehenen Primäraffecten vorkommen. In dieser Beziehung ist besonders an den nicht seltenen Urethralschanker zu erinnern; sehr interessant sind auch die Zahlenangaben, welche Fumon in seiner Thèse über "Syphilis ignorée" in jüngster Zeit gemacht hat; endich 4, weil Leute, die etwa im 1, oder 2, Lebensjahr an hereditärer Syphilis gelitten haben, davon in der Regel wohl absolut nichts erfahren.

Aus diesen Gründen ist wohl anzunehmen, dass die wirkliche, richtige Procentzahl noch etwas höher sein wird, als 89 per Cent.

Es würden somit unter 100 Tabischen mindestens 90 früher mit Schanker oder Syphilis inficirt gewesen sein und zwar scheint es auch nach meinen Erfahrungen, dass es besonders die leichten und anscheinend harmlosen syphilitischen Infectionen sind, welche später von Tabes gefolgt werden; dies gilt bekanntlich ebenso auch für Hirnsyphilis.

Zur definitiven Lösung der Frage aber ist es unvermeidlich, hier der Frage des Verhältnisses des Schankers zur secundären Syphilis näher zu treten. Bei den tabischen sind ca 60 per Cent mit secundärer Syphilis, 30 per Cent die angeblich nur Schanker gehabt haben (die letztere Zahl ist wahrscheinlich viel zu gross, da geringfügige secundäre Manifestationen vielfach übersehen oder vergessen werden). Sind nun alle diese Kranke, die nur einen Schanker gehabt haben, schon als "syphilitisch" Inficirte zu betrachten?

Damit ist der noch immer schwebende Streit zwischen der unitarischen und der dualistischen Auffassung der Syphilis (resp. der Schankerinfection) berührt; ich unterschätze die ausserordentliche Schwierigkeit der Lösung dieser Frage nicht; dieselbe muss von den Syphiliologen entschieden werden.

So weit ich sehe, neigen sich neuerdings wieder viele, und gerade der erfahrensten Beobachter mehr der unitarischen Auffassung zu; ich selbst bin in der Sache nicht com-

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petent genug, bin aber ebenfalls, seit ich mein Interesse wieder mehr diesem Gegenstand zugewendet habe, von meiner früheren dualistischen Anschauung zurückgekommen und neige mehr der unitarischen Auffassung zu. Jedenfalls darf meines Erachtens aus dem Fehlen der meisten, oder selbst aller sog. secundären Manifestationen der Syphilis (der Haut und Rachenaffectionen, der Schleimhautpapeln, der Condylome, &c.), durchaus kein sicherer Schluss auf die nicht-syphilitische Natur eines Schankers gezogen werden; und die Annahme, dass bei jedem Individuum, das einmal einen infectiösen Schanker gehabt hat, wahrscheinlich auch wirklich syphilitisches Gift in den Körper gelangt (mag dasselbe sich darin zunächst bemerkbar machen oder nicht), scheint mir durchaus gerechtfertigt.

Für die unitarische Auffassung des "weichen" und des "indurirten" Schankers (resp. der Syphilis) scheint mir ein wichtiges und interessantes Paradigma die von Pasteur gefundene Thatsache, dass das Gift der Hühnercholera bei Meerschweinchen nur locale Abscesse erzeugt, die aber das wirksame Gift in Menge enthalten und von welchen aus auch die Meerschweinchen selbst unter günstigen Umständen "allgemein" inficirt werden können.

Doch es ist hier nicht meine Aufgabe, dieser Frage genauer nachzugehen; möchte sie bald in definitiver Weise gelöst werden!

Dann, erst dann, wenn die Frage im unitarischen Sinne gelöst sein wird, werden wir ein Recht haben, zu sagen, dass die Tabes in ca. 90 per Cent der Fälle durch Syphilis ätiologisch mit bedingt ist.

Dann würde sich aber auch fast als ein logisches Postulat die Aufgabe aufdrängen zu untersuchen, ob nicht alle Tabesfälle von Syphilis herrühren; eine bejahende Entscheidung würde eine nach vielen Richtungen hochbedeutsame Vereinfachung der Aetiologie der Tabes darstellen, Jedenfalls dürfte es sich sehr verlohnen, nach entscheidenden Thatsachen in dieser Richtung zu suchen. Hier kommt vor allem die Frage der Tabes bei Weibern in Betracht. Ich habe dieselbe bei meinen bisherigen Untersuchungen absichtlich bei Seite gelassen, weil über die etwa vorausgegangene Syphilis bei Frauen so ausserordentlich schwer eine entscheidende Ansicht zu gewinnen ist; klare und sichere Beobachtungen kann nur der Zufall liefern, der Hausarzt wird eher Gelegenheit zu solchen haben, als der Spezialist; vor Allem wird es hier darauf ankommen, nachzuweisen, ob nicht etwa der Ehemann syphilitisch gewesen ist (was bei 2 Fällen, die ich beobachtete, der Fall war), ob Abortus vorgekommen, ob hereditär syphilitische Kinder geboren wurden u. s. w.

Zum andern drängt sich hier die noch viel schwierigere Frage nach der hereditären Syphilis auf; es ist gewiss nicht unmöglich, dass Menschen, deren Eltern syphilitisch waren und die an hereditärer Syphilis erkrankt waren, aus diesem Grunde ebenso gut tabisch werden, wie solche, die die Syphilis durch eigne Schuld erworben haben. Aber wer erfährt jemals, dass sein Vater oder seine Mutter syphilitisch gewesen? Auch hier kann nur ein glücklicher Zufall entscheidende Thatsachen liefern; das nächstliegende Untersuchungsobject wären die Fälle von sog. "hereditärer Atagie (Friedreich); leider sind dieselben zu selten! Aber hoffentlich schreiten wir auch hier bald von Vermuthungen zu wohlconstatirten Thatsachen vor.

Viele und wichtige Fragen bleiben somit noch offen; zu ihrer Bearbeitung und Lösung immer und immer wieder anzuregen, halte ich für meine Pflicht. Ein wahrhaft schreckenerregendes Bild von der Wirkung syphilitischer Infection entrollt sich da; je mehr ich in den Gegenstand eindringe, desto klarer wird mir, wie vielseitig, wie verderblich, wie dauernd die Wirkungen dieser Seuche sind. Das erste Mittel, ihr entgegenzutreten, ist, die ganze Grösse und Gefahr ihrer Wirkung kennen zu lernen, daher meine dringende und immer erneute Aufforderung zu möglichst genauen und umfas-

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senden statistischen Untersuchungen auch auf dem enger umgrenzten Gebiete der Tabes.

DISCUSSION.

Dr. J. T. Banks, Dublin: In the numerous cases of tabes which I have observed in hospital and private practice, a considerable number were syphilitic, that is to say, they had at some period, often years before, been affected with the disease, or I was able to recognise a syphilitic taint. Admitting, then, the frequency of the connection, I am not prepared to admit from my experience the positive etiological connection of the diseases. The cases which I have met in which the connection existed were, I should say, ten per cent.

Sir William Gull, London: Syphilis has direct effects on general nutrition beyond the local grosser effects. Even the sweat may have an odour proper to syphilis. The question, therefore, whether syphilis is a cause of tabes, cannot therefore be determined by coarser anatomical data, but by clinical history alone.

Dr. J. Althaus, London: Professor Erb's paper goes far to establish an invariable, or almost invariable, connection between syphilis and tabes spinalis, but I cannot agree with him that such is really the case. I cannot admit that the unrecognized existence of urethral chancre explains those cases in which there have been no syphilitic history, nor can I admit the unity of the syphilitic form, considering that the evidence in favour of quality cannot be set aside. Eliminating these cases, the percentage falls considerably, and amounts only to little more than forty. It appears to me that the question might be discussed historically, by analogy, and by clinical and statistical evidence. Hippocrates, Galen, and other ancient writers had evidently seen cases of tabes, while it is at least probable that syphilis was only introduced into Europe at the end of the fifteenth century. Analogy shows that iritis, lupus, psoriasis, general paralysis of the insane, &c., occur in syphilitic as well as in non-syphilitic subjects, showing that syphilis does not originate, but rather imitates, morbid processes, leaving all tissues more liable to disease, more especially to local inflammation. I have analyzed 1,000 consecutive cases of nervous affections which have occurred in my practice with regard to a syphilitic history, and have found that while in epilepsy the percentage was only 4, in ataxy it was 90. Amongst these cases, however, there were about forty of simple primary sores, and others doubtful, so that only about 40 per cent. had occurred in truly syphilized subjects. The results of treatment speak somewhat strongly against any frequent connection between the two diseases, inasmuch as mercury and iodide of potassium generally do very little good, while nitrate and phosphide of silver, ergot of rye, salicylate of soda, electricity and the baths of Rehme and Nauheim, all of which have no influence whatever on the syphilitic diathesis, often prove beneficial in tabes. Iodide of potassium, however, should from time to time be given, as it appears to prevent the disease from spreading further; for I have seen several cases of ataxy in syphilitic subjects, where the entire withdrawal of the drug had apparently led to the appearance of general paralysis of the insane and epilepsy.

Dr. Wm. B. Neftel, New York: The question raised by Prof. Erb as to the relation of tabes and syphilis is certainly of great importance, and cannot be solved by the statistical method alone. I would propose a method based on the geographical distribution of diseases, which would contribute in this particular case, perhaps more than any other, towards settling this difficult problem. In 1857 and 1858, during my journey in the Central Asiatic steppes I had the opportunity of observing a nomadic race—the

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Kirghises—who present some very interesting features. In the first place the Kirghises live exclusively on animal food—on meat and fermented milk (Koumiss); they use no bread, potatoes, or other vegetable food. The Kirghises are entirely exempt from pulmonary phthisis and tubercular affections in general. On the other hand, syphilis is the most prevailing disease amongst them, and almost every Kirghis, male and female, bears traces of some form of syphilis. It is remarkable that the syphilitic affections of the Kirghises, as a rule, assume a mild course, and appear often self-limited. Their treatment is a very indifferent one, and consists most in the use of a decoction of a shrub, the name of which escapes my memory at present, but can be found mentioned in my article published in *Verhandlungen der physikalisch-medicin. Gesellschaft*, in Würzburg, 1860. Syphilis being almost a universal disease amongst the Kirghises, it is evident that tabes ought to prevail amongst them to a very great extent, if the assumption of Prof. Erb is correct—viz., if tabes is caused in a direct manner by syphilis. Should, however, tabes be exceedingly rare amongst the Kirghises, this would prove, on the contrary, that syphilis ought to be considered merely as a predisposing cause or complication of tabes. I would propose that this Congress, whose influence will extend all over the world, should put themselves in communication with the few physicians who reside on the river Syr-Darya, in Fort Perowsky, or even in the City of Orenburg, and ask them to direct special attention to this matter, and report the result of their personal observations.

Prof. W.T. Gairdner, Glasgow: I think that the great respect which we all feel for the opinions of Professor Erb would predispose most of us to accept his conclusions on any doubtful point within the field of pathology, on which he has given us such luminous researches, almost without question or criticism, and I am very far from being in a position to maintain, by statistics or otherwise, a theory or a dogmatic position opposed to his; but I must frankly admit that I am not prepared to accept, according to the present state of my information, the conclusions to which he points as probably the ultimate result of his inquiries—viz., that about nine-tenths of the cases of typical tabes dorsalis, or spinalis, have a syphilitic origin. I believe, indeed, that my own experience, especially of those cases with the antecedents of which I was more intimately acquainted, can by no means be conformed to this conclusion, or anything like it; and yet I would by no means be understood to affirm the absence of a syphilitic history in others, or even a proportion which might justify a belief in its being really a part of the etiological total contributing to the occurrence of this disease. The question is one of great difficulty; for I do not understand Prof. Erb to affirm that the mere anatomical form of the lesion in tabes, is conformable to the regular types of the recognized secondary or tertiary lesions of syphilis; but rather that there is some other mode of association, so remote on the one hand as to be quite apart from the usual evidences of these, and yet so close as to dominate probably nine-tenths of the cases in the aggregate, which he claims to have demonstrated by the statistical method. The whole burden of the evidence is thus thrown upon the statistics he has submitted to us: and this gives peculiar importance to the question how far the facts on which these numbers rest may have been insensibly biassed by the personal convictions under which the inquiry was undertaken. Now here it is to be observed that, to carry the full weight of Professor Erb's conclusion, it is essential, even according to his own statement, that the unitarian view of syphilis shall be adopted; and under that view it is further necessary to affirm that the primary syphilitic sore may in many cases be wholly devoid of the consequences usually regarded as the only sure evidences of constitutional infection. Nay more, it is these "milder cases" that, according to the statement, form the bulk of those making up the statistical record of

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syphilis as a cause of tabes dorsalis. Therefore, the evidence of syphilis in these cases is for the most part not the fact, but the more or less vague presumption of an antecedent chancre years before, which again may, or may not, in the opinion of many or most of us, have been truly a syphilitic sore. Of course, in the very full and rigid investigation to which Professor Erb probably submitted all his cases of tabes, it may be supposed that the full amount of evidence was elicited which, upon his own theory, would justify belief in a remotely antecedent syphilitic history; and I do not at all dispute the importance of the facts so elicited. But the question on which the full force of the statistical conclusion depends is—was an equally rigid investigation, and one conducted on precisely the same general principle, applied to those 500 cases occurring in Professor Erb's *clientèle*, in which 77 per cent. were found to be non-syphilitic? From these 500 cases, it is to be observed, those suffering "directly from syphilis" were purposely excluded; it is, therefore, a selected class to that extent, and the question now comes to be—was it even possible to Professor Erb, or to any one else, however laborious or trustworthy, to put such questions and get such answers, in a set of cases entering his consulting-room in succession, and not avowedly syphilitic, as should amount to the kind of investigation bestowed upon the ataxic cases, and lead to anything approaching to paralellism in the statistical results? I will only say that, in my opinion, as a hospital and consulting physician, that was not possible; and therefore, while I do not think it is possible for any of us here to dispute entirely (and I have no wish to do so) the remote association of syphilis with locomotor ataxy as one of its etiological factors, it is, in my opinion, not less difficult for us to assent at present to his conclusions as stated numerically. There is a gap, to be filled up no doubt by-and-by, in the evidence; and it can only be filled up in the way suggested by Sir James Paget in his opening address, through "the collecting of facts well observed by many persons." I say by many, not only because many facts are wanted; but because, in all difficult research, it is well that each apparent fact should be observed by many; for things are not what they appear to each one's mind. In that which each man believes that he observes there is something of himself; and for certainty, even on matters of fact, we often need the agreement of many minds, that the personal element of each may be counteracted.

Professor Lancereaux, Paris: Professor Erb, in statements based solely on statistics, considers syphilis as one of the etiological factors of locomotor ataxy. I regret to be unable to share that view, and I believe that from statistics alone a solution of the question cannot be found. By parallel reasoning one might urge, from the frequency of syphilis at certain periods of life in the great centres of population, that most diseases, even typhoid fever, have a syphilitic origin. The solution of a question of this sort rests, according to my mind, on the anatomical and clinical character of the affection which produces it, else it is impossible to arrive at any exact conclusion; or the characteristics of syphilis are the anatomical lesion to which the symptom is subordinate. Therefore, in the study of this lesion and its development, the solution of Professor Erb's problem must be sought; we must therefore examine the general characters of the manifestations of tertiary syphilis and compare them with those of locomotor ataxy. The lesions of tertiary syphilis never invade an organ through its whole extent; they are characterised by being circumscribed, by forming single or multiple nodes grouped on one or several spots, which undergo a granular, fatty, or hyaline degeneration, and terminate always in loss of substance or cicatrix. Take the most obvious lesions, for instance, those of the skin; they show all these characters, they are circumscribed, affect only a small portion, and always terminate in scars. The lesions of the liver, kidneys,

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&c., are similar. As to syphilitic changes in the brain and its membranes, they are also confined to only a small portion of these organs. Yesterday I examined in the amphitheatre of Guy's Hospital, Dr. Moxon's drawings of syphilitic lesions of the spinal cord, and found in them all the characters I have just dwelt upon. The lesions of locomotor ataxy have not the same characters. Instead of being circumscribed they are diffuse, extended, systemic, and, although slowly developed (like those of syphilis), they do not produce loss of substance, nor do they leave a scar. Consequently, I believe we may affirm that locomotor ataxy has no etiological relation to syphilis, since the lesions have quite opposite characters. I nevertheless acknowledge, with Professor Erb, that locomotor ataxy is frequent among syphilitic persons, and I am perhaps the first who has observed this frequency. If you carefully examine the conditions of those individuals who become ataxic, you find very often that they confess to venereal excess, or have been largely exposed to syphilitic contagion. In the female sex, locomotor ataxy is found almost exclusively in prostitutes, or, as I have noticed, in women exposed by their occupation—as for instance, those who work sewing machines—to excitation of the genital organs. Thus, it would seem that affections which many authors are inclined to attribute to syphilis are really the consequence of the prolonged and evil action upon the spinal cord of a more or less normal sexual excitement.

Professor Rosenstein, Leyden: The solution of the question of the relation of tabes to syphilis will not, I am afraid, be facilitated by the communication of the observations of various men, until the principles have been laid down upon which the decision is to be made. Statistics, as employed by Professor Erb, are quite justified, since they exhibit the special points of his own experience in their etiological development, and form novel statistics on which original experience can be grounded. Another most important point is that touched upon by Professor Lancereaux—viz., that the anatomical changes of syphilis are circumscribed, those of tabes diffuse. But this assertion can hardly be admitted as correct, for—apart from changes in the vessels—quite diffuse affections of the liver and kidneys are met with. It is for this assembly to agree upon the principles to be laid down for this question, and in this respect my experience leads me to range myself on the side of Professor Erb.

Dr. Zambaco, Constantinople: I am of the same opinion as my *confrère* Lancereaux, that we must not misuse statistics with regard to syphilis. What percentage of persons exist who have never had syphilis? Syphilis might be accused of producing every chronic disease; but Professor Lanceraux stated one fact I cannot let pass. He does not admit a functional lesion of syphilis, he considers that every nervous trouble due to syphilis is necessarily connected with some organic lesion. He said that syphilis attacks the nerve-centres when it has arrived at the period when it destroys the tissues and leaves in them indelible traces. I have published several facts with post-mortems in my book on Nervous Syphilitic Affections, and have proved that syphilis without appreciable lesions may greatly disturb the nervous functions. The fact mentioned by Professor Lanceraux himself, that syphilis produces permanent destruction when arrived at the period at which it affects the functions of the nerve-centres, furnishes me with an argument against him. How are we to understand the complete disappearance of all traces of syphilitic lesion of the brain and spinal cord, if an appropriate anti-syphilitic treatment has cured the patient, and the functions of the nerve-centres are restored to their former integrity? Does the brain, in its infinitely more delicate texture, preserve no cicatrix or evidence of a permanent destruction of tissue like the skin and the other organs? We know that, once modified by material lesions, it never regains its primitive condition, and

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therefore, if the functions reappear intact, there can have been no organic lesions. We may therefore conclude that there may be functional troubles without material lesion.

Professor Erb, Leipzig (in reply): I am not surprised to find so many opponents to my views, since my conclusions are based on numerous and recent observations. A number of the objections raised in the course of the discussion have been already dealt with in my paper, but some remarks as to principle may still be added. Statistics are quite capable of deciding the question, but they need to be those of a careful observer, while retrospective statistics are quite inadmissible. Control statistics I consider most important, and I was careful to expend as much care in the examination of the 500 cases, included in my table, as I did on the cases of tabes. The unitarian theory of the syphilitic virus is of much importance, it is for the syphilologists to determine. I cannot share the scruples of Professor Lancereaux against the use of statistics in this question, still less can I agree with the exclusively anatomical standpoint taken up by him. I am firmly of opinion that anatomical examination and the microscope are incapable of definitely deciding what is syphilitic, and what is not; here (as in the case of tubercle) the etiological moment must be decisive. Again, I doubt whether many would acknowledge that syphilis gives rise to no diffuse processes. Finally, I repeat that the question is in my opinion by no means definitely settled, moreover I believe that at present it cannot be decided, because we know so little of the real nature of tabes or syphilis; it can only be advanced in time by means of clinical and statistical observations.

On Graphic Tracings of Tendon Reflexes.

Professor Eulenburg, Greifswald.

Allow me to obtain your attention a few minutes for the demonstration of my graphic tracings of the tendon reflexes (especially of the knee phenomenon). The method of this graphic exhibition itself, and its chief results, you will find briefly explained in the "Abstracts" (p. 82), to which I refer, in order not to abuse your time by the repetition. I shall first ask you to look at specimens of curves taken from healthy persons, in order to ascertain both the duration of the latency and that of the convulsion itself. There are generally a pair (or even two pairs) of curves delineated in the same plate; the one of which (usually the upper, marked with A) is to indicate the moment of incitation—*i.e.* of percussion of the lig. patellae, the other (marked with B) the moment of commencing muscular contraction and the processes of the convulsion itself. Experience gained shows that the duration of the latency usually does not exceed the thirtieth part of a second (more exactly 0·03226), but sometimes (especially in children) amounts to nearly the twentieth part of a second (0·04839). The duration of the convulsion varies in adult healthy persons from about the eighth to the fourth part of a second (0·11291—0·22582). Considering the length of the centripetal and of the centrifugal tracts which are to be traversed, we must presume, of course, that the actual interception of the reflex in healthy persons cannot be but a very small one. Experiments made on the same healthy persons successively or on different occasions give almost the same invariable results, as you may see by curves taken from the same man, at very different times (Fig. 2); while there are enormous differences both between the curves of healthy and sick people and those taken from patients with various nervous diseases, or under the action of various medicaments and poisons. In ataxic tabes you naturally will not find even the least elevation above the level; the curve absolutely

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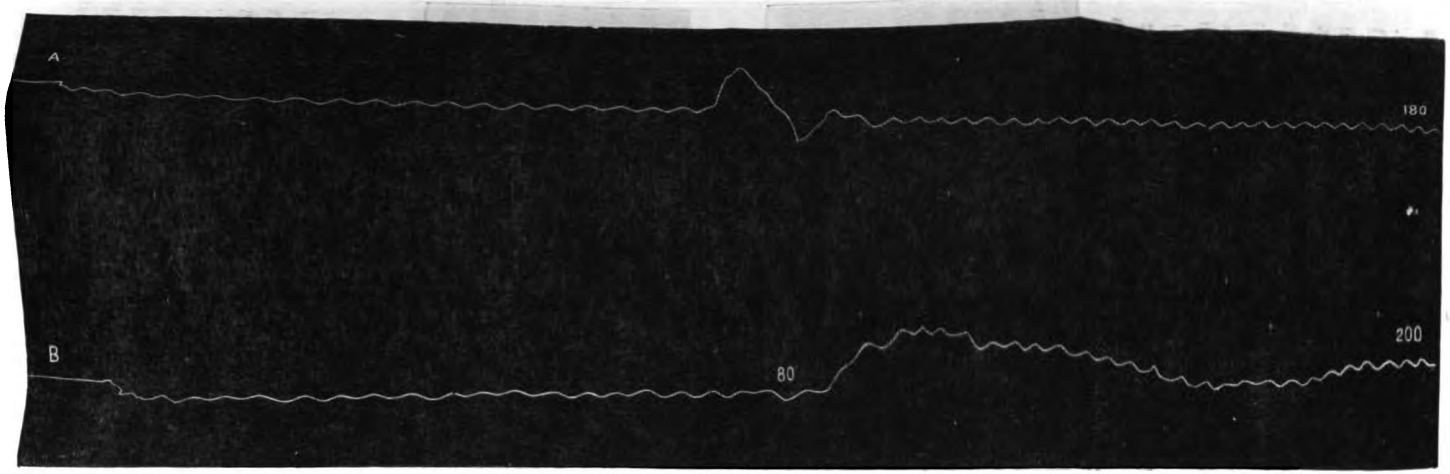

Fig. 2 represents the knee phenomenon of a healthy young man (the upper line, A, indicates the moment of percussion; the lower, B, the muscular contraction itself); the interval 2 vibrations of the tuning fork=0·03226" second.

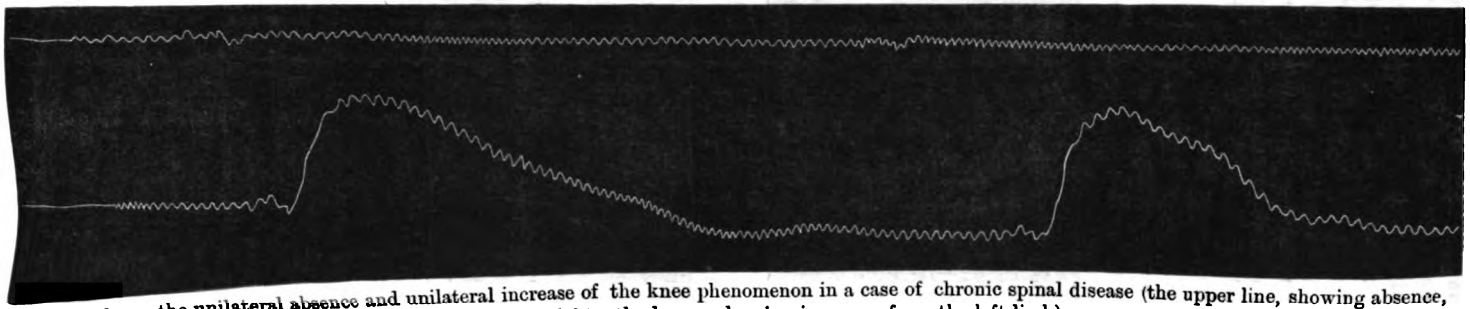

Fig. 3 shows the unilateral absence and unilateral increase of the knee phenomenon in a case of chronic spinal disease (the upper line, showing absence, taken from the right; the lower, showing increase, from the left limb).

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shows no other thing than the regular vibrations of the tuning-fork, as you remark in these curves taken from seven or eight tabetic individuals (compare them with other plates exhibiting the voluntary contractions of the extensory muscles in the same persons, Fig. 4. On the other hand you may verify in cases of spasmodic tabes (spastic spinal palsy, lateral or disseminated sclerosis) the enormous augmentation of tendon reflexes, both by the excessive elevation above the level and by their prolonged duration; and at the same time the extreme diminution of the period of latency, which indeed sometimes not even amounts to the sixteenth part of a second (remaining under 0·01613), Fig. 3. You may obtain the same results as with the knee phenomenon, also with other tendon reflexes, for instance with the adductory reflexes (as you may remark in Fig. 5). In cases of unilateral atrophic spinal affection (atrophic spinal palsy of adults and children, &c.) you may recognise the eminent difference of the curves of limbs, comparatively on the same plate: the knee phenomenon being quite absent on the one and appearing as

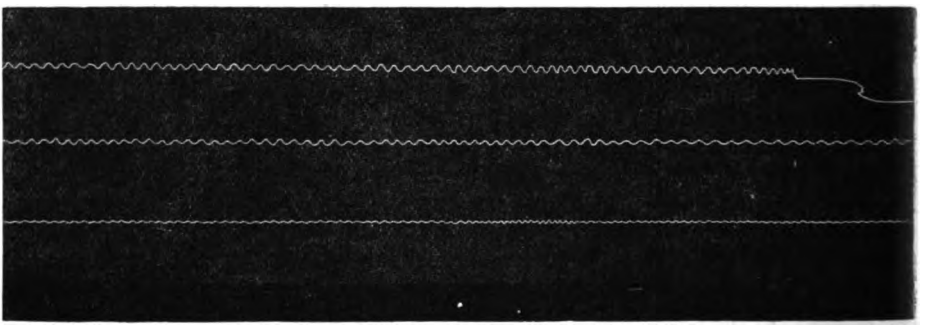

Fig. 4 shows the complete absence of the knee phenomenon in tabetic persons.

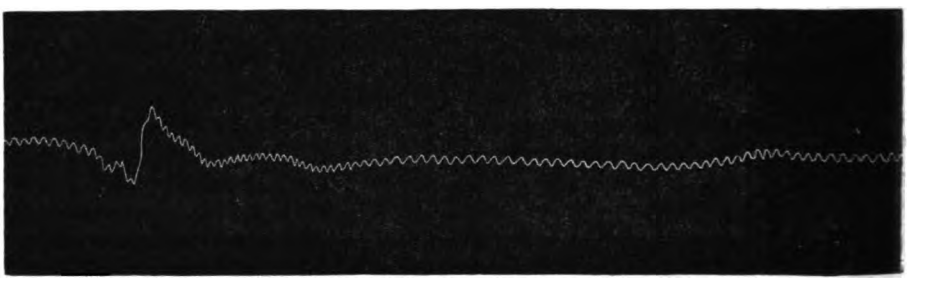

Fig. 5, the (increased) adductory reflex of the left limb of the patient from whom Fig. 3 was obtained.

usual or even increased and excessive on the other side, &c. In cases of tremor and shaking palsy, &c., you may besides easily recognize the characteristic forms of trembling convulsions, faithfully exhibited by the muscular tracings. In some cases, in which the knee phenomenon seemed entirely abolished on on one side, I succeeded in making it reappear for some time by the hypodermic injection of strychnia, made in the neighbourhood of the nervus cruralis or into the substance of the extensor muscles. The reflex reappeared and was gradually increased from 5–15 minutes after the injection (of one milligramme of nitrate of strychnia). The period of latency is notably shortened by injections of strychnia, while it is considerably lengthened by some important anæsthetic and narcotic remedies (such as chloroform, bichloride of methylene, some

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bromides, &c. &c.) in high doses. I should not insist on the single points, but in mentioning them I dare hope that this graphic method permitting a very high degree of exactness, it may be of some profit as well for the clinical diagnosis of dubious cases, as for the pharmaco-dynamic investigation of poisons and remedies acting upon the nervous system.

DISCUSSION.

Dr. AUGUSTUS WALLER, London: I do not believe it to be reflex at all, for the latent period is that of a direct phenomenon, because the latent period is identical with that of electrical stimuli, or of percussion directly applied to the muscle. I do not believe in the existence of "crossed reflex," because this phenomenon persists in rabbits after section of all nerve paths of the percussed limb.

Sur une cause peu connue de la Tabes Dorsale Spasmodique.

Docteur BRUNELLI, Rome.

Parmi les causes de la tabes dorsale spasmodique, les modernes neuropathologistes ne comptent que celles qui sont communes à la plupart des neuropathies (action du froid, traumatisme).

J'ai observé, il n'y a pas longtemps, dans mon cabinet électrothérapique de l'Hôpital du St. Esprit, cinq individus atteints de tabes dorsale spasmodique, et tous les cinq avaient été reçus au bureau de réception presque à la même époque (mois d'Octobre, 1880).

Je fus étonné de voir cette espèce d'invasion d'une maladie, dont auparavant je n'avais eu occasion de voir que quelques exemples bien rares, sur des individus ambulants.

Voilà maintenant les observations, que je viens de faire sur eux. Tout d'abord ces individus (quatre hommes et une femme) provenaient tous d'un même arrondissement du Royaume (Alatri, province de Rome); c'étaient tous des paysans, dont trois appartenaient à la même famille, et tous les cinq s'étaient nourris, pendant quelques mois de disette, d'un pain, moitié farine de blé, et moitié farine de grain de *Lathyrus cicera*, appelé communément par ces paysans *mochi*, en anglais *chickling*, *vetch* légumineuse qui est très-connue dans le midi de l'Italie.

J'appris d'eux-mêmes que dans l'arrondissement d'Alatri, il existait d'autres individus, atteints de la même maladie, pour avoir fait usage de la même nourriture.

Je vérifiai l'existence réelle du fait, en me transportant exprès à Alatri, avec mes confrères, les Drs. Sciamanna et Salaghi. J'y trouvai en effet six autres individus atteints de tabes dorsale spastique. Ce sont tous des jeunes gens, tous au dessous de 35 ans environ, presque tous des hommes. Les vieux et les enfants qui se nourrirent, comme eux, du même pain, ne furent pas atteints de ce mal. Dans ce pays cette maladie n'était pas connue, avant 1875, époque où l'on vendit sur cette place une grande quantité de graines de *lathyrus* à fort bon marché.

Les premiers symptômes de la maladie consistaient en faiblesse et tremblement des jambes; chez quelques uns, il se manifestait une espèce d'ivresse, surtout après le repas, quand celui-ci était exclusivement composé du pain susdit.

Depuis l'apparition des premiers symptômes, ceux qui s'abstenaient complétement de la nourriture nuisible, en étaient quittes. Si, au contraire, les conditions d'une misère extrême les obligeaient à l'usage trop prolongé du pain de cicerole, la maladie faisait alors des progrès rapides, et, au bout de deux ou trois mois, elle acquérait la forme caractéristique de la tabes dorsale spasmodique.

Quand je vis pour la première fois les cinq malades de l'hôpital, c'est-à-dire vers

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la fin du mois d'octobre 1880, il y avait déjà deux ans que la maladie avait commencé. Tous présentaient les caractères suivants, que je vérifiai aussi chez les six tabides que j'eus occasion de visiter à Alatri : Une grande roideur de jambes, une marche très-gênée, à pas courts et les pieds trainants sur le pavé, les jambes serrées par la contracture des adducteurs de la cuisse, les doigts des pieds contractés dans le sens de la flexion, le talon soulevé du sol, par la contracture des gastronémiens; un de ces malades marchait absolument sur la pointe des pieds. Ceux qui sont le plus atteints, lorqu'ils se trouvent assis, semblent encloués sur la chaise, et ce n'est qu'après de longs efforts qu'ils réussissent à se lever et à vaincre en partie la contracture ; ils se tiennent mal sur les béquilles, et bien pis sur des bâtons de mesure ordinaire; mais ils ont besoin de bâtons longs et solides qu'ils saisissent des deux mains du côté supérieur. La nutrition musculaire est normale; il n'y a aucun trouble de la sensibilité générale et des sens spécifiques.

Malgré mes recherches électro-diagnostiques, je n'ai pu découvrir aucune marque importante; car chez les uns je remarquai une certaine diminution de la sensibilité et contractilité électrique, chez les autres, celles-ci semblaient plutôt exagérées; le dynamomètre oscillait entre 20 et 28. Rien d'anormal du côté des sphincters, aucun symptôme cérébral. L'examen ophthalmoscopique ne fit constater rien d'anormal dans la papille. Le phénomène du tendon était exagéré, moins dans l'avancement du pied, que dans l'instantanéité du mouvement. Cette marque caractéristique, d'après moi, se trouve dans la tabes spastique plutôt que dans les autres maladies cérébrales et spinales, dans lesquelles le dit reflex est plus exagéré dans l'avancement que dans la célérité du mouvement. Ajoutez à cela la trépidation spontanée et l'épilepsie spinale, et vous aurez un tableau assez exact de l'état de ces malheureux.

Je n'ai rien d'important à vous communiquer sur le traitement. Mais il me semble que l'application du courant galvanique a réussi un peu à modifier l'état spastique des muscles. Je l'ai appliqué en direction ascendante, des pieds à la région dorso-lombaire, en me servant d'un courant à forte tension (100 petits éléments de Daniell), et en employant une méthode d'application que j'appelle *a piediluvio elettrico*, et qui consiste en faire passer le courant par les pieds préventivement placés dans une cuvette de porcelaine remplie d'eau salée; méthode que j'ai trouvée d'une très-grande utilité aussi dans les paralysies infantiles.

Maintenant qu'il me soit permis de faire quelques observations. Les épidémies *di storpio* comme on les appelle chez nous, c'est-à-dire *des estropiés*, survenues dans des moments de disette par l'usage des légumineuses du genre du *lathyrus*, n'est pas un fait nouveau, comme vous savez.

Tout en laissant de côté Hippocrate et Galien qui notèrent le *crurum impotentia* chez ceux qui se nourrissaient d'*Ervum*, légumineuse qui est analogue au *lathyrus ;* Ramazzini Vallisnieri et Targioni-Tozzetti en parlèrent déjà le siècle passé, et ce dernier les appela *epidemie di storpio*. Duvernoy et Virey traitèrent aussi le même sujet. Il y a environ trente ans, M. le Docteur Pellicciotti observa une épidémie de ces neuropathies survenues dans les Abruzzi par l'usage du Lathyrus ; et même dernièrement (1873) M. le Prof. Cantani en a illustré deux cas dans la clinique de Naples, en proposant d'appeler la maladie Lathyrisme de même que l'on avait appelé Ergotisme la maladie qui provenait de l'ergot de seigle.

Si l'on considère maintenant que les médecins du siècle passé appelèrent épidemie *di storpio* et non pas épidémies de paralysie les maladies qu'ils venaient d'observer, nous ne pouvons pas nous défendre d'apercevoir, sous cette dénomination, l'idée de ce qu'on appelle aujourd'hui démarche paralytique d'Erb. En effet les individus atteints de tabes dorsale spastique ont plutôt l'aspect d'estropiés que de paralytiques.

Pour moi donc il est prouvé, que l'usage trop prolongé du *Lathyrus cicera* peut

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chez un individu causer la tabes spasmodique dans sa forme caractéristique et telle que nous l'a décrite le Dr. Erb qui a été le premier à bien marquer les lignes qui séparent des autres myélites systématiques ce groupe spécial, en lui donnant aussi, par simple analogie, le substratum anatomique de la sclérose des cordons antéro-latéraux.

Un autre fait bien constaté est que les cochons qui se nourrissent des plantes fraîches du *Lathyrus cicera*, au bout de quelque temps, traînent les extremités postérieures; ce qui fût aussi remarqué, il n'y a pas longtemps, par un de mes collègues le Dr. Ferrares lorsqu'il se trouvait en qualité de médecin dans les Abbruzzi. On a aussi remarqué que les graines dessechées des plantes susdites, données comme nourriture, sont nuisibles non seulement aux cochons, mais aux lapins, aux chiens, &c. Nous nous trouvons donc devant certains faits qui nous indiquent non seulement une cause évidente de la véritable tabes spasmodique, et que les auteurs modernes de neuro-pathologie ont complétement negligés, mais qui nous fournit en même temps le moyen de créer artificiellement sur les animaux la maladie dont nous venons de parler, et d'en étudier ainsi, sur une large échelle, les altérations anatomo-pathologiques, toujours controversées. Je suis même d'avis que l'on pourrait, moyennant le même système expérimental, essayer d'autres substances, dont l'action sur les centres nerveux est bien connue, comme par exemple, la nicotine, l'acide sclérotique, la strychnine, le brome, l'arsenic, &c.

Maintenant je suis en train de faire des expériences avec la farine des graines de *lathyrus* sur les lapins; mais, faute de temps, il m'est impossible de présenter au Congrès une conclusion bien certaine. La farine du *lathyrus* tue trop vite les lapins, de manière que je n'ai pu obtenir dans mes premiers tentatives la forme morbide de l'empoisonnement chronique. Il faudra par conséquence que je m'en tienne aux cochons qui présentent une résistance plus grande et chez lesquels l'action paralysante du *lathyrus* est très-bien connue, comme je vous ai dit.

Le but de cette modeste communication au Congrès International de Médecine, est le suivant:

1.—De mettre en garde les neuro-pathologistes pour qu'ils recherchent la tabes dorsale spasmodique, maladie relativement rare, dans les endroits où l'on cultive *les lathyrus* et d'autres plantes semblables, et dont on fait usage comme nourriture de l'homme. Je laisse aux hygiénistes ce qui leur revient de droit dans cette question.

2.—De suggérer l'étude anatomo-pathologique de cette maladie par la pathologie expérimentale.

Der Entwicklungsgang der Tabes und ihre Behandlung mit Nervendehnungen.

Dr. Carl Langenbuch, Berlin.

Meine Herren! Ich möchte Ihnen über den Entwicklungsgang der Tabes, sowie über die Behandlung dieser Krankheit mit Nervendehnungen einige kurzgefasste Sätze vortragen.

Die Tabes wird mit Unrecht als Rückenmarkskrankheit aufgefasst, da doch in der grösseren Anzahl der Fälle auch das Gehirn vom gleichen Processe befallen erscheint.

Man müsste also von Tabes des Centralnervensystems sprechen.

Aber auch diese Namensfassung ist anfechtbar, denn es ist zwar bisher allgemein angenommen, aber nicht bewiesen, und nach den neuesten Erfahrungen auch nicht mehr so wahrscheinlich, dass die Krankheit als centrales Leiden beginnt.

Die pathologische Anatomie hat freilich, soweit es das Rückenmark angeht, bisher

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nur degenerative Veränderungen in den Hintersträngen und deren benachbarten Abschnitten nachweisen können.

Sie hat aber auch constatirt, dass die Processe in der Regel in der äussern Hälfte der Hinterstränge beginnen und dass erst im weiteren Verlaufe die innere Hälfte der Hinterstränge mit afficirt wird.

Durch die berühmten, entwicklungsgeschichtlichen Studien Flechsig's wissen wir, dass die äussern Abschnitte der Hinterstränge, die sogenannten Keilstränge, grösstentheils directe Fortsetzungen der peripheren Nerven sind, während die innern, dem Sulcus posterior anliegenden Hälften, die gracilen oder Goll'schen Stränge genannt, in keiner directen Verbindung mit den peripheren Nerven stehen.

Wenn also der Theil der Hinterstränge, welcher die Brücke für den Eintritt der peripheren Nerven in das Rückenmark bildet, ja nach Flechsig selbst noch aus peripherer Nervensubstanz besteht, wenn also dieser Theil immer die sichtbaren, ersten Veränderungen zeigt, während die Goll'schen Stränge noch frei bleiben, so ist daraus der Wahrscheinlichkeitsschluss zu ziehen, dass in dem Wesen der Peripherität der Keilstränge auch die Ursache für ihre Erkrankung gesucht werden darf, und es erhellt aus diesem, sowie aus manchen klinischen Thatsachen, namentlich aber auch aus den vielfachen, makroskopischen Veränderungen, welche man an den zum Zwecke der Dehnung freigelegten grossen Nervenstämmen regelmässig wahrnimmt, der Schluss, dass die peripheren Nerven eine wichtige Rolle bei der Entwicklung der Krankheit spielen.

Ich halte es für sehr wahrscheinlich, dass die peripheren Nerven zuerst erkranken, dass der Process durch sie in die Keilstränge, also die äussere Hälfte der Hinterstränge geleitet wird, und dass erst von hier aus ein Uebergreifen auf andere Abschnitte des Centralorgans stattfindet. Die Krankheit wird also, kurz gesagt, erst später central, nachdem sie längere Zeit bloss peripher gewesen ist.

Für diese Sätze, mehr aber noch für die gegentheilige Ansicht, dass nämlich die Tabes ausschliesslich nur als centrales Leiden im Rückenmark verläuft, steht der stricte Beweis noch aus.

Indessen für meine Ansicht spricht noch das "e juvantibus;" ich meine die von mir inaugurirte Therapie der Nervendehnung, welche das Leiden an seinem peripheren Sitze angreift, und von da aus bis in das Centrum verfolgt.

Ich glaube, dass unter den mancherlei Schädlichkeiten, welche auf die Summe der nervösen Endapparate in der Haut wirken können, der Erkältungsprocess obenan steht, dass aber auch die Syphilis die Defensivkraft der Nerven gegen äussere Schädlichkeiten erheblich herabsetzt und dadurch als dispositionserzeugend eine ernste Rolle bei der Entstehung der Tabes spielt.

Mag dem nun mit der Aetiologie sein, wie es wolle, unsere bisherige Therapie suchte vornehmlich mit Arzneimitteln, Badecuren, oder der Electricität auf dem Wege der peripheren Nerven auf das Rückenmark einzuwirken. Die Erfolge sind, wie Ihnen bekannt, nicht sehr befriedigend.

Meiner Meinung nach sind diese Mittel an sich sehr richtig gewählt, aber sie finden zu viele Hindernisse um, auf die krankhaft ernährten Nervenstämme genügend alterirend einwirken zu können.

Man suche also nach einem directeren Angriff, und den glaube ich sicher in der Dehnung der grossen Nervenstämme in den Extremitäten, besonders den untern, gefunden zu haben.

Sie bilden einen erwünschten Insult, der weithin die Nervenscheide zerreist und dadurch die Ernährung der Nervenstämme zeitweilig unterbricht. Eine Folge davon dürfte der Untergang der meisten oder aller kranken, also schlecht fungirenden Nerven-

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fasern sein, an deren Stelle und unter dem Einflusse einer neuentstandenen Ernährung sich neue gesunde Leitungsfasern bilden können.

Aber nicht nur eine Erneuung der kranken peripheren Nerventheile tritt ein, sondern auch eine deutlich umstimmende Wirkung auf das Rückenmark und Gehirn; auch hier scheint sich das noch lebensfähige Kranke wieder zu erholen, was daraus erklärt werden könnte, dass die Ernährungsvorgänge im Rückenmarke von den normalen oder anormalen Erregungszuständen der peripheren Nervenwelt sicherlich abhängig sind.

Es versteht sich von selbst, dass definitiv abgestorbene Nervenelemente nicht wieder ersetzt werden können, und dass überhaupt und namentlich bei schon vorgeschrittenem centralem Leiden nur von einer Besserung, nicht aber von absolut vollkommener Heilung die Rede sein kann.

Ebenso ist es erklärlich, dass frischere Erkrankungsfälle verhältnissmässig leicht, die späteren dagegen nur unvollkommen geheilt resp. gebessert werden können. Doch kann ich behaupten, dass ich auch bei den schwersten Fällen von ca. sechzig Tabikern, welche ich operirt habe, stets noch bis auf heute zunehmende Besserungen fast aller Symptome beobachten kann.

Zum Schlusse spreche ich die Hoffnung aus, dass in der Zukunft die traurigen Formen und Ausgänge der Tabes immer weniger zahlreich werden, wenn die ärztliche Welt die Therapie der Nervendehnungen adoptirt und weiter ausbildet, und vermittelst ihrer namentlich schon die Anfangsformen der Krankheit zu bekämpfen sich entschliesst.

DISCUSSION.

Professor Erb, Leipzig: I think that a discussion of this subject is very important, but I cannot immediately agree with the theory of tabes which Dr. Langenbuch has established, nor with the views which the said gentleman has expressed about the anatomical and physiological meaning of the *bandellettes externes*. Not only the experimental, but also the operative, results which have been made up to this time appear to contradict the view of their peripheric nature. I think that the results of nerve-stretching are considerable and worthy of attention; but that nothing definite can be said as yet, either about the theory of this method of treatment, or about the limits of its efficacy.

Dr. F. Müller, Graz: Since April I have observed nerve stretching in a number of cases of tabes; the immediate result has always been a surprising one, whether it will be a lasting one cannot yet be decided. But this much may be definitely stated to-day, that we possess in nerve stretching a very excellent remedy, better than any other symptomatic remedy known for some symptoms of tabes, but there can be no cure of tabes. On the other hand, I should not like to consider the operation as so little dangerous and harmless as is sometimes thought. As a proof that nerve stretching has not only influence peripherically, but also centrally, I may remark that, in a case where the left crural nerve was stretched, the sole-reflex disappeared, but at the same time there appeared in the other leg, which was not stretched, hyperæsthesia and hyperalgesia, a clear sign that in man the effects are the same as Brown-Séquard noted them to be in animals.

Professor Benedikt, Vienna: I consider nerve stretching a very valuable addition to the therapeutics of tabes. It may have a favourable influence on all the symptoms; and not only in the leg operated upon, but also in the other one, and also on symptoms which are connected with the direct course of the stretched nerves. Nothing can as yet be said about the duration of the stretching; but the necessity of a repetition of the operation after a considerable time is not so important. The operation loses much of its gravity by

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the iodoform-ligature, as the wound heals without drainage in a few days by first intention. Whether the theoretical views of Professor Langenbuch are right is not the first consideration, but rather his merits in devising a method of relief for such a sad disease.

Professor GRAINGER STEWART, Edinburgh: The usefulness of nerve stretching in locomotor ataxia must be admitted to be well ascertained. The difficulty is to find a satisfactory explanation. Certain cases, of which I recently published an account as examples of paralysis of hands and feet from disease of the nerves, seem fitted to throw some light on the subject. In these cases there was a peculiar change in the axis cylinder of many of the nerves—a change identical with that met with in locomotor ataxia. The clinical history of the cases presented certain analogies to those of the early paralysis in locomotor ataxia, and it seems probable that the paralysis may be due to local nerve lesions. We should then admit that in that disease not only do primary changes occur in the cord itself, but changes in the nerves also, and changes in the cord secondary to these. It may turn out that the benefit resulting from nerve stretching in this disease may be due to its removing certain changes in the nerves themselves. Nerve stretching is the greatest advance yet made in the treatment of this disease.

Dr. JOHN W. OGLE, London: I understand that Dr. Langenbuch considers that in some cases of locomotor ataxy the changes in the spinal cord are only secondary to changes in the peripheric nerves. I should like to know if the good, and relief to pain, found to follow stretching of the nerve in a limb, in cases of ataxy, is confined or not to those cases in which the affection was of peripheric origin; and whether or not nerve stretching is useful where the disease is primarily in the cord?

Prof. MORGAN, Manchester: In a case of idiopathic lateral sclerosis at present under my care in the Manchester Infirmary, accompanied by ankle-clonus and extreme pain in both the lower extremities, the left sciatic nerve was stretched under chloroform by Mr. Southam. Immediately after the stretching, the ankle-clonus on the side operated on ceased, and the pain also in both legs entirely disappeared. In the course of a fortnight, however, the ankle-clonus, to some extent, returned, but there has been no return of the pain, which has quite ceased.

Dr. BROWN-SÉQUARD, Paris: I wish to mention the fact that a section of a lateral half of the spinal cord having produced the ordinary effects of causing hyperæsthesia on one side, and anæsthesia on the other, it is found that the stretching the sciatic nerve on the side where anæsthesia exists changes rapidly the state of things; hyperæsthesia coming where there was anæsthesia, and a diminution of feeling where there was hyperæsthesia.

Prof. EULENBURG, Greifswald: The great interest everywhere taken in the recent operation of nerve stretching, and the somewhat exaggerated hopes raised in connection with the operative treatment of locomotor ataxy, make it our duty to bring our personal experience openly before this assembly. My own experience is not so great as Dr. Langenbuch's, but confined to cases of a severer character; in milder cases I could not make up my mind to operate. My surprise is therefore so much the greater to find in some of them instantaneous success, especially as to diminution of the lancinating pains, improvement not only of cutaneous sensibility, but also of muscular sense, the sensation of posture, passive movements, &c. In whatever manner this may have been effected I should consider it essentially due to centripetal impulses—*i.e.*, not to direct mechanical

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traction transmitted to the spinal cord, but rather through a dynamic (*sic veniat verbo*) agency of a nature as yet unknown, of a peripheral traumatic influence on the corresponding smaller or larger territories of circulation and nutrition in the spinal cord. In that way at least I can understand that the operation on the sciatic nerve of one side can have a favourable influence on that of the opposite side, or on the anterior crural of the same side, or on the nerves of the genito-urinary organs and those of the arm. We have analogous instances after other operations and injuries of nerves. I need only mention the known fact that neuralgia of one branch of the fifth nerve disappears after divisions of the others; and, *vice versâ*, neuralgia of the trigeminus arises after gunshot injuries of the nerve-trunks in the arm. Again, the experimental history of the so-called ascending migrating neuritis and consecutive myelitis may be conveniently mentioned here. On the other hand, of course, in nerve stretching a more or less considerable temporary influence on the peripheral conductivity takes place in the stretched nerve. Apart from the fact that in some nerves it may go as far as complete paralysis (as I have shown on the facial), it extends in the large trunks with mixed nerves, in the extremities principally centripetal, to the afferent nerve-fibres. In the experiments on dogs made by Prof. Voigt and myself, it was observed that after stretching of the anterior crural the tendon reflex (knee-phenomenon) completely disappeared for one or two days, which could only be referred to a deficiency in the centripetal conduction, because the centrifugal (motor) conduction above and below the point of injury showed no perceptible diminution. The irritability tested by almost minimal faradic currents, gave the same results before and after the operation. In some cases of nerve stretching a kind of paralytic tremor in the operated extremity was observed, similar to that described by Schiff after division of the hypoglossal in the tongue. It is most desirable that further investigations, which will certainly follow, should advance this question clinically and experimentally, and inquire especially into the apparently different behaviour of some nerves towards stretching, and also into the differences in part prevailing between human and animal experiments.

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Dr. Langenbuch, Berlin: Professor Erb yesterday warned us against too sanguine expectations concerning the effects to be obtained by "nerve stretching." On this point I entirely agree with him, for it is well known how greatly new methods of treatment may suffer in consequence of too enthusiastic praise; hence, both in my communication of yesterday and in my most recent article in the *Berliner klinische Wochenschrift*, I have been careful to express myself with due caution and reserve. In connection with the recent work of Charcot, Pierret, Leyden, Westphal, and others, I feel it my duty to mention the obligation we are under to my countryman Flechsig, and to point out how much light has been thrown upon the obscure field of diseases of the spinal cord by this learned and conscientious investigator. In his famous work on Diseases of the Spinal Cord, Professor Erb acknowledges that the experiments of Flechsig have led to very important and apparently definite results. The following passage is of special interest to us (p. 39):—"Die Grundbündel der Hinterstränge—Keilstränge—nehmen in der Höhe der Anschwellungen deutlich zu; sie sind zum grossen Theile directe Fortsetzungen der eintretenden hinteren Wurzelbündel, doch sind gewiss auch noch zahlreiche lange Bahnen darunter." The "lange Bahnen" here spoken of deserve our attention, as they may perhaps prove to be the ways by which the effects of nerve stretching are conducted through the spinal cord to the brain. Any modification of the above views of Flechsig would (as Professor Erb stated yesterday) be a great loss for a proper clearing up of the problematical origin of locomotor ataxy, as well as for the explanation of the effects of nerve stretching. Professor Erb's other objections are less directly connected with

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the subject; and it would require more than the time allowed to me to answer them here.

On Perforating Ulcer of the Foot as connected with Progressive Locomotor Ataxy.

Professor BENJAMIN BALL, and M. G. THIBIERGE, Paris.

The trophical changes which so frequently occur in connection with morbid alterations of the nervous centres have long been the subject of serious investigation; and many disorders formerly referred to various other causes are now generally recognized as being of nervous origin.

Such has, in particular, been the case with perforating ulcer of the foot. This local trouble, formerly ascribed to mechanical causes or to injury of the blood-vessels—nay, sometimes even to a syphilitic taint—is now almost universally considered as a trophical disease of nervous origin. It may, however, be questioned whether perforating ulcer is constantly due to the same causes; and it seems likely that there exist several varieties of the disease, which may be referred to different sources.

The labours of Duplay and Morat have proved, at all events, that degeneration of the peripheral nerves is unquestionably, in some patients, the *fons et origo* of this singular affection: and, in this respect, the numerous cases in which it appears to follow section or compression of the sciatic nerves are fraught with a deep significance. But, on the other hand, the same authors have met with perforating ulcers in diseases of the nervous centres, and in progressive locomotor ataxy; cases pointing to the same conclusion have been gathered since then, and we have ourselves been fortunate enough to collect a few.

The light of morbid anatomy has not yet, however, been thrown upon this subject. None of such cases have hitherto terminated fatally; and the state of the peripheral nerves remains, therefore, a mystery. Still, the alterations of nerves in locomotor ataxy have attracted the attention of various observers; some have found no change whatever beyond the spinal roots; others, and more especially Professor Leyden, have found degenerations of some few nervous fibres in the sciatic nerve; and, latterly, Professor Pierret has discovered in the parts which suffer from anæsthesia, distinct alterations of the terminal expansions of sensory nerves; but, after all, the subject still remains obscure, and demands further investigation.

Setting aside the consideration of individual cases, many an argument might be adduced to prove the connection between perforating ulcer of the foot and disease of the spinal cord.

1.—Surgeons have noticed the frequent and obstinate recurrence of the ulcer after its cure, as soon as the patient begins to walk again; an evident intimation that mere mechanical pressure is not the only cause of the disease, and that some general or central predisposition must here be called to account.

2.—It is well known how frequently perforating ulcer is developed symmetrically upon both feet; and, when upon the second foot no positive ulceration exists, a hard callosity is not unfrequently met with, which in process of time may lead to deeper injury. Now, symmetry is undoubtedly one of the leading features of all trophical changes connected with disease of the nervous centres.

3.—Shooting pains in the lower limbs exist in a great many of these patients, and were mentioned for the first time by MM. Duplay and Morat. The frequency of these pains and their peculiar nature point still more closely to the connection between spinal disease and the trophical change.

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4.—Ocular troubles, such as strabismus, have not unfrequently been noticed in cases of perforating ulcer; and, without laying too great stress upon this distant analogy, we still consider that it deserves notice.

We will now proceed briefly to record the cases in which a direct connection has been traced between locomotor ataxy and perforating ulcer; but, in order to avoid confusion, the facts brought under our notice must be divided into two distinct classes.

I.—In the first, the symptoms of tabes are still doubtful, and the outline of the disease is still hazy and ill-defined, when the trophical change first appears; it may then be said that the foot-sore is one of the earlier or premonitory symptoms of the spinal disease.

II.—In the second series, the perforating ulcer is found in patients who exhibit the well-known phenomena of tabes in its more advanced form, and it may then be considered as one of the later or terminal symptoms of locomotor ataxy.

Now, the patient who first attracted our attention to this subject emphatically belongs to the first, or precocious class of cases.

Case I.—A man, aged fifty-two, was affected, four years ago, with perforating ulcers on the phalangeal joint of the right big toe. He was taken into the wards of Dr. Tillany, and left the hospital perfectly cured; but, three weeks later, he began to experience most exquisite suffering in the right foot. In August, 1880, the shooting pains occupied the whole of the right lower limb, from one end to the other. A few weeks later he was struck with partial paralysis, or paresis of both legs and of the right arm. At the present moment the symptoms of tabes are superabundantly evident in this case; shooting pains, motor inco-ordination, impossibility of standing with his eyes closed, circular pains and constriction round the waist, and, last but not least, total abolition of the patellar tendon-reflex.

Here, then, we find a subject in whom the perforating foot-sore outstripped, if we may use the expression, all the other symptoms of tabes, the earliest of which—namely, shooting pains—only made their appearance three weeks after the ulcers had been successfully cured by surgical treatment.

Case II.—Our second patient, who is still an inmate of our wards at the Laennec Hospital, is a man of forty. It is four years ago since he noticed an induration or callosity on the outer side of the left big toe. An ulcer soon developed on the spot, and most obstinately refused to heal. Shortly afterwards the same process was repeated symmetrically on the corresponding joint on the right side; and the sores still existed at the time when he entered our wards (1881). For more than ten years this patient has suffered from what he calls electric pains in the ankles and knee-joints. Sensation is obtuse in a limited zone all round the foot-sore. Patellar tendon-reflex is completely abolished on both sides. Some slight pains are felt in the lumbar region. The patient's gait is tottering and unsteady, and when he shuts his eyes he cannot keep his balance. Speech is slow and somewhat difficult, and has been so for the last two years. The patient has also since that time been affected with deafness in both sides. The memory is weak, and the intellectual faculties are visibly impaired. In short, this patient seems to labour under what has been more particularly described as the bulbar form of locomotor ataxy.

Our excellent friend, Dr. Duguet, physician to the Hôpital St. Antoine, has favoured us with the two following cases:—

Case III.—A female, aged thirty-eight (the only person of that sex in our present record), entered the wards with a perforating ulcer on the inner surface of the left metatarso-phalangeal joint of the big toe, which had recently developed under a callosity, and was surrounded by an extensive zone of anæsthesia. Ten days after admission she was seized

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with lancinating pains in the diseased foot. The area of insensibility gradually extended, violent pains appeared in the lower limbs, and in the lower part of the chest. Patellar tendon-reflex was completely abolished. Shortly afterwards, tremulation in the upper limbs, contraction of the pupil, gastric symptoms, and weakness in the lower limbs, made their appearance; and so rapid was the progress of the disease that she is now unable to stand with her eyes shut. The peculiar interest of this case centres in the insidious progress of the disease, which, as first exhibited, showed but the symptoms of perforating ulcer, while, at the present moment, the signs of advanced tabes are self-evident.

Case IV. (Duguet).—A man aged forty-four, after suffering nearly six months from pain in left side of the head and face, was taken with a perforating foot-sore on the left big toe, and shortly afterwards with a second ulceration on the dorsal surface of the fourth toe on the right side. At the same time, tingling sensations and cramp occurred in the legs and feet, and, shortly after this, shooting pains appeared in both lower limbs. Four months later, the patient was taken into Dr. Duguet's wards, and the symptoms of tabes were easily ascertained. The patient exhibits an evident diminution of tactile sensibility, and the prick of a pin was only felt a long time afterwards. The patellar tendon reflex was altogether abolished, and the patient could not stand upright with his eyes shut. He also suffered from intense gastric symptoms. This patient, after leaving the hospital, committed suicide, but unfortunately no post-mortem took place, and a most interesting case was thus lost to medical science.

Case V.—The last case we have to report is a doubtful one; a patient entered the wards of Dr. Péan, with a perforating ulcer of the foot, on the plantar surface of the big toe, which had destroyed the joint, and necessitated resection of the bone. In this case the patellar tendon-reflex was completely abolished on both sides, and erratic pains were felt by the patient in both lower limbs.

We will now briefly proceed to enumerate the cases which I have been able to collect from various authors, and in which the foot-sore is one of the later symptoms of tabes, in an already advanced stage.

Case I.—The first case has been kindly communicated by our excellent friend, Dr. Lancereaux. A man, forty-four years of age, after suffering during twelve months from shooting pains in the lower limbs, and during the last six months from motor incoordination, was affected with perforating ulcer on the plantar surface of the left big toe.

Cases II., III., IV., V., VI., and VII., have been published by various authors, whose names are given below*:—

The total number of cases which we have been able to collect, amounts therefore to twelve; and this number might probably be increased by an excursion beyond the pale of French medical literature, to which we have confined ourselves.

In all these cases the patient had been affected with the symptoms of locomotor ataxy for some considerable time when the perforating ulcer first appeared; and in each and all of these patients shooting pains were one of the leading features of the disease.

The cause which we have endeavoured to plead, exhibits, it must be confessed, a most unfortunate flaw—namely, the total absence of post-mortem examinations. It seems, however, extremely probable that, within no considerable space of time, opportunities will arise for investigating the morbid anatomy of these trophical changes. It will then have

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Case II.—Daploy and Morat, *Arch. Générales de Médecine*, 6 Series, T. xxi. 1873. Case III.—Morat, *Arch. Générales de Médecine*, 1876, p. 346. Case IV.—Pitoy, Thèses de Paris. 1877. (1st case.) Case V.—Botreville, Thèses de Paris. 1878, p. 58. Case VI.—Mathieu, Thèses de Montpellier. 1878. Case VII.—Flavot, *Archives de Physiologie*. 1881.

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to be decided whether the corresponding alterations lie in the spinal cord itself, in the nerve-roots, or in the peripheral ramifications; and it may then be possible to adopt a more rational mode of treatment of the disease, by acting upon the real culprit—namely, the spinal cord. In the present state of things, the following conclusions may, we believe, be safely adopted:—

1.—Perforating ulcer of the foot in locomotor ataxy is a trophical disturbance, due not to local causes but to a central or spinal lesion.

2.—Perforating ulcer of the foot is not unfrequently an early symptom of the disorder, and may appear long before the locomotor troubles.

3.—The local disease is manifestly connected, as has been evinced in a large number of cases, with certain definite symptoms of tabes dorsalis, and more especially with intense shooting pains, with the absence of tendon-reflex, and with gastric symptoms, or the peculiar dyspeptic troubles of locomotor ataxy.

4.—In a practical sense, it will be in future highly important to examine the tendon-reflex in all cases of perforating ulcer of the foot.

5.—Females appear to be infinitely less subject than males to this particular consequence of locomotor ataxy.

6.—Although the symptoms of the spinal disease still pursue their downward course, the local disease may apparently be treated; this, however, must be due in some slight measure to the rest which the patient is forcibly compelled to take.

Before concluding this paper, I must distinctly beg to state that I do not consider spinal disease as the only cause of perforating ulcer of the foot. There exist, no doubt, a large number of varieties, as to the origin of this local complaint; and we have only laboured to show that it is unquestionably connected, in a tolerably large number of cases, with the earlier or later periods of tabes dorsalis.

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*On Percussion of the Skull in the Diagnosis of Diseases of the Brain.**

Dr. Alexander Robertson, Glasgow.

Disease in a limited portion of the surface of the brain and its membranes in some cases gives rise to pain at the affected part, of which the patient may be fully conscious and be able to point to its exact locality. In other cases the pain which it occasions is more diffuse in its character and variable in its position, and is no guide to the seat of morbid action. But, further, the disease in many instances reveals itself mainly by symptoms of a different kind, such as convulsive movements in the form now known as

* At the meeting of the International Congress, I learned that in 1874, and therefore three years before me, Dr. Ferrier had referred to the value of percussion of the skull, and had mentioned that pain might be elicited by that means, though not previously complained of by the patient. His observation is published in a paper, entitled Pathological Illustrations of Brain Functions, in the *West Riding Asylum Reports* for 1874, page 54. It only occupies five lines of the article, and, I need scarcely say, was quite unknown to me. I now fully acknowledge his priority, and regret that I should have overlooked his remark on the subject. The fact that two physicians, quite independently of each other, should have made the same observation, adds much to its value.

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Jacksonian epilepsy, or it may be by alterations in the mental state; and pain, if present, is so transient, possibly occurring only about the period of the convulsive fit, that the patient may say that he does not suffer from headache at all. It is in the last two classes that percussion of the skull proves especially serviceable, often indicating with precision the spot where the disease exists. But in the first group, also, it is frequently of no small value in corroborating the patient's impressions, as, with rare exceptions, the uneasy or painful feeling at the affected part is momentarily much increased by percussion over it.

When it is alleged that the existence of disease in a part of the brain's surface can be manifested by percussion of the cranial wall by the finger, doubt may be felt if the slight force applied is really transmitted, and is not rather diffused along the bone. This point then requires consideration. Apart altogether from clinical observation, Duret's researches on cerebral traumatism apparently furnish a sufficient answer to the objection. For he has shown by careful and laborious experiment that at the point of sudden impact a *cône de dépression* is formed which passes through the interior, in the line of the thrust, to the base of the skull. This, of course, applies to severe blows, but the slight strokes of percussion will act in a similar way, though the impression may not extend deeper than the cerebral cortex.

A somewhat analogous instance of the irritation of a morbid part into conscious sensibility by mediate percussion is sometimes supplied by disease of the lungs. For when softening tubercle or a cavity is pretty near the surface of a lung, the patient will often wince and complain of pain when percussion is practised over the affected part, whether the finger, used as a pleximeter, rest on a rib or in one of the intercostal spaces.

However, clinical experience furnishes evidence which seems conclusive. For a number of cases are now on record in which distinct localized pain in what is now generally regarded as the motor region of the convolutions, previously not felt, or at least forgotten by, the patients, was brought out by percussion, and the form of convulsions or palsy from which they suffered also pointed very clearly to that portion of the cerebral cortex as the seat of disease. In such cases the two kinds of symptoms lend each other mutual support in the localization of the morbid process. On the one hand, those indications of disturbed or destroyed function, though they may justly be regarded as very significant of the part of the brain involved, can scarcely yet, in the present state of our knowledge, be held as perfectly reliable in their import; at least the writer, in the patients under his care, felt his diagnosis of disease in the motor convolutions, founded on the character of the convulsive fits, to be greatly strengthened when pain was developed in that region, *and only there*, on percussion of the skull throughout its whole extent. On the other hand, the information derived from the pain thus developed was greatly increased by the co-existence of one or other of the functional disorders referred to. Again, when, along with epileptic convulsions of too general a nature to have any localizing value, pain was elicited by percussion in a limited area, and at no other part of the head, it was felt that there was strong ground for thinking that this was the centre of morbid action, though it might be distant from the so-called motor convolutions.

In certain cases under consideration, the patients stated that some years previously they had received blows on the back of the head in which pain could be induced; the conclusion to which the pain and other symptoms pointed was thereby corroborated. In some instances also the effects of treatment supported the diagnosis; for, so far as could be determined, the patients were greatly benefited by repeated counter-irritation over the seat of pain.

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But, further, it may be urged as an objection to the alleged development of pain, that the cerebral substance is altogether insensitive, and that the investing membranes are very slightly so. This is doubtless true of the brain in health, and, so far as known, in disease also; as is evinced in hernia cerebri where a patient may have a slice of his convolutions cut off without experiencing any sensation. The membranes likewise when exposed show little evidence of sensibility. But though their sensory endowment is limited, like other fibrous structures, when in a state of disease and at the same time subject to tension, they may give rise to pain which occasionally becomes excruciating.

The pain, it will be observed, is elicited by percussion. Mere rubbing of the part or gentle pressure does not induce it. It is therefore inferred that the disease is not in the bone, unless it be in the inner table of the skull, and if this part be affected it is of great importance that the fact should be known, as morbid action there usually involves the outer and often the inner membrane and brain itself. However, disease of the bone in adults in most cases is syphilitic, and the pain as a rule is so considerable as to stand in no need of artificial development to manifest its existence.

I shall now, in illustration, submit short abstracts of a number of cases that have fallen under my observation:—

*J. W., aged 38.—This patient was subject to convulsive seizures, affecting the right arm and leg, every three or four weeks. The disease was probably of syphilitic origin. He stated in answer to repeated inquiries that he did not suffer from headache, yet when the head was tapped all over with the finger, he without hesitation defined the limits of an area where the tapping was distinctly painful. This was situated on the left side, and included a space of about two inches in diameter, the centre of which was three inches above the highest part of the left ear. Nowhere else was there any unpleasant sensation, and the patient, who was an intelligent man, was quite unaware that there was any exceptional condition in the region mentioned till his attention was directed to it by the percussion. The pain was described to be of a dull heavy character, and was not produced by mere pressure of the finger without tapping. Besides ordinary constitutional remedies, ten cantharides blisters were applied in rapid succession over the painful region, and he seemed to derive very decided benefit from the local treatment. This was in the end of 1877 and beginning of 1878. He was dismissed apparently well after being some months free from seizures. He again came under observation in the latter part of 1880, and although after excesses he had in the interval suffered from two mild attacks, he still considered himself recovered and fit for his occupation as an engineer.

W. H., aged 39.—Patient stated that he has been subject to fits about once a fortnight for some years. For a day or two before they occur he has a heavy, severe pain across the middle of the head, extending to about two inches above each ear, which, when the attacks are imminent, becomes aggravated and seems to pass from the right to the left side. They begin in the left arm, then pass to the left leg, and thereafter become general. There is no pain in the head except near the time of the fits. In a free interval the head was percussed with the finger. This developed very distinct pain in an area about midway between the upper part of right ear and middle line of head. The patient left the hospital before there was time to test the effects of treatment.

J. M., aged 13.—For some months before admission this boy was subject to "night terrors," and on two or three occasions had fits of somnambulism. Before special treat-

* Cases marked with an asterisk were shown at meetings of the Pathological and Clinical Society of Glasgow, and have been reported in different medical journals.

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ment was commenced he had two attacks apparently of an epileptic character, being unconscious for some minutes, but without distinct convulsive movements. He suffered from giddiness, but said positively that he had no headache. However, on tapping the entire head with the finger, distinct pain was elicited in and around the right parietal eminence. A cantharides blister was applied over this part, and he was ordered a mixture containing the bromide and iodide of potassium. A few days afterwards, when the skull was again percussed, he stated that he had now no pain in the part mentioned, nor anywhere else. He continued free from headache and had no more fits during the two months he remained in the hospital, and left apparently well.

*H. B., aged 12.—This boy was struck with a piece of brick on the left side of the head about four inches above the summit of the ear and half an inch in front of the parietal eminence. Within a month he became subject to epileptic "absences," and rarer convulsive seizures, which began by his head turning to the right. For some weeks he had pain, which at first was severe, but after a time much less acute in the injured parts of his skull. When he came under my care the unpleasant feeling had passed away. Percussion, however, brought back the pain in a somewhat aggravated form, but after a few minutes it gradually disappeared. Much apparent benefit was derived from a succession of blisters applied over the painful part in this case also.

* J. R., aged 28.—Patient has been subject for upwards of three years to attacks of Jacksonian epilepsy, affecting chiefly the left arm and hand, at about weekly intervals. He has also suffered often from headache during that time, but the pain was generally diffused throughout the head during the attack, and he cannot say that one part was sorer than another. Pain was developed by percussion of the head which was localized in a region not very sharply defined above the right ear and a little in front of it. Though this was slightly anterior to the line of the motor co nvulutions, it was still sufficiently near it to support distinctly the conclusion as to the site of lesion derived from the character of the convulsive movements.

M. M., aged 44.—Patient's head was hurt by a fall when he was ten years old, and ever since he has been epileptic. The fits occur every three or four weeks. They begin as an aura on the back of the left hand, which creeps up to the shoulder, then he loses consciousness and convulsions become general. Before and after the seizures he usually suffers from severe and widely diffused headache, but in the intervals he is free from unpleasant feeling of any kind. On percussing the head pain is felt nowhere except in the middle of the forehead, but even slight tapping on that part induces intense aching. "It nearly blinds me in both eyes," he said. Careful examination of that part of the head seems to show a slight depression of the bone at the painful part.

In practising percussion of the skull it is well to tap the whole head with the finger before asking if pain is induced at any part in particular; and if some part be distinctly painful, the observation should be verified by a second percussion. Care should be taken to make the taps of the finger as nearly as possible of equal force, except in the temporal region where they should fall more lightly than elsewhere. It is well to percuss one's own head previously to ascertain the character of the tap which can be borne without discomfort. It is necessary to avoid either by remark or otherwise directing the patient's attention to any particular part of the head; this precaution is very important in the case of nervous hysterical people.

It is not claimed that this means of diagnosis is of very wide application. It probably will not be of service if the morbid action be diffused, as in ordinary cases of insanity. It is clearly of value where the disease is limited in extent and particularly if gross products exist, such as inflammatory lymph, producing local tension, and in tumours of the surface or in the membranes. In injuries of the head it may occasionally

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assist the diagnosis. Thus, in a somewhat doubtful case of fracture of the skull the writer has seen it of use in helping to mark out the line of the fracture. In disease of the inner table of the skull, where the pain of the head is widespread and felt without percussion, it may guide to the exact seat of lesion, as the developed pain is greatest at that point.

Wherever, therefore, there is the least ground, judging from the general symptoms, for suspecting that disease may exist superficially within the skull, percussion of the head should not be omitted. Failing any result, no harm will have been produced; when successful, it singles out the part to which remedial applications may be made, with the well-founded anticipation that not seldom the progress of the disease may thereby be checked.

On Addison's Disease.

Dr. Edward Headlam Greenhow, London.

Cases of diseased supra-renal capsules were related by various authors previous to Dr. Addison, but, until he did so, no one had connected any particular symptoms with disease of these organs. These symptoms are, nevertheless, so striking and peculiar that they could not fail to have been observed, though their nature was not understood. Accordingly, several observers had described cases of Addison's disease, more or less accurately, long before the publication of Dr. Addison's discovery of its connection with a morbid lesion of the supra-renal capsules. Thus, Risel relates an almost typical case which was recorded by Dr. Schotte in the "Halle Hospital Reports" for 1823,* and Dr. Bright described a case in 1831,† which is quoted as a true case of this disease by Dr. Addison in his monograph.‡

Dr. Addison appears to have communicated his views on the constitutional and local effects of disease of the supra-renal capsules to the South London Medical Society several years before the publication of his more complete treatise in 1855, but this earlier communication seems to have attracted little attention. Since the latter date, very numerous cases of the disease have been observed and recorded both in England and abroad, and although, as has so often been the case with other scientific discoveries, much scepticism was for many years entertained as to the relation subsisting between the constitutional symptoms and the local lesion in the supra-renal capsules, no one now, I believe, doubts the reality of this relation. My purpose in this paper being only to explain the present state of knowledge on the subject, I shall now pass at once to the consideration of the disease itself, and will commence by relating a hitherto unpublished case, as being the most convenient mode of bringing the phenomena of this disease prominently before my hearers.

M. W., aged eighteen years, needlewoman, was admitted into the Middlesex Hospital under my care, December 12, 1876. She stated that, with the exception that the catamenia were not quite regular, her health had always been excellent until about five weeks previous when she had a severe attack of bilious vomiting, and observed that her face, arms, and

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* *Deutsche Archiv f. klin. Med.*, vol. vii. p. 46.

† "Reports of Medical Cases," vol. ii. p. 247.

‡ "On the Constitutional and Local Effects of the Disease of the Supra-renal Capsules," by Thomas Addison, M.D., Physician to Guy's Hospital.

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hands had acquired a dark yellow colour. The vomiting continued to recur after meals for several days. She nevertheless gradually recovered from this slight illness and was in her usual health on the evening of December 9. On rising next morning she experienced pains in the knees and legs and at the lower part of the spine was very chilly during the day, and had a severe rigor in the evening. Next day the pain in the back had become more severe, and she again suffered from sickness and was unable to take her food.

State on Admission.—Pulse 104, small and compressible; temperature 99·0°; extremities cold; tongue clean; lungs and heart normal. Urine sp. gr. 1027, normal. Tenderness on pressure at the epigastrium and also over the sacrum. Bowels confined. There is slight duskiness of the skin, especially over the face and arms; on the face are several mole-like specks; there is also very slight darkness of the mammary areolæ, and a band of dusky pigmentation round the waist, corresponding to the situation of the strings of the dress. Conjunctivæ markedly white and clear. Ordered simple effervescing medicine, milk diet, and two ounces of brandy daily.

There was no vomiting from the day of admission until December 14, from which date it recurred frequently until the 18th. The pulse during this period ranged from 104 to 124, and the temperature from 97·2° to 99·9°. On December 18, when the vomiting had greatly subsided, tincture of perchloride of iron, combined with glycerine, was prescribed; and, the appetite being very capricious, the diet was ordered to be varied from day to day to meet the wishes of the patient.

Vomiting still recurred from time to time, but not very frequently, and on January 7 she was so comfortable that she was permitted to sit up in the ward. This exertion, however, brought on vertigo, sickness, and return of pain and tenderness in the epigastrium and over the sacrum. Pulse 130, very small and compressible. Temperature rather below the normal standard. Patient very feeble, the mere exertion of being examined having caused hiccup and vomiting. Considerable tenderness was now discovered, on pressure, in the left hypochondrium and the complexion was observed to have become darker since admission.

A microscopical examination of the blood made this day revealed nothing abnormal; it was perhaps a little poor in red corpuscles, but there was no excess of white ones.

From this time she appeared for a few days to be improving, and on January 16 the general discoloration seemed less marked, but the mammary areolæ had become darker. Vomiting and hiccup continued to occur almost daily, and sometimes several times a day, usually after some exertion, and she complained much of pain in the epigastrium and both hypochondria.

January 19.—Pulse 120, almost imperceptible; temperature 98·6°. Complains of severe pain in the region of the sacrum.

Next day she was found lying in a torpid state; she disliked being disturbed and answered questions with evident reluctance; refused food and retched on the slightest movement, but did not actually vomit. Complained of severe pains in the loins. It was now observed that small patches of discoloration resembling moles had become developed upon the lower extremities.

Ordered nutritive enemata, consisting of beef-tea, egg, and brandy, every four hours.

The enemata were retained, and on January 22 she appeared brighter, but still retched on being moved. A few days later she seemed still better and was able to eat some solid food. On January 28, the blood was again examined microscopically, and found to be normal.

February 4.—Has varied much during the last few days. Pulse 84. Distinct and of better volume. Discoloration of skin rather less. General aspect improved. Tongue

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clean. Still has occasional retching and vomiting, but is nevertheless able to take food and a small quantity of port wine. The nutritive enemata are still continued daily, but less frequently.

February 13.—Pulse 120, thready and feeble; temperature 99·0°; respiration 24. During the last week the pulse has ranged from 84 to 112; the respirations from 16 to 20; the temperature has for the most part been rather subnormal. Has been free from sickness, and able to take food. Her aspect this morning is rather more depressed; colour darker; has return of vomiting, retching, and hiccup. The mammary areolæ are darker; breath very offensive.

February 17.—Pulse 124; Temperature 98·6°; vomiting; very apathetic. Complains of pain in the sacrum and aching in the legs. Refuses food.

February 19.—Pulse 140, almost imperceptible; temperature 99·6°; respirations 12. Is very drowsy; urine sp. gr. 1015, normal. Tenderness over sacrum. Vomiting and retching continue. An offensive odour about patient.

February 22.—Pulse 128; temperature 97·8°; respirations 16. Is very prostrate. Answers questions slowly and reluctantly. Frequent sickness and hiccup. Voice faint. Colour of skin darker; mole-like spots on face appear larger and darker, being almost black.

March 1.—Pulse almost imperceptible, about 140; temperature 99·6°; respirations 16; free from vomiting and retching, but has frequent and violent hiccup.

March 3.—Pulse quite imperceptible; temperature 98·5°; lies in a semi-torpid state with the eyes half-closed. Sordes on teeth. Answers when addressed. Complains of general soreness of the surface of body.

Died at 8 P.M. of March 4th.

Post-mortem Examination.—Slight general duskiness of surface; skin of face, axillæ, abdomen, groins, and knees, of a pale dusky brown tint. Nipples ill-developed. Areolæ not very markedly pigmented. Besides the more diffuse pigmentation of the surface, minute dark brown specks of pigment were scattered sparsely over the face and limbs. Subcutaneous fat of a rich yellow colour and very abundant, forming a layer varying from three-quarters of an inch to two inches in thickness, in the walls of the thorax and abdomen.

Thorax.—Heart small, valves healthy, muscular tissue pale coloured. Auricles filled with tolerably firm decolorized clot. Both lungs slightly adherent by a few bands at the apex. Right lung healthy; scattered throughout the upper and contiguous part of the lower lobe of the left lung were some caseous nodules, varying in size from a pea to a walnut. They were wedge-shaped, and situated immediately beneath the pleura. Some of them had softened down into small cavities containing puriform fluid, each cavity having a distinct limiting membrane.

Abdomen.—Numerous bands of adhesion existed between the liver and diaphragm, spleen and stomach, and also between the omentum and parietal peritoneum. The gastric mucous surface presented numerous points of injection and ecchymosis; the intestinal mucous membrane was natural, with the exception of marked vascularity and slight prominence of the solitary glands.

The supra-renal capsules were firmly adherent to the surrounding tissues, much fat being intermingled with the cellular tissue, so that it was impossible to dissect out the nerves with any great exactness. Both capsules were much enlarged, hard, and nodulated. The right capsule was of an irregular quadrilateral shape, and measured two inches vertically by one inch and a half wide. The vena cava was firmly adherent to its anterior surface, concealing the lower half of the organ. The left capsule was triangular in shape; it measured two and a half inches vertically by one inch in width at its

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base. On section it presented numerous opaque, firm yellowish nodules, averaging a split pea in size, besides two much larger masses of the same character, situated in the lower part of the organ. These nodules were all separated from one another by bands of vascular fibrous tissue, the white glistening appearance of which contrasted with the opaque appearance of the nodules. The sympathetic nerves and plexuses, especially the solar plexus, were inextricably mixed up with the connective tissue which it was found impossible to separate. Numerous large nerves passed from each capsule to the semilunar ganglion on either side. These ganglia were much indurated.

This case presented most of the characteristic local signs and constitutional symptoms of Addison's disease. The dusky hue of the skin had been sufficiently obvious to have attracted the notice of the patient and her friends before she became an inmate of the hospital. It was chiefly developed in the usual situations—namely, the face, hands, axillæ, abdomen, and groins—but was by no means so dark as in many other cases which have come under my observation. There was also no pigmentation of the mucous membrane of the mouth, neither was there any deeper discoloration around the only cicatrix that was discovered. This was situated upon the outer aspect of the right thigh, was superficial, not much depressed, and was evidently of very old standing. The absence of pigmentation around this cicatrix, indeed, forms the most remarkable deviation from the usual course of the disease, for deeper discoloration of skin in the site of former injuries and around old cicatrices is almost invariable in Addison's disease.* Two circumstances may have contributed to this exception in the present case—namely, first, the very old standing of the cicatrix and its comparatively slight nature; and secondly, the very rapid course of the illness, only four months having intervened between its beginning and the fatal termination. In every other respect the discoloration, though slight, was quite characteristic.

On the other hand, the constitutional symptoms and the morbid appearances found at the post-mortem examination were those which have been noted in every well marked case of the disease. There was the progressive asthenia, with small, feeble pulse; languid exhausted aspect, indisposition for exertion, increasing prostration and apparently semi-comatose state; anorexia, gastric irritability, vomiting, retching, and hiccup, which are usually prominent symptoms of the disease. There were likewise occasional vertigo, on being raised into the upright position, and great exhaustion and depression when examined even in bed; pains in the loins, and tenderness on pressure in the epigastrium and hypochondria, especially during the recrudescences of the illness, and becoming much less obvious when the more urgent constitutional symptoms subsided. The temperature was for the most part subnormal, though sometimes a little elevated; the respirations were generally shallow and somewhat below the frequency of health. The urine was normal, but, after the first observation, of rather low specific gravity. An offensive odour was observed about the bed and person of the patient towards the close of life, a fact which has been noticed in other cases, both by myself and by other observers. The excessive restlessness during the last days of life, which was present in this patient, is also a not uncommon attendant of the closing scene in these cases.

The course of the illness presented the irregular remissions and exacerbations of symptoms which have been so frequently observed.† The course of Addison's disease is,

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* For cases illustrating this fact, see "On Addison's Disease: being the Croonian Lectures for 1875," by E. H. Greenhow, M.D., pp. 104 and 117; also *Trans. of Pathological Society*, vol. xv. p. 228, and vol. xx. p. 404.

† See Croonian Lectures, pp. 17, 117, 139; also *Trans. of Pathological Society*, vol. xix. p. 404; and vol. xxiv. p. 234.

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indeed, on the whole progressive, but it is, nevertheless, liable to such alternations as existed in this patient. During the remissions the more prominent symptoms abate, or may even from time to time almost entirely subside; but, such remissions are invariably followed, sooner or later, by a return of the symptoms of depression and asthenia. Such alternations may occur many times in the course of the illness; there may even be periods of comparative health, but, however slow and chronic in its course, the tendency of Addison's disease is always downwards; each exacerbation leaves the unhappy patient in a worse state than before, until at length he succumbs in one of these paroxysms. These exacerbations sometimes supervene without any obvious cause, but more commonly seem to arise as a consequence of some depressing circumstance.* Thus a fatiguing walk or a dose of purgative medicine has not unfrequently appeared to be the exciting cause of fatal exacerbations. In the patient whose case I have related the mere fatigue of being submitted to a careful examination in bed on several occasions brought on paroxysms of retching, sickness, hiccup, and intense temporary depression.

The constitutional symptoms of Addison's disease may be summed up as those of extreme nervous depression, with greatly enfeebled action of the heart, small thready pulse, faintness, shallow feeble respiration, more particularly during the paroxysmal exacerbations just described; hiccup is very common, and anorexia and irritability of stomach, with vomiting and retching, are almost invariably present during the course of the illness, and have never been absent in the last stage in any of the cases I have seen. There is no emaciation, unless the disease be associated with some wasting disease, such as phthisis, and there is almost invariably a large accumulation of fat in the integuments, and sometimes around the internal organs. Notwithstanding the breathlessness and inability for exertion, there is no real anæmia, the blood being often rich in red corpuscles; and in none of the cases in which I have examined it has there been any increase of white ones.

As a rule the temperature is subnormal in Addison's disease, the extremities cold, and the urine scanty, and for the most part of low specific gravity. There is generally constipation. There would thus seem to be decreased tissue change. The mind is often clear to the last, but so great is the prostration in fully developed cases, that the patient towards the end of the scene, often lies in a drowsy, apparently semi-comatose state, from which he can, nevertheless, be roused, and then gives pertinent, though usually slow and apparently reluctant, answers to questions, as if speaking required a great effort. Death commonly takes place by asthenia, and is sometimes sudden. In other cases incoherence or delirium, and more rarely coma, have preceded death; and again, in other cases, the end has been ushered in by subsultus and convulsions.†

Discoloration of skin, when present, is one of the most prominent and striking features of Addison's disease. It is not uniformly distributed over the surface of the body, but is developed chiefly upon the seats naturally most liable to become darker, either under the influence of exposure to the sun and air, or the excitement of certain physiological processes; and also around the seats of any local irritation. The peculiar change of colour in Addison's disease must, therefore, like many other pathological processes, be regarded as only an exaggerated and, therefore, morbid development of a natural physiological process. It commences earliest and becomes deepest on the more

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* See *loc. cit.*, pp. 107–130; also *Trans. of Pathological Society*, vol. xvi. p. 247, and vol. xv. p. 231.

† For cases in illustration see "Croonian Lectures," pp. 103, 108, ,115 118, 121, and 148; also *Trans. of Pathological Society*, vol. x. p. 269; vol. xvi. p. 247; vol. xvii. p. 307; vol. xix. p. 404; vol. xxiv. p. 224; and vol. xx. p. 378.

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exposed parts—the face, neck, and hands, and in the axillæ, the mammary areolæ, groins, genital regions, and abdomen. Sometimes it is only observed in these situations; but more commonly, though most obvious in them, it is also more or less generally diffused over the whole cutaneous surface. The transition from the darker to the paler parts is rarely sudden, but, on the contrary, the one merges into the other by insensible degrees; the single exception to this rule being where any local irritation has existed,* when the margin of the darker portion is often well defined. Thus, marks or bands of pigmentation have frequently been found in situations which have been the seats of irritation caused—as in the case I have narrated—by the pressure of strings or garters, and, still more notably, where a superficial abrasion, such as that caused by a blister, has recently existed. True cicatrices are, however, never pigmented, but there is usually a well-marked areola of discoloration around them, which contrasts vividly with the ivory whiteness of the true cicatricial tissue.

The discoloration varies in hue from a general greenish bronze, or a dark sepia colour, to mere duskiness of skin, and the patient presents very much the aspect of a person belonging to one of the darker races of mankind. Unless where it is intensified by some local irritation, the discoloration is symmetrically arranged: both axillæ, both mammary areolæ, and both groins, being similarly affected. The discoloration, as already stated, is often so marked as to attract immediate attention; but in several of my cases it has only been discovered when sought for to assist in the diagnosis of the case, and in a few instances it is said to have been altogether absent.† The mucous membrane of the lips, tongue and mouth is likewise often the seat of discoloration in Addison's disease. Its margin on the lips and tongue is usually more defined than on the skin, and on the tongue it frequently has an inky hue.

The colouring matter is deposited in the skin in the situation in which it exists naturally in the darker races of men—namely, in the deeper layers of the rete mucosum, the sinuosities of which it very exactly follows; the more superficial layers and the true skin being mostly free from deposit. Nevertheless, in a few cases I have found traces of pigment in the connective tissue of the cutis, at some distance below the rete mucosum. The pigment deposited in the tongue, lips, and mucous membrane of the mouth is deposited in the lower cells immediately overlying the papillæ; the superficial epithelium, like the superficial layers of the epidermis, being quite free from deposit. The seat of the discoloration in the mucous membranes therefore exactly corresponds with that in the skin.

The pathological appearances found in Addison's disease are invariably of the same kind, though they vary somewhat in details according to the stage at which the morbid process has arrived, and, probably, the more or less rapid or slow course of the disease. In uncomplicated cases, the primary lesion is situated in the supra-renal capsules and their immediate neighbourhood. These organs are hard, nodulated or uneven, misshapen, and frequently much increased in volume. More rarely they are normal in size; and, in a few instances, they have been found contracted and smaller than natural. They are usually extensively adherent to neighbouring organs—to the liver, diaphragm, spleen,

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* "Croonian Lectures," pp. 105, 118; also *Trans. of Pathological Society*, vol. xv. p. 228; and vol. xix. p. 404.

† See Bristowe, *Trans. of Pathological Society*, vol. xi. p. 276; Bruce, *Trans. of Pathological Society*, vol. xvii. p. 401; Gull, *Medical Times and Gazette*, 1863, vol. i. p. 83; Murchison, *Trans. of Pathological Society*, vol. xix. p. 406; Owen Rees, "Guy's Hospital Reports," vol. viii. p. 38; *Medical Times and Gazette*, 1866, vol. ii. p. 313; Tuckwell, *Trans. of Pathological Society*, vol. xix. p. 419.

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pancreas, kidneys, stomach, and vena cava, and are surrounded by a dense mass of connective tissue, by which the numerous branches of nerves, which pass into the capsules and the adjoining ganglia and plexuses are intimately matted together.

When the diseased capsules are cut across, the distinction between cortex and medulla is found to be almost, if not entirely, obliterated, and the organ is seen to be composed of two materials of different appearance and character. One of these is a semi-transparent tissue, of firm consistence, and of a grey or greenish-grey colour when recently cut, but rapidly acquiring a reddish hue when exposed to the air. The other material is opaque, of a yellowish or cream colour, and mostly of cheesy consistence. It consists of irregular roundish masses embedded in the translucent substance already described, from which it can frequently be readily enucleated. This mixture of materials of such different appearance gives the cut organ a marbled appearance. The proportion of these substances varies much in different cases, and probably depends upon the stage, and more or less rapid progress of the local changes. In the less advanced cases, some traces of the normal structure of the capsules may occasionally be found in the semi-transparent tissue; but in the greater number of cases every relic of the original structure of the organ has usually disappeared. On close examination, intermediate gradations between the firm translucent tissue and the opaque, friable, cheesy nodules may be seen. In some cases the cheesy substance has become cretaceous, grating under the knife; and in others it is found replaced by a creamy, puriform fluid, which may be so abundant that the whole organ is converted into an abscess-like cavity. In rare cases the capsule is converted into a hard calcareous mass, and is then sometimes found of less than the natural size. These varieties in the condition of the supra-renal capsules in Addison's disease depend, probably, upon the more or less acute or chronic course of the disease, the morbid process being essentially the same in all of them.

On microscopical examination, the translucent substance is seen to be formed of a fibrillated stroma, containing numerous lymphoid corpuscles; the opaque cheesy material to consist of amorphous granular matter, mixed with shrunken cells, nuclei, and oil, in varying proportions. It would thus appear that the primary morbid process in the supra-renal capsules consists of an inflammatory exudation of low type, which encroaches upon and destroys the natural tissue of the organs, and itself ultimately undergoes caseous degeneration; this is accompanied by great proliferation of the connective tissue around the capsules, producing the adhesions to neighbouring organs, and the appearances in connection with the nerves, ganglia and plexuses already described.

Several morbid conditions are so frequently found to co-exist with Addison's disease that they must be looked upon as bearing some definite relation to it. Not being found in every instance, they cannot be considered as essential, but I feel confident that I do not err in ranking them as secondary consequences of the disease. Perhaps the intimate matting together of the nerves, ganglia, and plexuses, might be also regarded in this light; but they have been so constantly present that I consider them as forming an important, and, indeed, as I shall hereafter explain, essential element of the disease. The conditions to which I now more particularly refer are enlargement of the lymphatic glands in the vicinity of the capsules, due, undoubtedly, to the extension to them of the inflammatory process; swelling of the agminated and solitary glands of the intestines; and a mammillated appearance of the mucous membrane of the stomach. This latter appearance arises from an overgrowth of lymphoid tissue around and between the gastric tubules. It did not exist in the case which I have related; but it is so frequent an attendant of Addison's disease that it falls into the category now under consideration. Enlargement of spleen has also been observed sufficiently often to render it probable that it also forms an occasional secondary consequence of the disease.

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In a large number of cases of Addison's disease, no other morbid lesion has been found on post-mortem examination besides those I have described. In others, pathological conditions have been observed, which it is impossible not to regard as having been either directly causative of the disease in the supra-renal capsules, or as indicative of a constitutional proclivity to analogous morbid processes in other organs. To the former class I shall revert when I speak of the exciting causes of the disease; but I may here mention that caries of the vertebræ and abscesses in the vicinity of the capsules appear to be often the starting-point from which Addison's disease takes its origin. To the other class must be referred the very frequent association of Addison's disease with other local morbid conditions, which may be broadly spoken of as connected with a strumous, or tubercular constitution, or as a vulnerable constitution, prone to the development of chronic inflammatory processes of low type.

The course of Addison's disease is always chronic. It may last for several years: there may even be long intervals of comparative health after it has become manifest, but its tendency is invariably towards a fatal termination. In a few instances it has proved rapidly fatal, almost coincidently with its first distinct manifestation; but the revelations of the post-mortem examination in such cases have invariably shown that the local lesion must have been long in progress.* The constitutional symptoms and local signs do not invariably appear to run together; sometimes the discoloration has been absent, or nearly so, when death has speedily followed the development of the constitutional symptoms; in other cases, the discoloration of skin has been very obvious for a considerable time before the constitutional symptoms have appeared.

The diagnosis of Addison's disease is not more difficult than that of many other chronic diseases; indeed, when both the constitutional symptoms and local signs are well-marked it is very easy. In uncomplicated cases it is not very difficult to diagnose from the well-marked constitutional symptoms alone, even though the discoloration of skin be absent, or so slight as to escape detection until sought for. At the same time, I must admit that errors of diagnosis are by no means uncommon. Such errors, for the most part, arise from the circumstance that whenever the skin is bronzed, the existence of this disease is apt to be at once suspected. Now, whilst on the one hand there is no other disease which presents precisely the same train of constitutional symptoms, so that their presence, when unattended by any other obvious cause, ought to lead to a suspicion of the true nature of the case; so, on the other hand, pigmentation of skin more or less closely approximating that of Addison's disease occurs in the course of several other diseases.

Bronzing of skin, which, on a cursory examination, has frequently been mistaken for that of Addison's disease, is not rare in elderly persons of uncleanly habits, especially when infested with vermin; the condition is called "vagabond's discoloration" by the Germans, among whom it would seem to be more common than in this country. Several such cases have fallen under my observation.† The most remarkable of these was sent into the the Middlesex Hospital, as an unquestionable case of Addison's disease, several years ago. There was dyspnœa on exertion, cough, occasional vomiting and anæmia; but the whole train of symptoms did not correspond with those of Addison's disease, and those symptoms which existed were fully accounted for by the existence of aortic incompetence. Although, therefore, the discoloration more nearly resembled true bronzing than that of any other case of the kind I had

* See "Croonian Lecture," p. 107; and *Trans. of Pathological Society*, vol. xvi. p. 247.

† For a well-marked case see *Trans. of Pathological Society*, vol. xv. p. 226.

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previously seen, I negatived the original diagnosis, and on the death of the patient, eighteen months after his discharge from the hospital, the supra-renal capsules were found quite normal, and are now deposited in the Hospital Museum.*

Hepatic disease, and very chronic phthisis, sometimes give rise to bronzing of skin which might, on a cursory examination, pass for that of Addison's disease. Even the very characteristic discoloration so often found upon the tongue and buccal mucous membrane, and the areolæ of pigmentation around cicatrices, are sometimes present in cases of chronic phthisis.† I have myself seen several such cases in which the supra-renal capsules were found healthy after death. In lymphadenoma and leukaemia, associated with enlarged spleen, discoloration of skin in many respects resembling that of Addison's disease is not uncommon; of this, likewise, I have seen several examples. A very remarkable case of the kind was published by Dr. Paget, of Cambridge, in the *Lancet* for 1879, and as I shall have occasion to refer to it hereafter I subjoin a brief abstract of it.‡

The patient, a railway labourer, aged thirty-seven years, was admitted into Hospital in September, 1878, for well-marked lymphadenosis: the glands above the clavicles, in the axillæ, at the bends of the elbows, in the groins, &c., were much enlarged. The most prominent symptom was very profuse sweating, and the patient complained of weakness and of irritation in the palms of the hand and the soles of the feet. The blood was found on microscopical examination to contain an excess of white corpuscles. The temperature was above the normal standard, and there was a rise of from three to four degrees every evening. The appetite was good, and the constitutional symptoms of Addison's disease were absent. On admission, the patient's skin was of a dirty hue, but whilst under observation in hospital, patches of brown discoloration appeared on the loins, on the abdominal and thoracic walls, and in the flexures of the arms and legs, "resembling in all respects those met with in Addison's disease." A few weeks before death the areola of the left nipple became of a dark hue, and somewhat later that of the right also also became discoloured "but not nearly to the same extent or degree as the left." The face and backs of the hands did not participate in the discoloration. He lost flesh and strength, became markedly cachectic and died on January 19, 1879.

At the post-mortem examination, the internal lymphatic glands, more particularly those in the anterior mediastinum, were found to be greatly enlarged. The liver and spleen were also much enlarged, and the latter was studded throughout with hard, yellow growths about the size of peas. "On attempting to trace down the splanchnic nerves to the solar plexus, they were found to run into one mass of enlarged retro-peritoneal glands lying in front of the spine. The semilunar ganglion and solar plexus were entirely involved in this mass. The semilunar ganglion could not be found. The supra-renal capsules appeared normal in every respect."

Hence, it is evident that, however closely brown discoloration of skin may resemble that of Addison's disease, it is not in itself sufficient ground for forming a diagnosis in the absence of the constitutional symptoms.

The prognosis of Addison's disease is always grave; its course is ever downwards, and in the end it always proves fatal. But, though incapable of cure, its progress may

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* *Trans. Clin. Soc. of London*, vol. ix. p. 44.

† See "Croonian Lecture," p. 46; also "Trans. of Pathological Society," vol. xxiv., p. 94.

‡ *Lancet*, vol. i. 1879, p. 258.

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sometimes be much retarded by suitable management, and life has in a few cases been prolonged for several years.

The exciting causes of Addison's disease are usually very obscure. In a large number of cases it has been found to exist where no other morbid lesion, besides that in the capsules, was found at the post-mortem examination; and in most of these cases the clinical history has not afforded any clue to the causes of the disease.* We cannot, therefore, hesitate to admit that in many instances, so far as can be ascertained, it arises spontaneously. On the other hand, it has been so frequently associated with tubercular or quasi-tubercular products in other organs, that we cannot fail to recognize that the feebleness of constitution which is favourable to such lesions constitutes, at least a predisposition for Addison's disease. Again, it seems certain that a considerable number of cases of Addison's disease have originated in the extension of the inflammatory process from diseased or injured organs in the vicinity of the supra-renal capsules. I have myself seen four cases, and I have notes of thirteen others, in which the disease in the capsules was associated with abscesses in their neighbourhood, or with angular curvature of the spine.† In one of the earliest cases under my care, the patient herself attributed her illness to a strain in the back received several years before, at which time she thought something had given way in her right loin.‡ In that case, however, being unaware of the importance of the investigation, the condition of the vertebræ was not examined at the post-mortem examination. A similar history attaches to cases reported by Drs. Burdon Sanderson, Bristowe, Munro, Murchison, Risel and London, and by Mr. Workman, in all of which the patients referred their illness to the occurrence of some strain or fall.§

This view of the occasional origin of Addison's disease from disease or injury in the neighbourhood of the capsules, derives support from the circumstance that males are much more liable to it than females, and that a large proportion of the cases among them have been persons between the ages of twenty and fifty years—being just that period of life when men are most exposed to such injuries. Again, on inquiring into the nature of the occupations of the patients, I find that a very large proportion of them were men engaged in laborious out-of-door work.|| Still, notwithstanding the undoubted fact that a considerable number of cases of Addison's disease arise from some disease or injury in the neighbourhood of the supra-renal capsules, I must repeat the statement with which I started, that the causes of this disease are often very obscure.

It might not unnaturally be supposed, as Dr. Addison appears at first to have thought, that the complete destruction of the normal structure of the supra-renal capsules, and the consequent abolition of their proper function, whatever that may be, must be the cause of the train of symptoms characteristic of Addison's disease. This view is, however, conclusively opposed by the fact that the supra-renal capsules are said to have been extirpated in animals without sensible impairment of their health,¶ and by the even more pertinent fact that these organs have on many occasions been found to have had their normal structure entirely supplanted by cancerous deposit, and yet none of the symptoms

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* For references to 103 cases of this class see "Croonian Lecture," p. 195.

† See "Croonian Lecture," pp. 86, 104, 114, 125, 167; also *Trans. of Pathological Society*, vol. xv. p. 228; vol. xvii. p. 307; and Cotton, *Medical Times and Gazette*, vol. xxxvi. p. 33.

‡ See "Croonian Lecture," p. 101; and *Trans. of Pathological Society*, vol. x. p. 269.

§ For reference to these cases see "Croonian Lecture," p. 92, and App. A.

|| For fuller details see "Croonian Lecture," p. 94.

¶ See Harley, *Trans. of Pathological Society*, vol. ix. p. 401.

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of Addison's disease have ensued.* Indeed, it appears certain that, in many cases, the normal structure of the capsules must have been completely destroyed, by the morbid process of Addison's disease itself, for some considerable time antecedent to the development either of the discoloration of skin or the constitutional symptoms.† It follows, therefore, that this disease is not a direct, but can only be an indirect result of destruction of these organs. Excluding, therefore, the mere destruction of the capsules as inadequate to produce the disease, we are now to consider what other condition may account for it.

The supra-renal capsules are supposed by some physiologists to be ganglionic nerve centres. Without entering upon the consideration of this debatable question, it is sufficient here to observe that they have undoubtedly very extensive and intimate relations with the solar plexus and semilunar ganglia, and that very numerous branches of nerves pass to the capsules, among which are comprised several from the pneumogastric and phrenic nerves. The supra-renal capsules are thus not only very intimately connected with the chief centres of the ganglionic nervous system in the abdomen, but also both indirectly through that centre, and, more directly, through the pneumogastric and phrenic nerves, with the cerebro-spinal nervous centres. The condition of these nerve plexuses and ganglia has been carefully examined in all my later cases, and similar investigations have been made by other observers, both English and foreign. Among the the former, I may especially mention Drs. Habershon, Wilks, Cayley, Tuckwell, Renton, Coupland, and Barlow. In all these cases the neighbouring ganglia, nerve plexuses, and branches of nerves, including those from the pneumogastric and phrenic nerves, have been found to be firmly and sometimes inextricably embedded in the dense connective tissue by which the supra-renal capsules are surrounded. Moreover, the nerves themselves, even beyond the area of this general mass, have frequently been found thickened and enlarged by reason of increased thickness of their neurilemma;‡ and, in some instances, inflammatory changes have been met with in the connective tissue sheaths of the individual nerve fibres. Not to dwell upon other morbid changes, such as atrophy, fatty degeneration, and hæmorrhage, which are said to have been found in the nerves by several observers,§ it may, I think, be inferred that the pressure exerted upon the nerves by the hard connective tissue in which they are enveloped, and which, like similar chronic inflammatory growths, contracts as it becomes older, must seriously interfere with their functions. The question for present consideration, therefore, is how far the symptoms of Addison's disease may be referred to this condition of the neighbouring nerves. It is a question very difficult to answer in the present state of our knowledge, but I am myself of the opinion that most of the more prominent symptoms of the disease may be set down to this cause.

It is a well-recognized fact, that irritation in one set of the peripheral branches of a nerve may excite disturbance in organs supplied by other branches of the same nerve. This appears to be the explanation, for example, of the profound collapse which often so rapidly follows the ingestion of certain irritant poisons, or the perforation of the bowel in typhoid fever.

The functions of circulation, respiration and digestion, are each of them largely under the control of the pneumogastric nerve; they are, likewise, all of them much

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* For references to cases see "Croonian Lecture," App. B, p. 208.

† See "Croonian Lecture," p. 107; and *Trans. of Pathological Society*, vol. xvi. p. 247.

‡ See "Croonian Lecture," p. 128.

§ Ibid. pp. 70–73.

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disturbed in Addison's disease. When we remember that peripheral branches of this nerve are implicated in the hard, dense mass of connective tissue by which the supra-renal capsules are surrounded in Addison's disease, it is not difficult to believe that the depressed action of the heart, small compressible pulse, feeble, gasping respiration, retching and sickness may arise from reflex action set up by irritation of these branches. This supposition accords well with the clinical history of the disease, and seems to explain the remarkable alternations of exacerbation and remission which have been observed in all those cases which have been for some time under observation. It is quite likely that such irritation of the nerves should vary in intensity in different cases, and from time to time, in the same patient. In very chronic cases the disease may possibly proceed so gradually that few or no symptoms may arise until the structure of the supra-renal capsules has become totally destroyed. No doubt, in such cases as I am now supposing, the nerves are involved, but the process has gone on so gradually that the condition is tolerated, until some exciting cause produces the intense depression and other symptoms characteristic of Addison's disease, which may then prove very speedily fatal. On the other hand, under the influence of rest and judicious treatment, the irritation may temporarily subside, and so periods of more or less marked remission may intervene between the paroxysms. In thus attributing the prominent symptoms of Addison's disease to the effects produced upon the pneumogastric nerves by the pressure of the hard adventitious tissue in which they are involved, I by no means overlook the fact that the ganglia and plexuses of the sympathetic system are equally embedded in the mass of dense connective tissue; but my purpose being only to show that Addison's disease is a secondary consequence of the lesion in the supra-renal capsules, and that it is, in all probability, due to the morbid state of the important nerves supplied to or surrounding those organs, I need not speculate upon the precise share to be ascribed to the pneumogastric nerve and sympathetic nerves respectively; indeed, so intimate are the relations between the pneumogastric and sympathetic nerves in the abdomen, that it appears impossible to discriminate their separate influence.

If we admit that the symptoms of Addison's disease are really produced by the condition of the nerves, and not directly by destruction of the supra-renal capsules, it may be supposed that a similar condition of the nerves ought to be followed by the same consequences, even though the capsules themselves remained healthy. At present, the only authentic case of the kind with which I am acquainted is that recorded by Dr. Paget, an abstract of which has already been given. It was a case of pigmentation of skin resembling in many, though, as I have already pointed out, not quite in all respects, that of Addison's disease; but inasmuch as none of the very striking characteristic symptoms existed, it cannot be regarded as having been a case of that disease.

Various explanations have been proposed of the cause of the peculiar discoloration of skin in Addison's disease. My own opinion is that, like the constitutional symptoms, it is produced through the medium of the nervous system, though I am not able to explain in what manner. Dr. Paget's case affords very strong confirmation of this view, which is entertained also by several eminent pathologists. Virchow considers it as due to disease of the nerves in close relation with the supra-renal capsules, and Jaccoud and Bartsch, who have studied the subject very fully, refer it to disease or irritation in the sympathetic system of nerves. Although so commonly present, and, often, so obvious a feature of Addison's disease that it has obtained undue prominence, and led to many errors of diagnosis, it is not peculiar to that disease. It occurs in several other morbid conditions, to some of which I have referred in an earlier part of this paper; and even in Addison's disease it bears no constant relation to the constitutional symptoms. It is sometimes so marked as to attract immediate attention, whereas in other cases it has

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been very slight, and in a few cases, where the disease has been associated with wasting disease in other organs, such as phthisis or psoas abscess, it has been entirely absent.* Furthermore, when present, it does not run parallel with the constitutional symptoms, for in many cases it has preceded them, and in others it has not become apparent until some time after them. This is quite readily understood if we regard the constitutional symptoms as mainly, though probably not exclusively, caused by the injury to the branches of the pneumogastric nerves, but the pigmentation of skin to the sympathetic system. Whether the views here enunciated meet with general acceptance or not, they naturally suggest a careful study of cases of pigmentation of skin, especially in relation to the condition of the ganglia and nerve plexuses of the sympathetic nerve system, with the purpose of ascertaining whether, in cases of discoloration of skin unattended by the constitutional symptoms of Addison's disease, any common condition such as that present in Addison's disease and in Dr. Paget's very remarkable case be found.

Lastly, to sum up in few words the main points I have endeavoured to set forth in this paper. Addison's disease presents certain very characteristic and obvious constitutional symptoms—namely: asthenia, feeble action of the heart and very small compressible pulse, gastric irritability, hiccup, gasping, sighing and breathlessness on exertion; and these symptoms are, for the most part, attended by a peculiar and symmetrical discoloration of the skin. It has been invariably associated with one particular morbid lesion in the supra-renal capsules; but though invariably associated with this lesion, its symptoms would appear to be only indirectly due to it, for other morbid changes in the capsules, which have equally destroyed their normal structure, have not been found to produce the like symptoms. It is almost certain that these symptoms are caused, therefore, not by impairment of any special function of the supra-renal capsules themselves, but by injury to the important branches of nerves which pass into the capsules, and of the ganglia and plexuses in their immediate vicinity. Let me not be misunderstood: Addison's disease is a very definite disease, and is always connected with a very definite local lesion. Its diagnosis is as certain as that of any other chronic disease. I have myself diagnosed it repeatedly, and the correctness of the diagnosis has been verified after death; and upwards of two hundred and fifty cases bearing out the same statement have been reported by other observers.

DISCUSSION.

Prof. Semmola, Naples: Comme je ne comprends pas l'anglais je n'ai pas pu suivre la communication de mon honorable confrère, le Docteur Greenhow. Seulement d'après le résumé qui nous a été communiqué en francais, il paraît que, selon mon honorable confrère, il faut à l'avenir faire une étude attentive des cas de pigmentation de la peau, sans maladie d'Addison, quant à ce qui regarde l'état des nerfs, ganglions, et plexus du sympathique. Je me permets de rappeler à ce propos, qu'il y a déjà six ans, que j'avais l'honneur de présenter au Congrès de Bruxelles un travail dans lequel je soutenais que la maladie d'Addison devait être considéré comme une maladie des centres nerveux ganglionaires, et que les altérations anatomiques des capsules surrénales n'étaient pas du tout le point de départ de la maladie; et représentaient, quand elles existaient, le dernier effet des troubles trophiques produits par les filets nerveux qui président à la nutrition de ces organes. A cette époque (1875) je présentais principalement des arguments thérapeutiques en faveur de la pathogénie nerveuse de la maladie d'Addison. Mais depuis j'ai continué mes recherches dans cette direction, et aujourd'hui j'ai l'honneur de mettre sous vos yeux

* For references see "Croonian Lecture," App. p. 205.

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cette figure qui représente les altérations microscopiques de quelques points des centres ganglionnaires et de la section dorsale de la moëlle. Il s'agit, comme vous voyez, d'une transformation myxomateuse du stroma du ganglion cæliaque et de l'infiltration leucocytique de la névroglie de la moëlle épinière vers le canal central. Dans un autre cas j'ai observé une dégénérescence graisseuse de plusieurs points du plexus solaire. Ce qui rend très remarquable le premier cas, dont vous avez la figure, c'est que l'analyse microscopique des capsules surrénales donna des résultats complétement négatifs. Elles étaient à l'état normal. La conclusion est très facile, et la voici. La maladie d'Addison est un profond trouble de la nutrition déterminé par l'altération successive des fonctions du sympathique et des divers centres nerveux de la vie organique (ganglion cæliaque, etc., etc.). Il n'y a qu'à suivre le déroulement caractéristique des symptômes de cette maladie vis à vis des différentes fonctions des centres nerveux ganglionnaires pour saisir facilement que la production des troubles digestifs, de l'état cachectique, de l'état asthénique, de l'abaissement de la température, etc. etc., ne sont que l'envahissement successif d'un processus morbide qui commence par altérer les travaux digestifs et finit par affaiblir considérablement les oxydations organiques et les échanges nutritifs; c'est-à-dire, les fonctions de la vie végétative qui se trouvent sous la dépendance du sympathique et de ses annexes. L'on pourrait presque dire que dans ce cas de la maladie d'Addison la clinique nous fait la démonstration pathologique des fonctions physiologiques du système ganglionnaire. L'état asthénique, qui est incontestablement un des traits les plus saillants, du tableau clinique présenté par cette maladie, ne peut pas du tout nous embarrasser dans cette pathogénie que je défends, parceque il me semble d'une évidence indiscutable que, puisque l'activité chimique des éléments histologiques est tellement troublée, dans son degré et dans la nature des produits, que la chaleur animale en est vivement blessée, l'on ne peut s'étonner que la force de l'organisme tout entier soit considérablement baissée, puisque sa source est en voie de se tarir. En effet c'est dans les activités chimiques de l'organisme (dédoublements, oxydation, etc. etc.) qu'il faut retrouver la source incessante de la chaleur et de la force. Il n'y a donc pas lieu de s'étonner que l'état asthénique soit le trait le plus saillant du tableau clinique. C'est la lampe qui s'éteint; et, puisque ce n'est pas l'huile qui fait défaut, ou la mèche qui est finie, il faut bien recourir aux centres nerveux qui président aux fonctions de l'huile et de la mèche. Pardonnez-moi cette comparaison. Il resterait, cependant, la mélanodermie qui est aussi un symptôme caractéristique de la maladie d'Addison, lorsqu'elle accompagne l'état asthénique et les troubles digestifs. Eh bien, d'abord les capsules surrénales ne peuvent pas être en question, comme cause de la mélanodermie, parceque, dès qu'il existe un seul cas de maladie d'Addison bien caractérisée, dans lequel l'analyse histologique prouve péremptoirement qu'elles sont à l'état normal, en bonne logique expérimentale, il n'y a pas de temps à perdre, il faut tourner la page, et diriger ailleurs ses recherches au lieu de s'entêter à vouloir trouver par force la démonstration d'une fausse idée étiologique, seulement parcequ'elle est préconçue. Il sera, toujours très utile d'étudier l'état des capsules surrénales, ce sera même nécessaire, mais cela ne sera jamais plus pour y puiser la cause de la mélanodermie, parceque dans la nature, Messieurs, vous le savez très-bien, il n'y a pas d'exception possible; les exceptions ne sont que l'effet de notre ignorance et dès qu'une seule exception se présente dans l'explication d'un fait, il faut changer de plan au lieu de subtiliser et de torturer les faits pour avoir raison. Sans cela on est sûr de faire fausse route. En conséquence, dès qu'il est incontestable que la mélanodermie d'Addison n'est pas un effet nécessairement lié à l'altération des capsules surrénales et dès qu'il y a tant d'autres raisons pour admettre que les autres points du tableau clinique de la maladie bronzée sont parfaitement éclaircis par la physiologie du sympathique et de ses annexes, la logique impose

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d'examiner si cette théorie nerveuse peut aussi contenir une explication raisonnable de la pigmentation morbide. Et bien, oui; rien ne s'oppose à admettre que le sympathique et les autres centres nerveux ganglionnaires soient capables de présider à la formation des pigments. D'abord il y a une considération physiologique d'ordre général en vertu de laquelle il est très-naturel de présumer que la formation des pigments soit présidée par l'influence nerveuse trophique, c'est-à-dire qu'elle soit sous la même influence qui préside à tous les actes chimiques de la nutrition, dans le mécanisme de laquelle la pigmentation représente une étape normale de la matière vivante. Des recherches expérimentales, fournies par la physiologie, confirment directement cette manière de voir. Viennent après des cas bien sûrs de pigmentation morbide des cheveux de la peau sous l'influence de causes morales violentes. La science a enregistré quelques exemples de mélanodermis survenue rapidement en peu de jours après des émotions morales violentes, et qui ont duré pendant toute la vie. Enfin les preuves thérapeutiques ne font pas défaut, car le cas clinique que je présentais au Congrès de Bruxelles était très-démonstratif. Il s'agissait d'un malade d'Addison qui suivit un long traitement électrique sous ma direction, en employant le courant constant entre le cou latéralement et l'épigastre. Ce malade qui avait des syncopes dès qu'il s'efforçait de s'asseoir au milieu du lit, après le traitement électrique, qui dura trois mois, avait repris ses forces tellement qu'il pouvait faire chaque matin une promenade à cheval de deux heures, sans se plaindre de faiblesse. Eh bien, chez ce malade, avec les forces, la chaleur animale avait repris son degré normal, les digestions n'étaient plus troublées, et ce qui est principalement à remarquer, la pigmentation cutanée dans plusieurs points avait disparu. En conséquence, je le répéte, dans les conditions actuelles de la science, la théorie la plus admissible pour éclaircir la pathogénie d'Addison, c'est que cette maladie est constituée d'abord par un épuisement fonctionnel des centres ganglionnaires abdominaux avec troubles successifs des fonctions digestives et des fonctions nutritives. Peu à peu, à un simple épuisement fonctionnel succède une altération histologique de ces mêmes centres qui peut être variable et dont je mets un exemple sous vos yeux. Selon que différents points de ces centres nerveux ganglionnaires sont attaqués, il y a des altérations dans la nutrition des capsules surrénales ou autres organes, sans que pour cela ces altérations anatomiques soient le vrai point de départ de la maladie. Il resterait à voir si dans la maladie d'Addison toutes les dégénérescences qui frappent les éléments nerveux ganglionnaires abdominaux peuvent aboutir à la même production de tous les symptômes comme simple conséquence de l'épuisement ou de la suppression fonctionnelle, ou s'il s'agit de quelque cause spécifique qui frappe ces centres nerveux. Dans l'état actuel de la science, une réponse définitive est impossible; mais, cependant, l'on peut présumer avec beaucoup de fondement que cette cause spécifique n'existe pas, et qu'il faudrait plutôt rechercher rigoureusement laquelle des influences étiologiques est capable d'être le point de départ de cet état morbide qui a quelque chose de trop caractéristique pour qu'on puisse le croire un simple effet des causes épuisantes ordinaires.

Dr. Noel Gueneau de Mussy, Paris: I have been glad to hear, supported by the authority and eloquence of Dr. Semmola, an opinion which I defended a few days ago before the Academy of Medicine in Paris, and I ask for permission to bring forward a few more arguments in its favour. If one meets with cases of extensive pigmentation of the skin, without lesion of the supra-renal capsules, and if we find profound alteration of these capsules, without manifest pigmentation, whatever real importance we must attribute to them, the lesions of these organs are not the direct cause of pigmentation; and this cause is to be looked for elsewhere. The intimate and very numerous connections of these capsules with the solar plexus, directs our attention to this structure

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where the pneumogastric and ganglionic nerves unite, constituting a sort of brain for the ganglionic nervous system. The experiments of Dr. Brown-Séquard have proved that direct irritation of this plexus was followed by an appearance of numerous pigment-granules in the blood: thus, experimental physiology supports pathological induction in localizing in the abdominal nervous system the starting-point for integumentary pigmentation. Moreover, we find a remarkable production of pigment in many cases where the abdominal nervous system is the seat of an anomalous irritation. This production of pigment is certainly not Addison's disease, but purely from the point of view of the integumentary lesion, it seems to me that it may be considered as an approach to it. Thus, in pregnancy, everybody recognizes the habitual development of pigmentary deposit in the nipples, on the linea alba, on the forehead—where it has received in France, amongst the vulgar, the name of mask of pregnant women. In the phthisical, the old physicians noted as a symptom of abdominal phthisis, the earthy colour of the face; and it is now more than thirty years since I called attention to the appearance of pigmentary stains on the face as a sign of the invasion of the abdominal organs by tuberculosis. These spots are first seen on the temples and forehead; they may extend over the face, and even on other regions. Their presence has enabled me several times to predict the onset of functional disturbances in the abdominal organs before they actually appeared. I do not aver that abdominal tuberculosis is always accompanied by this pigmentation; but in all cases when in the phthisical I have noted this symptom, the abdominal organs were implicated. Twice only, in my long practice, have I seen this pigmentation well pronounced, without diarrhœa or other considerable disturbance of the digestive functions. In these two cases I have been enabled to make the autopsy, and found tuberculous ulceration of the intestine and tubercular infarction of the mesenteric glands. In cancer of the stomach, in cirrhosis and other affections of the abdominal organs, which may produce irritative action on the solar plexus, one may observe these pigmentations. Whether irritation of the pneumogastric suffices, as I believe Dr. Greenhow has declared, to produce pigmentary deposits, I shall not venture to deny absolutely, for I have seen, as have others, pigmentary stains in an illness where the functional disturbances of the pneumogastric seemed to me to play an important part—exophthalmic goitre.

Dr. Zuelzer, Berlin: The speaker mentions as characteristic the "apparent diminution of organic change." I should like this expression to be understood in this way: that the process of organic change is not, as in the normal state, distributed over all the tissues of the body, but is chiefly confined to the blood, extending in a very small degree to the other tissues. The analyses of urine in this disease, although not numerous, seem to prove this. They show that there is not only a considerable diminution of nitrogen, but that the relative value of the phosphoric acid is also greatly reduced. Patients suffering from Addison's disease, though nourished like healthy people, show this peculiarity to such an extent that we may well base the diagnosis on it. I hope to have the honour to refer to this subject again, but should wish to profit by this occasion, to direct the attention of those gentlemen who may have an opportunity for further observations, to the examination of the urine.

Professor Paget, Cambridge: The facts of the exceptional case published by me and quoted by Dr. Greenhow, were observed by others as well as by myself, the supra-renal capsules having been carefully examined by Dr. Creighton, and the facts are entirely free from any doubt.

Professor Gairdner, Glasgow: I will proceed to refer to a case which has occurred to me in private practice, and which, though not involving any question of doctrine as

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discussed in Dr. Greenhow's elaborate and admirable paper, appeared at the time, and still appears, to present questions of fact, and of legitimate inference from the facts, which the present occasion seems an almost unique opportunity for answering. In this case, which was brought under my notice at an advanced period of the disease, I had no doubt or difficulty at all in recognizing both the constitutional and the local symptoms of Addison's disease in a marked form, and the prognosis, accordingly, was exceedingly grave; although, perhaps, the one remark to be made on this point would be, in my opinion, that the immediate prognosis is stated by Dr. Greenhow in too extreme terms, and that in some cases, at least, the downward progress may be temporarily arrested. In this case it was not arrested; death took place, and a post-mortem examination showed, as regards the supra-renal capsules, exactly the appearances figured in Dr. Greenhow's drawings. The only point which gives to this case special importance is the fact that it was seen at a somewhat earlier period by an eminent dermatologist, Professor McCall Anderson, who pronounced it a case of leucoderma, as it undoubtedly was; the peculiar pigmentation characteristic of Addison's disease being associated with well-marked circular leucodermatous patches on the wrists, hands, scrotum, &c., around which the peculiar darkening from pigment flowed, as it were, diffusely over the general surface, being specially dark immediately around the circular pale patches. Now, the question suggested to me by this case is, whether the association in question was purely accidental, or whether there is to be admitted a form of Addison's disease in which the pigment is so irregularly disposed; and, as in the earlier stages of the controversies in London about the disease, Dr. Wilks had been led to disown a case in Dr. Addison's own book, on the ground, chiefly, of the irregular type of the pigmentation, it appears to me not superfluous to bring up this detail in connection with the general question.

Dr. Matterson, York: Believing that many skin affections are dependent upon disordered innervation, I would wish to confirm the opinion that morbus Addisonii is due probably to disease of the solar plexus. As bearing upon the prognosis, I have had under my care a case of nervous pulsation of the abdominal aorta, accompanied by pigmentation of the skin, resembling morbus Addisonii. The patient recovered under the use of iron and digitalis, and change of air.

Sir William Gull, London: We may regard the supra-renal bodies as nerve centres related, at least in texture, to pituitary bodies. The pigmentation of skin in Addison's disease for the most part is peculiar in its general diffusion—as in the skin of an Arab—and more marked at points, as axillæ, areola of nipple, and groin. In all cases the signs of asthenia, without emaciation, must be taken as the basis of the malady. Probably all cases do not have a fatal course. One case, aged fifty-one, with all the symptoms of morbus Addisonii has recovered, and is now well.

Correlating Function of the Sympathetic Ganglia.

Dr. Edward Woakes, London.

The inferior cervical ganglion a correlating nerve centre; instances in which it determines the localization of morbid phenomena; and the evidence thereby afforded that the sympathetic ganglia subserve the purpose of correlating centres for distant tissue areas.

Subject stated. The conclusion generalized from the following remarks is to the effect that the *afferent fibrillæ* of a sympathetic ganglion, which are for the most part

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associated with sensori-motor nerves, are in *reflex relationship* with the *efferent vaso-motor nerve*, furnished to the arteries from the same ganglion which receives the afferent fasciculi. In other words, an excito-vaso-motor function is established between the afferent and efferent elements of a given sympathetic ganglion. It is in this sense that these organs are said to exercise a *correlating function*, in regard to such operations as belong to them, between tissue areas often widely separated.

The inferior cervical ganglion is selected to illustrate this position, because the author's experience lies largely within the domain of its operations; and because, it being the controlling centre of the circulation of the labyrinth, the phenomena exhibited by alterations in the tension of this organ, whether extrinsic or intrinsic, direct or reflex, are attended with phenomena so striking and pronounced as to admit of no misinterpretation.

Date on which conclusions are based.

The central fact in connection with this subject is one first observed by Drs. Weir Mitchell, Moorhouse and Keen ("Gunshot Wounds, and other Injuries of Nerves," Philadelphia, 1864) to the effect that gunshot wounds of the brachial plexus, whether inflicted in the upper arm or neck, cause the subject of them to fall down, consciousness being sometimes, but not always, momentarily lost. In all, a degree of general weakness amounting almost to collapse is present. The conditions portrayed are most marked where the nerves are crushed or contused, rather than when they are divided; they occur irrespectively of loss of blood, because the vessels escaped injury.

Whatever may prove to be the explanation of this occurrence, it has the value of a vivisection experiment possessing crucial indications.

For its interpretation it is necessary to discover whether corresponding phenomena can be produced by other means, and, if so, to ascertain whether the mechanism involved in the imitated occurrences connects itself in any reasonable way with the lesion first noticed.

An experiment, originating with Weber-Liel, of Berlin, and which any one with sufficient tact may easily repeat—shows that pressing on an exposed stapes through the middle ear, causes the subject to stagger, fall down, and sometimes to lose consciousness. If considerable pressure be exercised, vomiting and general collapse ensue. It is a fact well known to otologists that external syringing, or inflating the middle ear with air—the former by exerting pressure on the ossicles, the latter by the same effect being exercised on the membranes of the fenestriæ—are sometimes attended with like results.

Pressure then on, or tension of, the organ of equilibration, which all evidence concurs in locating in the labyrinth, can produce the phenomena of falling and unconsciousness seen to attend contusions of the brachial plexus.

The next point is to determine the association between these regions. This, in brief, is found to reside in the inferior cervical ganglion, fibrillæ from which enter the cords of the brachial plexus, whose function is afferent to the ganglion; while other fasciculi from it furnish efferent vaso-motor nerves to the vertebral artery, the latter being the chief source of blood-supply to the labyrinth.

My contention is that the shock resulting from the concussion of the brachial nerves, in gunshot wounds of them, is, so far as their contained sympathetic elements are concerned, propagated first of all to their centre, the inferior cervical ganglion, and thence is reflected as a wave of vessel-dilatation to the vertebral artery. This dilator wave is appreciated first of all in its peripheral branches, producing an immediate large accession of blood to them. That we are made cognisant of its effect upon the internal auditory branch of the dilated vertebral artery, because it alone is associated with an organ capable of demonstrating its presence by so palpable a feature as the immediate

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destruction of the equilibrium of the subject. This sudden tension of the intra-labyrinthine fluid, brought about as described, is comparable in degree and in effect to that similar tension seen already to be occasioned by the direct pressure of a probe upon the exposed stapes.

Alternative channels of conduction examined.

Before accepting definitively this explanation of the phenomenon of falling in gunshot wound of the brachial plexus, it is necessary to examine whether the effect could be produced through any alternative channel to that above delineated. That the shock is not conveyed by way of the spinal column to some cerebro-spinal centre, is evidenced by many subsidiary facts, which will be developed later on. One negative proof to this effect exists in the absence of evidence that the *centrum cilio-spinale*, located in the zone of the spinal cord, above the level of the second dorsal vertebra (Budge, Waller, Eulenberg, and Guttman, &c.), within the limits of which the brachial plexus arises, was in any way implicated in the shock. As is well known, irritations of the cord within these limits—at least such as involve the sympathetic fibres contained therein—produce changes in the size of the pupil, and of the commissure of the eyelids, which were entirely absent in the cases reported by the American authors referred to. Indeed, they distinctly affirm, "we have seen as yet no such result in wounds involving the plexus." The clearing away of this alternative pathway of impression, by which otherwise it might be conceivable that a hypothetical centre of giddiness, seated somewhere in the medulla, may have been affected and so produced the falling, simplifies the argument, by confining the sphere of operations to the only other medium—viz., that which has been indicated above.

A remarkably suggestive case, No. 21 ("Injuries of Nerves and their Consequences," S. W. Mitchell, M.D., Philadelphia, 1872), affords direct evidence in favour of the course already pointed out. G. A. was shot through the left ulnar nerve, and had at once great *agitation of the heart*, which was for some days his dominant symptom. When it is remembered that the inferior cardiac nerve of this ganglion is an important factor in regulating the heart's action, and that a shock reaching the ganglion from the afferent fibres of the wounded ulnar nerve may be reflected in the ganglion to the inferior cardiac, it is easy to understand how such "agitation of the heart" as happened in this case may be thus brought about. Farther on, it will be shown that parallel symptoms of cardiac disturbance are met with all through the series of phenomena traced to lesions of this ganglion, and this when no question of alternative channel can arise, as when the organ itself is proved to be diseased. Indeed, the cardiac symptoms assume such importance in this relationship, that I have been elsewhere led to assign a conservative reason for linking the organ of equilibration with the heart, through the medium of this ganglion, because it thereby comes to pass that, when the heart is so suddenly and violently disturbed, the labyrinth being likewise exposed to tension, the subject is compelled to fall down, and is perforce placed in a position most favourable to a continuance of the circulation, under circumstances otherwise calculated to bring it to a stand.

Crucial meaning of foregoing facts.

These facts, together with others corroborative of them to be shortly detailed, the author regards as possessing a crucial significance. Not only do they correspond with Sanderson's views ("Handbook for the Physiological Laboratory"), to the effect that sympathetic nerves which are associated with sensori-motor nerves, are in afferent relationship with the ganglion with which they communicate, but they further indicate that these *afferent fibres are in reflex relationship with the efferent vaso-motor fasciculi furnished to the arteries from the same ganglion which receives the former.*

It is in this sense that the ganglion is regarded as correlating the areas implicated by

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both sets of nerves. This fact is of the widest importance, inasmuch as it implies that an excito-motor function appertains to these two elements of the sympathetic system, of quite the same meaning, as is that established between sensory and motor nerves of the cerebro-spinal system; with this difference, however, that afferent impressions reflected through the medium of a sympathetic ganglion, take effect upon the walls of the blood-vessels, and by enlarging or contracting their calibre, produce alterations of nutrition or of function in the areas affected by them.

The further evidence adduced to substantiate the foregoing position is gleaned from the phenomena of disease, of accidents, and from such as have been elicited purposely, though with other objects in view. By this means the position in question is subjected to the Baconian rule of varying the circumstances by which a given result is brought about, a principle accepted by J. S. Mill as the first rule of physical inquiry, and the foundation of every other.

Referring first to chronic diseases, the symptoms of which are manifested in the areas innervated by the inferior cervical ganglion, we have in *vertigo* a good example for illustrating the correlating function of this centre. It must be admitted that the ideas associated with the term vertigo are somewhat vague, perhaps because the associated symptoms are so varied and puzzling in their distribution. Yet I shall hope to show that the application of the correlating principle resolves them into intelligible, if not simple, proportions.

Giddiness is a well-recognized concomitant of deranged digestion, and in this relationship it is necessary to point out the anatomical connection between the stomach and the labyrinth. After parting with its recurrent branch, the vagus nerve receives several branches from the inferior cervical ganglion, which accompany it to the stomach and adjacent viscera. These are afferent as regards the ganglion; consequently, peripheral impressions received by them will be conveyed to the ganglion, to be thence transmitted as reflex efferent impressions to the vessels receiving vaso-motor nerves from this centre. As already stated, that share of the impulse participated in by the vertebral artery will express itself as vertigo. It will be difficult to find a better illustration than one I have already published, although such cases are of frequent occurrence. It is that of the aged gentleman, who, having lost his teeth, and having no artificial substitutes, partook freely of a meat diet, and became vertiginous—that is to say, on rising or walking, he experienced a tendency to fall forwards. These symptoms disappeared whenever his diet was restricted, to return whenever butcher's meat was resumed. So often was this rôle of events repeated that there could be no doubt as to the cause of the symptom. In explanation of it, one cannot fail to recognize the analogy between the shock propagated from a contused brachial nerve in the cases referred to as the result of gunshot wounds, with their attendant vertigo, and the lesser shock produced upon the nerves of the stomach by the lumps of unmasticated meat, in the case of the toothless patient, and reflected by the channels indicated to the labyrinth, and, like the former, causing disturbance of equilibrium, though to a lesser extent.

The labyrinth has a sentinel-like function.

Here it seems necessary to repeat an observation respecting the sentinel-like function subserved by the labyrinth under these circumstances. Although the internal auditory artery which supplies it is one of the smallest branches of the vertebral, yet, owing to the noticeable character of the symptoms to which its distension gives rise, it is enabled to give an effective warning of the state of the circulation in all the important nerve-centres which receive their blood supply from the same source. Only, to profit by this conservative voice of Nature, it is necessary first of all to recognize the mechanism on which it depends—which is, in fact, that of the correlating function of the ganglion under review.

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But vertigo, having a determining influence in the *primæ vitæ*, is a trite event where the patient is neither aged nor toothless. In many such instances the subject distinctly recognizes a sensation at the epigastrium—a stomach aura—as preceding the giddiness which not only warns him of its approach, but leads him to identify this viscus as in some way concerned in its occurrence. In some of these instances the patient has spontaneously referred to a precedent sensation travelling upwards from the epigastric region along the side of the neck to the ear, thus almost mapping out the anatomical route which has been above assigned to impressions reaching the labyrinth from the stomach.

Ear lesion always present in stomachic vertigo.

Speaking generally, it may be affirmed that in all cases of so-called stomachic vertigo there will be found a superadded lesion of the auditory apparatus, quite discernible, and associated with defect of hearing. This objective lesion consists of a greater or less degree of retraction of the manubrium mallei, necessitating pressure on the stapes. Unless there be also, which has not happened in the instances above alluded to, destruction of the malleo-incudal joint, the mechanism of the latter is such as to prohibit any great degree of extension being maintained through its means upon the labyrinth. It is not in any sense comparable to that exerted by the direct pressure of a probe on the exposed stapes, in the experiment above detailed.

Occasional character of giddy attacks explained.

A slight degree of pressure upon the labyrinthine contents is, however, constantly maintained as the result of this local lesion, but it is insufficient to explain the occasional attacks of giddiness which is the peculiarity of these cases. It needs the introduction of another factor to do this. Such additional factor is provided whenever the stomach disturbance reaches a point in excess of that which usually obtains in these subjects, as when from indiscretions of diet, or other cause, the irritability of the digestive apparatus becomes extreme. Then are set up those impressions upon the afferent sympathetic fibres distributed to this viscus, which reflexly express themselves by dilating the vertebral artery. The intrinsic blood-pressure thus exerted on the labyrinth, plus the already existent local tension, constitute the conditions which determine an attack of vertigo. These, therefore, are cases of stomach vertigo, "grafted upon a labyrinth already prepared by local mischief to appreciate very slight impressions upon its circulation" ("Deafness, Giddiness, and Noises in the Head," by the Author). There can, I think, be no doubt that stomach vertigo is of rare occurrence where such previous local preparation in the auditory apparatus does not coexist. The combination of these two factors can alone be held to elucidate the uncertain and irregular appearance of the giddiness, which has hitherto been an unexplained characteristic of the disease, and which must so remain until the link uniting the stomach and the labyrinth—viz., the inferior cervical ganglion with its physiological potentialities—receives recognition.

Cardiac symptoms.

Prominent among the symptoms associated with vertigo, are those which implicate the heart, such as *palpitation*, *præcordial distress*, and others indicative of functional disturbance of the central organ of the circulation. These cardiac symptoms result from the reflex participation of the middle and inferior cardiac nerves proceeding from the respective cervical ganglia, in the initial impression, the origin of which is in the stomach. Although the physiological function of these nerves in man has not been completely determined, the general tenor of evidence points to them as accelerators of the heart's action. Reflex impressions reaching the heart through the channels indicated, would, if they correspond in kind to those previously investigated, be of the nature of paresis—that is, a suspension or diminution of function, rather than an accession to it. Interference with the share exercised by these nerves in regulating the rhythmical movements of the heart, may be expected

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to show itself in perturbation of its action, accompanied with pain and sense of oppression in the præcordial region, which are the usual conditions met with in vertigo.

Mottling of hands, &c.

Another symptom associated with vertigo, and to which I have elsewhere called attention, is that of marked *mottling of the hands and forearms*, due to congestion of the superficial arterioles. This condition points to the reflex implication in the vessel-dilating waves transmitted through the inferior cervical ganglia of the vaso-motor branches furnished by it to the brachial artery and its ramifications. This congestive mottling is an important symptom, because it illustrates the tendency of the dilatation to become most evident in the peripheral portions of a vessel area; and because, coming as it does under direct inspection, it affords confirmative evidence of the state of the vascular system in those correlated regions which are not thus open to direct observation.

Aura.

The occurrence of a *premonitory aura* has been already alluded to. Besides the instance quoted above, in which the premonition appeared to the patient to proceed from the stomach along the neck to the ear, it is not rare to hear such a sensation described as travelling upwards along the arms to the side of the head, and always, so far as my observations extend, to that side to which the affected ear belongs. In each of these instances it will be observed the tracks followed by the aura correspond with fasciculi in afferent and efferent association with the inferior cervical ganglion.

Paresis of inferior cervical ganglion.

Passing from these reflex phenomena, we next are confronted with a group of analogous symptoms, in which the organ primarily responsible for their production is the ganglion itself. Here the sympathetic centre must be regarded as being in a condition of inherent or acquired defect—*paresis*, that is—so that it permits faulty impressions, usually having the character of deficient inhibition to radiate along the lines of its distribution, and so giving rise to symptoms which correspond to the organs implicated. These are, as before, palpitation, gastralgia, injection of the palmar surfaces, with or without vertigo, as well as some others to be immediately noticed. The subjects exhibiting this complexity of symptoms are chiefly young and, so-called, neurotic females.

Gôitre.

A feature of special interest in these patients is that they frequently develop in addition a small *goitre* or *bronchocele*. This fact testifies that another efferent vessel-area of the ganglion, and of its congener, the middle cervical, is involved in the process—viz., the vaso-motor nerves of the inferior thyroid artery; from which ensues first, passive congestion, and subsequently hypertrophy of the thyroid body.

But the study of the pathogeny of gôitre affords direct evidence of the correctness of the position attempted to be here established—*i.e.*, that lesion of the ganglionic centre is directly responsible for the symptoms herein referred to it. Briefly stated, the nature of this evidence is as follows:—1st, a fatal case of exophthalmic gôitre reported by Dr. Shingleton Smith of Clifton, in which the autopsy showed that the inferior cervical ganglion had undergone degeneration, and was represented only by a mass of cellular tissue, with a calcified substance for its contents, while the middle and superior ganglia afforded indications of commencing atrophy. 2nd, the fact that goitre frequently dates its commencement from a shock to the general system, which manifests itself in the part of least resistance—this part being the nutritional centres constituted by the cervical ganglia, the defect in which may be usually traced to hereditary influences. One such case I have reported recently in the *Lancet* (The Pathogeny and Treatment of Gôitre, March 19, *et seq.* 1881). In intimate association with this is the fact that the

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incidence of goitre is occasionally preceded immediately by an attack of vertigo of extreme intensity. A suggestive instance of this sequence—viz., long-continued exhaustive environment, access of vertigo, commencing enlargement of the thyroid body developing into a fibro-cystic bronchocele enduring over many years—is at present under treatment by my colleague, Dr. Whistler, and will be doubtless reported by him.

Neuralgia.

The category of symptoms referable to paresis of this ganglionic centre includes certain *neuralgias* — *e.g.*, shoulder-tip pain, neuralgia mammæ and irritable breast, brachial neuralgias and some cervical ones. These conditions have a parallel explanation to the preceding ones, of which they are the frequent companions—that is to to say, the neuralgia is due to the dilatation of the vasa-nervorum, the nutrient vessels contained within the nerve-sheaths, and to the pressure thus occasioned to the associated sentient fibres. The *diurnal variations* of such neuralgias are traceable to the recuperation of ganglionic force by rest and food, and its exhaustion by the occupations of life. The former state implies vessel-inhibition from the exercise of the renewed ganglionic energy—the latter, vessel-dilatation, when this energy is exhausted.

Summary of symptoms and proposed term.

It will be seen that the symptoms traced in the foregoing review to a paretic state of the ganglionic centre in question are both varied in character, and widely diffused as regards their localization. Withdrawal of sympathetic nerve-force is thus seen to express itself either simply, as passive congestion, witnessed in the palmar surfaces; or, if long maintained in a region suitable for exhibiting such phenomena, there are added those of hypertrophy, seen in the thyroid body, and the mammary gland; or, if occurring in organs of specific function, its presence is indicated by interference with that function, an example of which is afforded in vertigo from implication of the labyrinth; and again, by pain, as in neuralgia, from sentient nerves becoming involved in the process. This group of symptoms, with more or less of completeness, and in ever-varying combination, occurs so frequently as to constitute a disease *per se*. As such they seem to demand a distinctive appellation. Having regard to the organs whose correlating function determines their localization, the author proposes to identify their occurrence by the term "*Paresis of the lower cervical ganglia.*"

Before leaving this part of my subject, it is desirable to state that the apparent ignoring of the influence of the spinal cord, with its contained sympathetic fibres, and general vaso-motor centre, has been purposely adopted, because from long consideration of much evidence impossible to detail on this occasion, it is clear that the subject under discussion is altogether independent of these factors. While, however, it is impossible to supply all the physiological data upon which the foregoing conclusions are based, the following supplementary evidence should be borne in mind, when judging of their correctness.

One such fact of very wide significance is quoted by Power (*Practitioner*, 1873); it is to the effect that "children destitute of brain and spinal marrow are capable of attaining fœtal maturity, and consequently of maintaining their nutrition at the highest point." In such anencephalous children it is apparent that, so far as their nutritional development was due to the regulatory influence of nerve-centres, these must be referred to the sympathetic system which alone was possessed by them.

As regards the vessel-phenomena above attributed to the influence of the sympathetic ganglia and their nerves, it is to be observed that the researches of Morat and Dastre (*Comptes Rendus*, 1878) which are the most satisfactory, as well as the most recent with which I am acquainted, are consistent in almost every particular with the views advanced in the foregoing remarks.

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On the Treatment of Scrofulous Glands.

Dr. Clifford Allbutt, Leeds.

The purpose of my present paper is to insist upon the influence of local causes in the production of that disease of the glands, either of the neck or elsewhere, which goes by the name of scrofula.

This inquiry is not important only from a pathological point of view, but also bears very strongly upon the principles and procedure of cure.

The name scrofula, as most of my hearers are aware, is derived from the word *scrofa*, a sow, and is given to the malady in question because of the fulness of the neck therein—a fulness which approaches the contours of the neck of the swine. The condition is so conspicuous that it could not long escape description, and it has, in fact, been described from very early times. These descriptions, at first pictorial only, gradually became more analytic, and in our own day the mapping of the diseased glands and lymphatics and the revelation of the morbid elements of the same parts are very complete. On this morbid anatomy of the subject I have not to dwell to-day. Concerning its pathogenesis the prevalent opinion has always set strongly in the direction of hereditary causation. I am probably correct in saying that heredity almost alone is assigned as the cause in our text-books and popular conversation.

Nor am I unmindful of the potency of hereditary causes in the development of the malady. While, on the one hand, I must roundly assert that scrofulous neck may be, and is, often found in persons having no history nor foregoing signs of such hereditary habit, yet, on the other hand, I see as clearly that hereditary habit is an important factor in its causation in others.

Nevertheless, I believe that in all cases—in the disposed and the undisposed alike—local causes are essential; indeed, I am prepared to believe that scrofulous neck does not and cannot arise by inward vice without the co-operation of certain peripheral antecedents.

The question thus, in my opinion, is reduced to one of the degree, duration, and kind of local irritation, this being the essential factor to consider both in respect of causation and cure; the co-operation of heredity being various and inessential.

Where heredity is predominant, local causation takes, no doubt, a lower place; where heredity is less, local causation is more; and where it is absent, local causation stands alone. The operation of local causes may in some few cases be obscure or hidden throughout, and is to be assumed in them only by deduction from a large previous experience; but these cases are few and rare, if we confine ourselves to those of which the history is tolerably complete.

In the cases of which our knowledge is more perfect, we find that the glandular enlargements are bubonic in origin, and that they arise in the neck from two main sources—the mucous lining of the pharynx and that of the ear. Irritations acting upon the buccal mucous membrane, such as bad teeth and the like, are not to be forgotten; but the most effective source of irritation by far is the mucous membrane of the pharynx, whither irritants are carried by atmospheric connection. The nasal and pharyngeal passages are the gates of the lungs, and over them the air passes, depositing its particles as it enters. When the particles which the air contains are made visible to us, as by solar or electric ray, we realize the incessant deposit which must ever be gathering upon those surfaces, and the incessant labour of removal which thus falls upon the intercepting membrane. By a beautiful automatism we know that these membranes are constantly passing this silt outwards in an endless stream. Could this perennial stream be made as clear to us as are the motes of the air, in Professor Tyndall's well-known experiment, we

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should likewise realize the danger of its slackness or arrest, and with our eyes we should discover the settlement and adhesion of its burden. Let there be a trifling delay or languor of the epithelial function, and particles mechanically, chemically or vitally irritant, will imbed themselves on the soft and absorbent vesture of the throat, and thence may themselves be carried within, or may set up such action on the surface as to generate materials which, harmful or harmless, find their way into the lymphatics. It seems likely that of all such, those septic particles which infest the lower terrestrial atmospheres are the most potent. By some vital mechanics a chill to the skin disturbs the nutrition of the pharyngeal epithelium. If this occurs in aseptic air, as on the ocean, for instance, we notice that the sore throat is simple and transient. The effective cause having ceased, the parts regain equilibrium. But most of us breathe air far from pure; on the contrary, we inhale it often highly charged with septic particles, organized or organic. No longer arrested and rejected by the outward current, these settle and work mischief. The mucous membrane may still overpower the evil, as strong turnips outgrow the fly; or it may fail to do so. If it fail, the failure is to be attributed either to that bad healing power, which seems to be the chief feature of scrofula, or to the virulence or continuance of the incident poison. The same argument must be carried forwards likewise to the glands. If the poison, or morbid juices generated by the poison, reach these, they may throw off the mischief, either in virtue of their vigorous health, or because the poison is of feeble nature, of short continuance, or in limited quantity. Where the inherent weakness of the tissues is chiefly to blame, we have natural scrofula, heredity and local causes going hand in hand for evil; where the tissues are overpowered by the dose, virulence or continuance of the poisonous process in the peripheral membrane, we have artificial scrofula. It needs not to be said that in equal degrees the latter is far the more curable malady.

Unfortunately, however, after the original local cause may have ceased to operate, or, indeed, when the throat or other outer part is clean again, secondary mischief may abide within, and become itself a new source of centrifugal mischief. A healthy girl, let us say, is exposed to foul air. The first sore throat she chances to have stops the mucous current, and the poison settles on the injured surface. If she be continually exposed thus to the aërial poison, she will probably have a continual sore throat, and this will set up secondary bubonic disturbance in the neck. This will happen more or less readily, according to the age and disposition of the sufferer.

Or if, indeed, the constitution be sound, suppose that a small gland, the size, say, of a pea, is damaged beyond recovery. Such a gland, undergoing retrogressive change, becomes in its turn the source of scrofula, and the patient after some interval comes with a diseased neck to the doctor. Sinuses have now probably formed, connecting the small diseased glands with others suffering in their turn; these, again, communicate with diffuse suppurative gatherings under the skin, or surround themselves with dense fibrous tumefactions.

Of this kind was one of the cases upon my list. Miss B. had always been a very healthy girl, and inherited a sound habit of body. Nevertheless, her neck had become greatly disfigured by scrofula, the main mischief being found on surgical dissection to be secondary to a small deep-seated caseous gland under the jaw, and connected with it by long dense-walled sinuses. This gland had decayed very slowly, and the malady had thus gradually declared itself.

The lesson we learn from this and like cases is, that as scrofula is very often due chiefly to local causes, and in a large proportion of cases to septic poisoning of the pharynx, so is a local therapeusis indicated.

If it should be that the pharynx is still exposed to fouled air, or that a bad tooth, an

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aural inflammation, or the like, is present as a peripheral irritant, it is clear that such an agency, being sought out and discovered, must in the first place be eliminated.

This done, and the parts reduced to their simplest terms, the future line of treatment must comprise something far more than a mere temporizing with cod liver oil and iodide of iron—more, indeed, than a mere nicking of soft subcutaneous abscesses, or a mere aspiration of such inner collections of matter as will run through the needle. By or through such procedures, no doubt, most cases do make some kind of ultimate recovery, but few do really well; few of the serious cases fail to be deeply marked in the neck, few or none recover quickly, and few save the primitive strength of their constitutions. Dragging on a chequered and tedious course, drifting, perhaps, from doctor to doctor, consuming volumes of cod liver oil and medicines, breaking up life and prospects by prolonged exile from home, pestered by filthy discharges, or poisoned by decay which is not discharged, disfigured by sinuses, sluggish streaks and lumps of fibrous increase, seamy scars and indurated gland-remnants, such patients, thanks to the marvellous pertinacity of life, do generally fight their way into complete or partial recovery, but at the price of permanent disfigurement, at the price of tell-tale corrugations in the neck; at the price, perhaps, of a deferred pulmonary phthisis, set up by absorption of the partially voided caseous products. I ask you, gentlemen, whether this be an overdrawn picture? You will admit that it is not, and you will agree with me that scrofula as it now is, is an *opprobrium medicinæ.*

Is it not idle work to let a case drift in this fashion, and then to hasten to find learned names for the consequences?

Modern medicine says that where decay is, there, if possible, must drainage be carried. This is as true of the dwelling-place of the soul as of the dwelling-place of the body. Internal and external sanitation are one. I was fortunate in finding a colleague who took the same theoretical views of this subject as myself, and who acted upon them with boldness, decision, and skill (Mr. Teale).

We agreed in these cases that our first duty was to inquire into local causation, to examine the surfaces, and to investigate the outer conditions of life. Having secured outer and superficial purity, we next had to secure internal cleanliness. Mr. Teale secured this by freely incising the affected parts, under anæsthetics, by letting out thus all pent up-liquids, and by scraping out and enucleating all pulpy and solid products of disease. The wound is then closed upon a drainage tube, so that adhesion of the parts without further accumulation is attained. The drainage tube is of course removed as soon as this end seems to be secured. It turned out, on the surgical dissection of a number of cases, that the superficial softening abscesses of the neck so commonly handled in these cases, are not glandular, but subcutaneous abscesses, secondary to diseased glands, and communicating with these, so that the emptying of these superficial abscesses is no removal of the real and underlying mischief. It also turned out that the diseased glands and masses of decay lent themselves more readily than we had imagined to the process of enucleation.

The results of the operations have at least equalled our most sanguine hopes, and I lay the cases now before this Congress, withholding none, and adding the latest reports we have been able to obtain from the patients themselves. I must here express, on behalf of Mr. Teale and myself, my best thanks to Mr. Hartley of Leeds, who has prepared the schedule of cases. He has sought up the records with great industry, and his notes are the more valuable as he assisted Mr. Teale in many of the operations.

I believe from these cases we are justified in drawing the following conclusions:—

1.—That scrofulous neck, and glandular scrofula elsewhere, owes its origin to local causes, with or without the co-operation of hereditary tendency.

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2.—That by efficient sanitation such maladies may be prevented, even in the most susceptible.

3.—That scrofula, when established, even in severe forms, lends itself easily to radical methods of cure.

4.—That, especially by surgical dissection, the disease may be promptly removed, and the patient cured in a few weeks.

5.—That by this method disfigurement, if not wholly averted, is reduced to the least amount.

6.—That the subsequent health is, after such procedure, re-established in a way that is not possible under any other mode of cure, and that the risk of future phthisis or other malady caused by septic absorption is averted.

SELECTION FROM APPENDIX OF CASES.

Name, &c.	Duration.	Family and personal predispositions.	Causes—general and hygienic.	Previous history and treatment.	Present treatment.	Result as to— *a*, how long in healing. *b*, condition of scar. *c*, enlargement of other glands. *d*, effect on general health.
Miss Heaps æt. 24.	...	...	...	...	*First operation*, Feb. 6, 1881. Two incisions, A and B. At A, one large gland easily enucleated: semi-purulent fluid in centre. At B, four caseous glands scooped out, with much broken-down material; others felt, but were too deep to be removed without a fresh incision. Tissue round both places much thickened.	SECOND OPERATION. April, 1881.—Fresh softening gland near site of former incision B; this was reopened and gland scooped out. At the same time another large gland immediately above the clavicle was enucleated.
Mrs. W. æt. 27. May 17, 1881.	4 years.	No positive inherent tendency to disease, but very marked threatening of "artificial struma," from the long continuance of discharging sinuses in neck.	Glands began to swell after attacks of "sore throat" following confinement.	Swelling in neck "cut" by doctor 2 years ago, discharging ever since. The site of this incision presents an unhealthy surface of flabby granulations, constantly discharging. During last 3 months some glands (superficial) have swelled. Treated by poultices, ointments, cod-oil, tonics, &c.	Exuberant granulations snipped off with scissors and surface well-scraped: also sinus burrowing under surface. Two recently enlarged glands excised and cavity scraped.	*a*. (July 1881.) Quite healed (2 months). *b*. Scar quite sound. Extensive cicatrix where granulating surface was. *c*. No other glands at present show signs of swelling. *d*. General health "already better than it has been for years."
Miss H. æt. 17. May 5, 1881.	8 mons.	No bad family or personal history. Chronic ill-health last 6 months.	Came on after measles.	Glands first formed hard swelling, then superficial suppuration, opened by doctor; following this, extensive subcutaneous ulceration, with enlarging proximate glands, and burrowing discharging sinuses. Treated by poultices, oil, medicine, &c.	Sinuses well scraped; some remnants of caseating glands excised.	*a*. Quite healed in 6 weeks. *b*. Scar sound, but at present red. *c*. No further swelling. *d*. "Much better in health since operation."

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Name, &c	Duration.	Family and personal predispositions.	Causes—general and hygienic.	Previous history and treatment.	Present treatment.	Result as to— *a*, how long in healing. *b*, condition of scar. *c*, enlargement of other glands. *d*, effect on general health.
Andrew J. æt. 10. April 10, 1879.	18 mons.	No faults of family history. Threatened "artificial struma" since glands have been enlarged.	Glands began to swell after measles. Three sets of enlarged gland. Have been treated with iodine, medicine, &c.— A. Right side—near angle of jaw—threatening suppuration. B. In median line, under jaw. C. Left side, at border of sterno-mastoid.	...	A. Superficial abscess opened, and sinus followed to cavity of suppurating glands; thoroughly scraped, &c. B. Second incision—five caseating glands dissected out here; size from walnut downwards. C. Operation similar to A (supra).	*a*. A. Quite healed in 7 weeks. B. Quite healed in 3 weeks. C. Quite healed in 6 weeks. *b*. Scar (July 1881). A. Slightly raised white scar. B. Fine white linear scar. C. Scar nearly white—a little puckered at one point. *c*. No subsequent enlargement of others. *d*. General health much better since operation and perfectly good now.
Annie L. May 16, 1878.	5 years.	...	"Swelling" in neck for nearly 5 years: of late it has seemed much more prominent and softer. No cause made out: supposed to be "delicate" in health. Treated with external and internal medicines.	...	Incision—creamy pus evacuated. Small opening in fascia leading to interior of gritty calcareous softening gland. mass; well scraped.	*a*. Healed "within 2 months." *b*. (July 1881.) No scar visible except on close inspection. *c*. No other glands enlarged. *d*. General health excellent since operation. Is now a robust, perfectly healthy girl.
A. Harrowby, æt. 10. Aug. 19, 1878.	2 years.	Tendency to chronic sore throats, and always out of health.	...	Superficial abscess twice previously incised, but never healed: discharging ever since.	Opening in fascia enlarged, leading down to remnants of softened and calcareous gland beneath sterno-mastoid.	*a*. Entirely healed in 35 days. *b*. (July 1881.) Scar scarcely visible, with a very slight pucker. *c*. No further swelling. *d*. General health "very materially improved."
Miss C. æt. 9. Dec. 1880.	5 mons.	No special inherent tendency to disease, but ill-health, with gland-swelling in neck, evoked by living in badly-drained house with unventilated cesspool, &c.	...	First, hard swelling; lately, with formation of superficial abscess. Treated at first with Pig. Iodin. and medicine.	Suppurating cavity incised; sloughy material, size of walnut, scooped out; then hole in fascia found leading down to remnant of degenerated gland, which was excised and scraped.	*a*. Entirely healed in 5 weeks. *b*. Scar linear, and July 1881) commencing to fade. *c*. No other glands enlarged. *d*. General health now excellent.

DISCUSSION.

Dr. Bowles, Folkestone: Notwithstanding the graphic description of the causes of this disease by Dr. C. Allbutt, they are at present theoretical only; extremely probable, no doubt, but not proven. We await more evidence. I wish to draw attention to a class of cases of so-called scrofulous glands, which rise rapidly in anæmic, delicate subjects, and which are wholly unaffected by the usual tonic treatment, and are immediately relieved and cured by active saline aperients, followed by aperient salines and iron, and leave no trace behind.

Dr. Ord, London: The paper marks an important step in diagnosis and treatment.

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For years I have refused to regard the enlargement of lymphatic glands in the neck as simply indicating a particular constitutional weakness, and as conditions to be treated by strengthening remedies, and the local application of iodine only. I have been able mostly to recognize, on the contrary, that these enlargements are related with some irritation or morbid state of surface, in mouth, throat, pharynx, œsophagus, ear, or skin; and treatment to be successful must always commence with the attack of such local states. So far I have gone with Dr. Allbutt. But the practice which he has adopted, in conjunction with his able colleague, Mr. Teale, goes far beyond mine; goes, as I believe, in a direction justified by the pathology and clinical history of these cases, and is a contribution of the very highest value to this Congress.

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Dr. T. D. Griffiths, Swansea: I am much interested in the subject of Dr. Allbutt's paper. Some nine months since, at a local meeting of my confrères at Swansea, I ventured to express identically the same view as Dr. Allbutt has just done here, on the local causation and development of scrofulous glands in the neck. After considering the primary sources of irritation in decayed teeth, in the mucous membrane of the mouth, pharynx, and nares, I also pointed out that certain diseases of the ear, and eruptions on the face and scalp, were frequently observed as the primary irritation in the causation of scrofulous glands of the neck. I related three interesting cases of the so-called scrofulous glands in the neck, distinctly and obviously traceable to wearing earrings—irritation of the lobe of the ear, followed by bubonic swelling of the glands below. The first case—a young woman, ended in scrofulous phthisis. The second had a chain of diseased glands of the neck below the inflamed lobe, extending to the clavicle. She persisted in wearing the earring until the glands in the axilla became affected; she then discontinued wearing the ornament. The primary irritation being removed, the lobe of the ear healed, and the morbid action in the gland ceased. The third case was that of a woman, aged forty-five, similar in every respect to the last, with the exception that a large abscess formed in the axilla. On this occasion I also called attention to similar sequences in the causation of scrofulous glands in the mesentery (tabes mesenterica). There is no doubt this has its origin, as a rule, in local irritation of the mucous membrane of the intestines. The child is badly clothed, badly fed, and, perhaps, badly housed. He is "out of sorts"—suffers, perhaps, in the first place, from constipation; he is dosed with various drugs; he has diarrhœa and sickness, green stools, &c.; no improvement is made in the diet; irritation of mucous membrane of the intestines is kept up; this is followed by irritation of the mesenteric glands—the result is tabes mesenterica. No doubt the same law is observed in the causation of scrofulous glands in the mediastinum. The primary source of irritation may be in the pleura, mucous membrane of the bronchial tubes, or even in the blood-vessels. Not long since, I was able to trace most distinctly, the source of the primary irritation of a suppurating gland at the base of the heart to an atheromatous ulcer in the aorta, in the body of a patient who had died from aortic regurgitant disease It may, I think, be taken as a rule, that whenever any set of glands are found scrofulous, they are bubonic in character—due to some local irritation in the neighbourhood. Although local irritation is the main factor, it should not be forgotten that hereditary tendency also plays an important part, in some cases, in the development of these glands. Local irritation and vitiated nutrition (or a low state of health—call it by whatever name you like) are often aided by hereditary tendency. The hereditary predisposition may be due to a development of local irritation on the skin, mucous membrane, or elsewhere; or, it may be, a tendency to the development of scrofulous glands.

Mr. Frederick Treves, London: I would agree with Dr. Allbutt that a spontaneous

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origin for scrofulous gland-tumours is extremely rare, if not doubtful. In nearly every instance it is possible to make out some lesion at the periphery. Such lesion acts as the exciting cause only; and I would strongly oppose Dr. Allbutt's view that scrofula may be due to local causes, and local causes only. In every case I believe there is a tissue defect, a peculiar vulnerability of the part, a tendency for the glandular apparatus, as well as other structures, to react upon the most trifling irritation. This tissue defect may be either inherited or acquired. In no perfectly healthy person could the gland manifestations of scrofula be artificially produced. The exciting causes of these cervical gland tumours that are most effective, are those that involve the adenoid tissue of a mucous membrane. Inflammation of the adenoid masses in the pharynx will cause almost immediate gland enlargement, but an eczema on the face may exist for a long time before it produces such a result. The commonest seats for scrofulous gland tumours are the neck, the bronchial region, and the mesentery; and it is significant that the glands in those regions receive lymph from the largest districts of adenoid tissue in the body —viz., the naso-pharyngeal mucous membrane, the lungs, and the lining of the intestine. It is important to recognize the fact that the gland mischief extends locally, and that one gland can infect its neighbour by means of the lymphatic vessels that connect them. This I have elsewhere demonstrated microscopically. I fully agree with Dr. Allbutt as to the treatment of certain of these glands by operation. The treatment by excision is applicable to only a few cases, to glands that are few in number, and not yet adherent. The treatment by scooping out the contents of the gland has been extensively tried in London, but is apt to lead to some undermining of the skin and troublesome sinuses. The treatment I would advise is that by the actual cautery. The point of a thermo-cautery is thrust into the middle of the gland in one or more directions. Through the sinus thus established, the degenerate matter of the gland is gradually discharged. I have practised this method in some twenty cases with very good results.

Dr. Allbutt, Leeds: The cases in which hereditary antecedents were unfound are becoming too numerous to be explained by mere imperfection of record. I think that in some persons there seems to exist scarcely any liability to secondary lymphatic enlargements. In others we find the scrofulous habit; while another class of persons, without having scrofulous characters, nevertheless have very susceptible lymphatic glands. Of the pathological anatomy of the subject I have not to speak to day, interesting as that subject undoubtedly is, and the difficulties of the surgery of the cases have been dealt with by Mr. Teale.

Du Rétrécissement Congénital de l'Intestin Grêle.

Dr. Gibert, Havre.

Les malformations de l'intestin grêle ne sont pas connues. Je n'ai trouvé dans aucun recueil français ou étranger même une allusion à la possibilité des rétrécissements que j'ai observés et que je vais décrire. On s'est beaucoup occupé, sans doute, des nombreuses variétés de malformations du rectum et de l'anus—mais c'est tout. On n'a pas songé à la possibilité que l'intestin grêle peut subir, pendant la vie fœtale, l'influence de malformations; et l'on a certainement rapporté à des causes fausses une certaine quantité de faits cliniques qui ne peuvent trouver d'explications plausibles que par le fait de ces malformations.

Je vais donner un résumé rapide, mais suffisamment complet, des trois observations que je possède, toutes trois ayant la sanction d'autopsies minutieusement faites.

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Obs. I. *Enfant d'un mois—digestions troublées—puis empêchée—signes d'obstruction intestinale—puis de péritonite—mort. A l'autopsie rétrécissement considérable de l'intestin grêle en forme de chapelet.*

En 1863 je fus appelé en consultation par le Dr. Tarral du Havre pour voir une enfant âgée d'un mois qui était atteinte depuis sa naissance de phénomènes insolites dont le diagnostic ne se rapportait à aucune maladie connue. Cette enfant était très bien constituée, avait une bonne nourrice et tétait avec avidité. La digestion stomacale se faisait bien; mais peu de temps après l'ingestion du lait de sa nourrice elle était prise au milieu de son sommeil d'une agitation continue; elle relevait les jambes vers l'abdomen puis elle poussait des cris plaintifs qui ne cessaient que par l'expulsion d'une garde-robe.

Ces cris pendant la période de la seconde digestion avaient, naturellement, attiré l'attention sur l'état de l'abdomen qui n'avait jamais présenté rien d'anormal. Mais au bout d'un mois les cris au lieu d'être périodiques devinrent continus; le ventre était sensible à la pression et ballonné; c'est alors que je la vis avec le Dr. Tarral et je constatai les signes d'un entéro-péritonite manifeste.

L'enfant mourut quelques jours après, ayant présenté les vomissements, la face grippée, le pouls misérable qui précédent la mort dans la péritonite. A l'autopsie nous trouvâmes un épanchement séro-purulent dans la cavité abdominale, mais limité à quelques anses intestinales; celles-ci furent detachées avec soin et nous vîmes alors que l'intestin grêle présentait deux étranglements, à peu de distance l'un de l'autre. Entre les deux points rétrécis l'intestin reprenait sa forme et son volume.

Après avoir lavé l'intestin avec soin nous pratiquâmes l'insufflation et les deux étranglements devinrent aussi manifestes que les représente le dessin suivant.* Le rétrécissement, comme on peut le voir, occupe toute la circonférence de l'intestin, les trois tuniques participant à la malformation.

Tous les phénomènes observés pendant la vie s'expliquaient facilement. Quand un grumeau de lait se présentait à l'orifice d'un des points rétrécis, étranglés, il ne pouvait passer librement, et jouait le rôle d'un bouchon. Passait-il, par les efforts de contraction de la tunique musculaire, d'un rétrécissement à l'autre, puis enfin dans le courant intestinal, les symptômes douloureux cessaient et la digestion reprenait son cours. La succession de ces efforts de l'intestin pour amener la régularité du cours des matières avait fini par produire de l'entérite puis de l'entéro-péritonite qui avait enlevé l'enfant.

Obs. II. *Enfant né à terme—bien conformé—difficulté extrême de la digestion intestinale—mort au bout de huit jours—Rétrécissement d'une partie étendue de l'intestin grêle.*

Deux ans après avoir observé le 1er cas, je me trouvai en présence d'un enfant du séxe masculin, qui tetait très-bien, mais qui dès le premier jour fit entendre des cris plaintifs pendant tout le temps que durait la digestion. Le méconium avait, cependant, fait place à des selles jaunes bien digérées. Les cris devinrent continus, les selles rares, et l'enfant succomba le huitième jour, après avoir présenté une éruption confluante de petites tâches rouges, papuleuses, couvrant presque tout le corps.

A l'autopsie, je trouvai un rétrécissement de l'intestin grêle, dans le milieu du trajet de l'iléum, et interessant l'intestin dans une étendue de huit centimètres. Dans cette partie rétrécie l'intestin avait à peine le calibre d'une plume à écrire, et je le trouvai rempli de grumeaux de lait qui formaient une obstruction intestinale complète.

* The drawing was exhibited, but has not been engraved.

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L'éruption papuleuse du dernier jour avait été produite par cette retention complète des matières fécales et l'absorption de produits intestinaux non digérés.

Obs. III. *Enfant bien constituée—ayant vécu six mois—en ayant présenté pendant tout le temps de sa vie des symptômes de troubles digestifs, qu'on put rapporter à un rétrécissement de l'intestin—convulsions et mort au 6me mois. A l'autopsie rétrécissement très étendu du petit intestin.*

La 3me observation présente cet intérêt que le diagnostic de la lésion put être fait pendant la vie. Cette petite fille avait, en naissant, le poids de 9 livres, c'était une superbe enfant, pleine de vie et de santé. Elle était nourrie par sa mère.

Pendant le premier mois on n'observa aucun trouble dans sa santé—elle tétait bien, digérait bien, et accroissait de poids d'une façon régulière. Dans le courant du 2me mois les troubles digestifs commencèrent. Comme les deux premiers enfants, elle faisait entendre des cris plaintifs; en même temps elle faisait des efforts d'expulsion manifestes, et ne cessait de s'agiter jusqu'à ce qu'une garde-robe fut expulsée. Subitement alors, elle s'endormait d'un profond sommeil jusqu'à ce qu'ayant tété de nouveau elle présenta une heure après avoir quitté le sein les mêmes cris suivis des mêmes efforts, jusqu'à ce qu'une nouvelle garde-robe fut expulsée.

Les garde-robes étaient normales, souvent vertes et toujours mélangées de quelques grumeaux de caséum.

Parfois il arrivait que l'enfant passait 8, 10, 12 jours sans présenter aucun accident morbide—dès que les cris réapparaissaient on était sûr d'en trouver la cause dans la présence de grumeaux de lait non digérés.

La régularité avec laquelle se présentaient et les crises de coliques et leur cessation, rendait facile le diagnostic. Il était évident qu'une partie de l'intestin grêle était rétrécie et que le trajet rétréci, la filière, une fois franchie tout rentrait dans l'ordre.

Vers le 4me mois les crises de colique au lieu d'être espacées devinrent quotidiennes. L'enfant pleurait, criait, s'agitait pendant tout le temps de la digestion. Un phénomène curieux se produisit alors; on trouva assez souvent du sang dans les garde-robes—sang pur, brillant, tâchant les linges comme si l'enfant avait eu une petite plaie subitement ouverte. Je découvris bientôt qu'il s'agissait d'un polype rectal, que les efforts persistants de l'enfant pour expulser ses garde-robes avaient produit, après avoir congestionné, la muqueuse rectale. A ces cris presque continus se joignirent bientôt de petits mouvements convulsifs, puis de vraies convulsions cloniques qui accompagnaient le travail digestif. L'enfant s'affaiblit peu à peu, et mourut après le 6me mois.

L'autopsie faite en présence des Drs. Louis Monod, de Paris, et Lafaurie, du Havre, nous présenta une altération singulière de l'intestin grêle. Toute la première moitié de l'intestin grêle avait son calibre normal, mais la seconde moitié avait un calibre différent, presque de moitié moindre que la première, tout en présentant une régularité parfaite. Nulle part de collet rétréci, d'étranglement, comme dans les deux premiers cas, mais une continuité parfaite dans le calibre réduit comme s'il s'était agi d'un intestin parfaitement sain mais appartenant à une autre espèce animale.

Il résultait pour nous de l'inspection de ces deux parties de l'intestin que l'enfant aurait pu vivre si celle qui était rétrécie s'était développée au lieu de rester étroite.

Conclusion.—L'intestin grêle comme le rectum peut subir l'effet des causes qui produisent les malformations congénitales. Il peut être rétréci sur plusieurs points de son trajet, présenter des étranglements, ou une filière allongée et compatible dans une certaine mesure avec les actes normaux de la vie. Il résulte également de cette constatation qu'un grand nombre de faits cliniques obscurs peuvent être expliqués d'une manière rationnelle.

Voilà, par exemple, un homme de 20 ans, qui succombe rapidement, quelquefois en

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24 en 36 heures, à un étranglement interne que rien ne faisait prévoir. Mais si l'on interroge avec soin les antécédents du malade on voit qu'il a toujours souffert de quelques désordres subits dans la digestion, qu'il a ressenti souvent des douleurs abdominales que rien n'expliquait, ni un écart de régime, ni une indigestion.

On fait l'autopsie, et l'on trouve un étranglement intestinal. Il s'agissait pour moi d'une malformation congénitale de l'intestin qui, à un moment donné, a opposé un obstacle invincible au cours des matières.

Plus facilement encore le rétrécissement congénital de l'intestin expliquera les cas d'invagination. On fait intervenir en effet pour expliquer l'invagination des phénomènes nerveux produisant une contraction spasmodique de l'intestin qui va s'invaginer. N'est-il pas plus rationnel d'admettre un rétrécissement relatif de l'intestin et des lors la possibilité mécanique du phénomène de l'invagination.

Quoiqu'il en soit de la valeur des dernières remarques, il n'en reste pas moins que nous devons porter notre attention sur les malformations. Quand elles seront recherchées par les chefs de service des hôpitaux d'enfants, on en trouvera certainement un nombre suffisant pour écrire leur histoire. Trois observations ne suffisent pas pour cela.

Typhoid Fever considered as Fæcal Intoxication.

Dr. Jules Guérin, Paris.

For a long time the deposits of human excrement were considered susceptible to contributing to the development of typhoid fever. This opinion, expressed with a certain character of generality, and, besides, deprived of any serious demonstration, had remained in conflict with other opinions of the same kind. Up to what point was it founded? In what measure, under what form, in what conditions, could the fæcal excretions have a share in producing typhoid fever? In order to elucidate, if not to solve, these various questions, the author of this work has given himself up to experiments on animals, and to clinical researches, which have led him to the following conclusions :—

1.—The specific diarrhœic matter of those affected with enteric fever, contains, at its exit from the body, toxic elements resulting from the fermentation of fæcal matters, retained and accumulated at the end of the small intestine, behind the ileo-cæcal valve.

2.—The organic lesions, considered heretofore as specific signs of typhoid fever, congestion and ulceration of the mucous membrane, alterations of the glands of Brunner, Peyer's patches, and mesenteric glands, are effects of the virulent and ulcerative action of typhoid matters on these parts; and the functional troubles, or general symptoms of the illness, are at the same time the result of the penetration of these same matters into the organism, and of the organic changes which they determine.

3.—The complications which present themselves in the course of typhoid fever, under the form of meningitis, pleurisy, pneumonia, and other marked affections, are only more distinct localizations of its poisonous principle, just as those illnesses which begin at the outset with typhoid symptoms, are in themselves nothing but primitive effects of fæcal poisoning.

4.—Typhoid poison engendered by fæcal fermentation, spreads itself incessantly abroad by all the excretory ways of the body, from whence the transmissibility of the illness, and the formation of seats of infection, susceptible of reproducing it in an endemic and epidemic form.

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Every one of these propositions has been the object of experiments, and of clinical and anatomo-pathological observations, stated in three memoirs read before the Academy of Science and Medicine, in Paris, in the years 1877 and 1878, with proofs to the point.

The Relations which exist between Gout and Rheumatism.

Mr. Jonathan Hutchinson, London.

I have been requested, Mr. President and Gentlemen, to make some remarks in introduction to a discussion upon the relations which exist between gout and rheumatism. I do so with much diffidence, for although the subject is one to which I have devoted great attention for many years, and upon which I have collected a large body of clinical and statistical evidence, it is also one which is beset with peculiar difficulties. These difficulties are, I am convinced, to a large extent caused by the vague meanings which we have attached to certain words in common use. In what I shall have to say, there will appear to be considerable dissent from the dicta of some of our best authorities on these subjects; but I may express my belief that if we could agree on the definitions of the words which we employ, and if those definitions were made on a natural scale, comprehensive, instead of being conventional and restricted, it would soon be found that the real differences are not great. Those who persist in going into market without having first agreed as to the value which shall attach to the different coins employed, must lay their account for quarrels afterwards, which, on a more reasonable plan, might have been easily avoided. I shall therefore trespass on your patience for a few minutes with an attempt to explain the sense in which I shall use certain of the more important terms with which we are concerned in this discussion. We will, if you please, understand by the term arthritic any malady which displays a tendency to affect temporarily many joints at once, or one joint by repeatedly recurrent attacks. The arthritic type of inflammation is peculiar, often sudden in onset, usually very painful, almost always showing a definite tendency to spontaneous remissions, and very rarely causing suppuration. All forms of rheumatism and all forms of gout are thus included under the common term arthritic. But we cannot limit the term to the joints, as its etymology might seem to require, but must allow it to apply also to certain affections of the muscles, fasciæ, tendons, and other fibrous structures, which have been proved to be dependent upon the same peculiar state of health. Let me here assert, in the strongest possible manner, that all arthritic tendencies are prone to be transmitted to offspring whilst they are at the same time capable of production *de novo* in the individual in connection with their special causes. In some cases inheritance takes a large share, and the influences which have been brought to bear upon the individual a small one; in others the reverse may be the case. Under the term rheumatism we include all arthritic maladies which are not proved to be gouty, and we recognize several important varieties, the chief an acute poly-arthroidal affection, termed rheumatic fever, and affecting by preference the larger joints, next a very persisting chronic and yet more general malady in which the smaller joints are also affected, and much crippling often ensues, and a form occurring most frequently in the aged, in which only one, or at most a few joints are affected, in which remarkable changes of a permanent and slowly advancing character are produced. The term "rheumatoid arthritis," has been often applied to the first of these two chronic forms, and those of osteo-arthritis, "arthritis deformans," "rheumatic gout," "senile arthritis" to the latter. The cases which are ordinarily spoken of as "chronic rheumatism," are not examples of any separate malady, but simply of various forms of one or other of those just mentioned. "Gonorrhœal rheumatism" is a term applicable to rheumatoid arthritis, or chronic rheumatism more or less general in extent, when it occurs in connection with urethral

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inflammation. We come now to the word, concerning which it is most likely that differences of opinion may be felt: I must protest at once against any attempt to limit the term gout to cases in which acute attacks of inflammation of the great toe occur. Podagra is an important and frequent sign of gout, and constitutes, when it occurs, a most valuable revealing symptom, being absolutely conclusive as to the nature of the disease, but it is by no means essential to gout, and indeed occurs perhaps in only a minority of cases. We must count as gouty all forms of joint inflammation, and all affections of fascia and allied structures which occur in association with the accumulation of lithate of soda in the blood or with a state of health whether in the individual or his near relations, in which there is a tendency to such accumulation. The proof of gout after death is the discovery of deposit of this salt in the tissues, and during life the same may be proved either by analysis of the blood, by examination of the urine, or by observation of the diet-peculiarities of the individual. Any person who is liable to pains in the joints as the consequence of taking malt liquor, or certain wines, is "gouty." The terms lithiasis and lithæmia denoting the occurrence of lithic acid in excess in the urine and blood respectively, although possibly nearly synonymous with gout, are not exactly so, since, if they occur without any tendency to local disturbance of nutrition, we are scarcely entitled to speak of their subjects as gouty. On the other hand, we certainly are not to wait for proof of the presence of these salts in the blood, far less of its deposit in the tissues, before we are entitled to use this term. Whatever can be proved to be essentially connected with a tendency in this direction, must rank as "gout," although there may be no tophi, no lithæmia, and no acute joint attacks. Much that occurs in connection with inherited gout in young people, and many forms of "quiet gout" in those who have both inherited and acquired it, is probably without any proved tendency to the accumulation of lithates. The gout process is partly due to defective assimilation, and partly to deficient excretion, and it is probably only when the kidneys are decidedly affected that any great tendency to the formation of tophi is witnessed. It is possible, indeed—perhaps, probable—that in some of the inherited forms neither digestion nor excretion are much impaired, and that the inheritance is of peculiarity of tissue. Whilst the voice of experience both professional and popular is unanimous as to the association of gout with special habits as to diet, it is equally so in the negative as to rheumatism. Every one knows the general rules as to eating and drinking in reference to gout, and there exists neither knowledge nor tradition nor prejudice in respect to any articles of food as likely to cause rheumatism. To put this in other words it may be said that whenever arthritis occurs in connection with food, we call it gout, and wherever such association is wholly absent, we name it rheumatism. But our knowledge as to the causation of the latter malady, although vague, goes much beyond the mere negation mentioned. We know that exposure to cold and damp, especially when combined, will cause it, and in consequence of such exposure it may occur suddenly to those who were previously in excellent health. The condition likely to cause ordinary catarrhal inflammation, in some persons, are precisely those likely to cause rheumatism in others. The effects of suddenly arrested perspiration—of "chill" the consequences of resting on wet grass or sleeping in damp sheets, are indeed so precisely parallel in the case of rheumatic fever, acute pleurisy, or pneumonia, or even nasal catarrh, that we might almost define a rheumatic person as one who is prone to catarrhal cold in his joints. Not only, however, do we recognize in exposure to wet and cold a common cause of rheumatic arthritis, but we know further that it may be produced by suppurative urethritis, by certain affections of the female generative organs,* and perhaps by dysentery. The inference which we are entitled to draw from these facts is, as it appears to me, that rheumatic arthritis in its various forms is mainly produced through the nervous system, and is in no way primarily connected with the state of the blood. It is in short a catarrhal neurosis, the exposure of some tract of skin or mucous

* See an able Lecture on this subject by Dr. Ord.

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membrane to cold or irritation acting as the incident excitor influence. This granted, we shall have no difficulty in admitting that the malady is likely to receive endless modification of character in connection with the age, temperament, and, above all, the state of the nervous system of those to whom it happens. Hence the liability of those who have been enfeebled by losses of blood, or otherwise, to become the subjects of persisting "rheumatoid arthritis," hence the peculiarities of arthritis deformans when it occurs in senile persons, and hence, perhaps, the very remarkable disorganizations seen in the joints of those in whom the nervous system has been partially destroyed, as in locomotor ataxy.

I have given to gout a twofold means of recognition, one applicable to the living and the other to the dead; but as yet, in respect to rheumatism, I have spoken only of the former. There exist, however, certain pathological conditions well known to characterize it, though none of them so definite as is the lithate of soda in gout. Many rheumatic inflammations clear away completely, and leave no trace, and this absolute disappearance proves their nature. Others, however, lead to ulceration of cartilage, to fibrous degeneration of cartilage, to eburnation of bone, to the growth of lips on the joint ends, to alterations in the form of the joint ends, and other changes sufficiently definite to be pathognomonic.

Having thus defined gout as a tendency to arthritis in connection with blood disorder, and rheumatism as a tendency to arthritis in connection with catarrhal nerve disturbances, we are now in a position to recur to our first question as to the mutual relationship between these maladies. The fundamental proposition which I have to submit to your consideration on this point is, that these two classes of causal influence are in no way antagonistic, but are, on the contrary, often met with together. The person who is liable to get his blood disordered when he drinks beer or strong wine, is also liable to catch cold in his joints when exposed to damp. This form of arthritis is a mixed one, in part rheumatism and in part gout, or on some occasions it may be one and in others the other. This proposition once admitted, all difficulties as to rheumatic gout and its diagnosis vanish. Most cases are mixed, and the problem for the physician in each instance is to ascertain the proportions of each, to assign to each its proper remedies. The term rheumatic gout is thus once and for all justified, completely justified; it is an exact expression of the pathological nature of the malady. Rheumatic gout, that is, a mixed disease, is probably far more common than pure gout, but infinitely less common than pure rheumatism. A rheumatic susceptibility is indeed well-nigh universal in greater or less degree, but the susceptibility to diet which we know as gout, is confined to a few, and is, perhaps, not easily produced in one generation. My proposition then, is, that the elements of rheumatic causation, and the elements of gout causation, can mix easily—let me say, for illustration, as easily as spirits and water, and in as various proportions. Although contrary to the doctrine of some of our best specialists, I believe that this creed is quite in conformity with the floating, and perhaps ill-defined, opinions of the bulk of the profession, and that so far from causing astonishment, it will be accepted by most as a truism. Let me next assert that it is probable that, in most cases, as met with in practice, the mixture is not a simple meeting of two different kinds of causal influence, but the modified result of such in former, perhaps in many, preceding generations. Hence the inseparable blending of the two which we so often witness, and hence the peculiar modifications of type of disease which we sometimes encounter. In explanation of these modifications, we must also bear in mind that not only may an inherited gout tendency be mixed with that of rheumatism, but that other forms of more or less definite diathetic proclivity, such as that of struma, that which gives liability to chilblains, may also in like manner modify the result. If we once admit the possibility of combinations, the rest is comparatively easy. It remains only for our industry and ingenuity to discover what the facts are as to admixture in each group of similar cases. What I am chiefly concerned to protest against is the habit of selecting a few well-marked and probably exaggerated cases, giving to them a special name, and then proceeding to describe and classify the disease

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so named as if it were essentially distinct and needed only acumen in diagnosis for its recognition. It is this method which has, I think, led to the perplexity which at present prevails as to the relationship between the different members of the great arthritic family. It is a vice of clinical study in general, but nowhere has it led to greater error than in inducing a denial of relationship between gout and rheumatism, and a rejection, as inappropriate, of the excellent designation, "rheumatic gout." Briefly to recapitulate, this, then, is my chief hypothesis, that there exists a state of tissue-health which is transmitable by inheritance, which involves liability to inflammations of joints and fibrous structures, and upon this arthritic diathesis, as a foundation, may be built up under the influence of special causes, a tendency to gout, rheumatism, or any one of their various modifications and combinations. So long as an arthritic person has sound digestion and healthy kidneys, so long will his rheumatic manifestations be free from any trace of gout, but once let him fail in these respects, and it is perhaps scarcely possible for him to have a rheumatic inflammation, which is not modified and made gouty by the previously existing peculiarity of his blood. In this hypothesis we see an explanation of the well-known fact that so many persons fail to develop gout who indulge in habits likely to produce it, and so many get it who have carefully regulated their lives with a view to its avoidance. The primary arthritic liability is a thing which, once inherited, a man cannot much modify by any precautions. I by no means go the length of suggesting that all gout when it attacks joints is merely gouty rheumatism, and I fear that some of my medical friends may think that I degrade the aristocratic malady more than enough when I suggest that after all it is in some sense a *parvenu*, a super-addition to a disease, which, although commonly stigmatized as plebeian, can in reality boast an ancestry far more ancient than its own. We have been engaged hitherto in the attempt to give a clear statement of general principles, and it is time that I should proceed to support my propositions by an appeal to facts. This I must do very briefly.

First, then, as to the reality of an arthritic disease, I appeal to the facts that various maladies of a cognate character, such as acute rheumatism in young persons, chronic rheumatoid arthritis in adults, and the senile forms in the old, are met with often in the same person, or in different members of the same family. With them also go lumbago, sciatica, recurring iritis, &c. The remarkable proneness of the children of the gouty to suffer from acute rheumatism did not escape the attention of the older observers, and it is equally a matter of general knowledge, that in families in which the males are attacked by gout, the females often suffer from chronic rheumatism, nodi digitorum, &c. It has been asserted that, whilst gout is well known to be hereditary, rheumatism, on the contrary, is not so. This I believe to be simply a blunder. By the careful compilation of statistics, I find the evidence of heredity to be nearly as strong in the one as in the other. From the investigation of the history of a large number of families in whom gout has occurred, I am justified in asserting that it is exceedingly rare not to find that many of the persons comprising them have suffered from maladies which could only count as rheumatism.

In support of the proposition that gout and rheumatism may mix in the individual, and constitute a true rheumatic gout, I appeal alike to clinical and pathological evidence. It is very rare to examine a patient who has been for long a sufferer from gout without finding some proof of rheumatism too, and very often a definite history of attacks which were distinguished as rheumatic. There is usually the same liability to influence from weather and seasons which we note in rheumatism. This feature is very conspicuous in gout, and it, with some others, have led Dr. Duckworth, in a recent ingenious and able article, to endeavour to support the hypothesis that gout is not so much a blood disease as a neurosis. From my point of view, Dr. Duckworth has simply taken cognition of the rheumatic element, which complicates almost all cases of gout.

Any one who has doubts as to the frequent co-existence of the maladies in the living has but to visit the wards of any one of our London workhouse infirmaries. He will there find plenty of patients in whom the existence of tophi proves gout and the

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alterations in the shape of the ends of bones, the outgrowth of lips in the femoral condyles, the grating on movement, and possibly chronic and persisting synovitis, prove rheumatism; the history of the case, and of the patient's family, fully supporting the diagnosis. In like manner, any one who doubts the co-existence of the pathological changes which characterise these two maladies, has but to examine for himself the specimens which are to be found in the museums of our London hospitals. They are not very numerous, but they are very conclusive. Thousands of specimens will be found showing rheumatism only, but of those which show gout (lithate deposits) there are but few which do not also show some traces of the changes characteristic of rheumatism.

There is yet another topic upon which a few words must be said. I refer to certain diseases which occur in the children of those who have had gout, but which, neither in their phenomena nor pathological results, offer us anything by which we could diagnose them as gouty. Some forms of general rheumatoid arthritis with crippling, come, I believe, into this category, and many obscure joint affections in young persons. There is no deposit of lithates, nor any lithiasis, and yet the disease is a direct consequence of descent from gouty parents. The disease which I shall prefer to take as proof and demonstration of what I refer to, is a peculiar form of iritis, which I described about two years ago. In this affection, the iris and vitreous body inflame, first in one eye and then again later in the other. The changes occur insidiously, and sometimes almost painlessly, and they result in destruction of the organ. I have collected about ten examples of this malady, and they almost, without an exception, occur in young persons, in whom the family history of gout is very strong. None of the patients had themselves had acute paroxysms of true gout, but several had presented slight symptoms. In two of them all the terminal joints of the fingers and toes were affected by a sort of rheumatoid arthritis, which resulted in destruction of the articulation; but which was, like the iritis, comparatively painless.* More cases occur, I believe, to those who, in conjunction with the inheritance of true gout, inherit also that peculiarity of tissue-health which gives proneness to chilblains, and the last joint arthritis is to be regarded as in part gouty, and in part a kind of articular chilblain. I have been somewhat detailed in my reference to these cases, because I claim for this group that it illustrates a general law. From actual examination of the structures implicated, I am in a position to assert that there are no lithate deposits present, and yet I hold that the clinical evidence proves the affection to be gouty. May we not believe that many other disturbances of nutrition, inducing such affections as neuralgia, sciatica, lumbago, iritis, in various modifications, also occur in connection with gouty dyspepsia, or with inherited gout, but without lithiuria, and with deposit of urates. If such patients were to live long enough, and to live sufficiently freely, and, above all, if disease of the kidney should supervene, then true paroxysmal gout would, and will probably, be developed. There is, however, an early stage of the diathesis—one which, let us note, many patients never pass; in which, although there is proclivity to many ailments, no production of chalk-stones ever takes place.

DISCUSSION.

Dr. Dyce Duckworth, London: Exception may well be taken to Mr. Hutchinson's opinion that uratic deposit is not always present in true gout. At present, it is held by the best authorities that this deposit is always found, in greater or less quantity, in some part of the body, in true gout. If this point is to be yielded, we should land in a great difficulty, and it would be almost impossible to recognize gout post-mortem. I am not prepared to say that Mr. Hutchinson is wrong in his opinion, and I regard the question as still open for careful investigation. I believe gout can often be recognized clinically, in

* A lecture on these cases was published in the *Lancet* for Jan 1873, and some further facts Fascic. XIII. of "Clinical Illustrations."

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cases that are apparently doubtful, by attention to the mode of the attack, and other features peculiar to gout alone. Certainly, gout is often met with during life, when no uratic deposits can be detected anywhere; and, as certainly, such depositions may occur in large amount without any of the phenomena of gout as commonly witnessed. I believe it to be an axiom in pathology that "coincidence of structural change does not imply identity of process" leading up to it, and that true gouty disease will produce severe arthritis, resembling in many particulars, but not in all, those caused by other forms of arthritis. I consider that hybrids are the exception, and not the rule, in Nature; and may refer to the extreme difficulty of securing, from all classes of patients, any reliable history of gout in their ancestors. I believe that careful clinical study will yield facts which would go far to prove the distinction between gout and rheumatism, and that dead-house research alone cannot, at present, be trusted to afford the most satisfactory evidence on this point. I must allow that a coalescence of gout and rheumatism is possible, and think it would be strange if such did not sometimes occur. The cases which prove this conjunction, as shown by museum specimens, are very few in number, are the exception and not the rule. The cases alluded to by Mr. Hutchinson had been under review by the late Dr. Fuller, and by him relegated to their several positions in pathology.

Dr. Garrod, London: I am of opinion that the views expressed by Mr. Hutchinson in his communication are somewhat retrograde in their nature; for, during the last half century, the great object of pathologists has been to differentiate diseases, which at first appeared somewhat allied—as typhus and typhoid fevers—and not to mass together several morbid affections, which, although exhibiting some external symptoms in common, are nevertheless pathologically distinct. For the last thirty years my investigations have been directed to the separation of gout, rheumatism, and rheumatoid arthritis, and I have yearly become more and more convinced that each of the three diseases has a separate pathology of its own. As far as gout is concerned, I feel convinced that urate of soda is invariably deposited in the affected tissues, and that such deposition is not the effect of the inflammation, but rather the cause; for when it takes place in structures little prone to take on inflammatory action, a large amount of deposition may accumulate without any very appreciable signs of inflammation. Mr. Hutchinson has stated that uratic deposits occurred in cases of rheumatoid arthritis, and in proof of this he brought forward examples of joints found in museums, of which the history is quite unknown; but I think this view erroneous; for I doubt, had the specimens been taken from museums in Scotland, and other countries where gout is little known, whether such deposits would then have been found; in fact, I am informed by Scotch physicians now present at the Congress that they are not found; although rheumatoid arthritis is very common in these localities. As far as skin diseases are concerned, I have found (as I have insisted in my communication to the Congress) that eczema is the accompaniment of gout, and not of rheumatism; and that psoriasis is not unfrequently observed in rheumatoid arthritis; facts pointing to the essential difference between these diseases. With regard to urethral rheumatism (gonorrhœal) I cannot look upon it as closely allied either to gout, true rheumatism, or rheumatoid arthritis; as it is always connected with the formation of pus, and often leads to serious injury to the joints from the formation of fibrinous adhesions, and occasionally from the presence of purulent matter; I think it is more allied to pyæmia. Lastly, as to the statement of Mr. Hutchinson, that the children and the female descendants of gouty parents are apt to suffer from rheumatoid arthritis, I fully agree with him on this point; but, from a careful examination of a great number of cases am convinced that rheumatoid arthritis is not more common in such subjects than in the offspring of other parents who have been weakened by disease of a different character.

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Mr. HUTCHINSON, London: The intricacy of the subject is, I think, well illustrated in the remarks which have been made in this discussion, for most of the criticisms upon what I have advanced seem to me to have been based on misconceptions of my meaning. One speaker, Dr. Maclagan, has reminded us that there is much rheumatism which is not caused by exposure to cold and wet. Now, in placing these influences in the prominent position which I did, I had not the least intention of excluding others. My argument is that rheumatism differs from gout in being of nerve origin and due to reflex disturbance of nutrition. Cold applied to the surface is only one of the incident excitor influences, and I think I expressly stated my belief that urethral irritation may, in those who are predisposed, induce a similar kind of disturbance, the result being gonorrhœal rheumatism. So also certain disturbances of the female generative system, a fact which has been well brought forward in a paper by Dr. Ord, to which I have already referred. After what I have just said, it is needless to add that I cannot join with Dr. Garrod in thinking gonorrhœal rheumatism akin to pyæmia. Nor does my experience agree with his, that this form of rheumatism is often attended by suppuration. I have seen a great deal of gonorrhœal rheumatism in all degrees of severity, and have scarcely ever seen suppuration. It appears to me to be a true rheumatism, and follows the usual course of that type of joint disease, and from statistics which I have carefully collected, I have no hesitation in believing that the predisposing cause of it usually is the inheritance of arthritic tendencies. Very often the subject of gonorrhœal rheumatism will give a family history of gout. Dr. Duckworth has misunderstood me very much if he thinks that I wish to disconnect the deposit of lithate of soda from my definition of gout. With Dr. Garrod and all recent authorities I accept lithiasis and the deposit of tophi as the final proof of gout, and as the conditions about which all our conceptions of the malady thus named must centre. I do believe, however, that there are conditions of disturbed health which we must count as gouty, in which the deposit of lithates plays no part. These are especially frequent in those who inherit the tendency, and in whom lithæmia has never risen to a point high enough to cause a gout paroxysm. Let me, however, again repeat, that this distinction between what we know as gout and rheumatism is for me the tendency to deposits of lithates in the former, and their absence in the latter. I do not know whether I understood Dr. Duckworth correctly when he seemed to think that the abundance of museum specimens which show the changes of chronic rheumatism, and none of them of gout, was a fact in opposition to my argument as to their frequent combination. It certainly is not so; for rheumatism is, according to my hypothesis, the basic diathesis to which in a small minority of cases gout is superadded. It is very common to encounter all forms of rheumatism without the slightest admixture of gout, the rarity is to find gout without rheumatism. It was in reference to this last point that I made my appeal to museum specimens. I have examined most of the museums, boh at home and abroad, in reference to this question, and know well that specimens illustrating rheumatic changes are to be found in abundance everywhere, and not from the human subject only, but also from the lower animals. Specimens showing gout—*i.e.*, deposit of lithates—are not numerous except in our own museums. Let me repeat, that most of them show some evidences of rheumatism also. Between Dr. Garrod and myself there is a difference which I fear is more than one of mere words or definitions, for he thinks that it is progress backwards to assert that rheumatism and gout are often mixed, and that rheumatic gout is a result of their mixture rather than a third and wholly distinct malady. He believes that the separation of these three maladies, gout, rheumatism, and rheumatoid arthritis is simply a matter of skill in diagnosis. From this belief, if I have correctly expressed it, I differ. Whilst I fully admit the convenience of separating temporarily these three maladies, and gladly acknowledge the great service which Dr. Garrod has done to medical science in describing their symptoms, I am yet concerned to deny that they are absolutely separate. On the contrary, I believe that they have important mutual relationships, and that the attempt to make a definite differential diagnosis in all cases results not unfrequently in a

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violence to pathological fact. I believe that in many cases of rheumatic gout, and a tendency to rheumatism, a blood peculiarity and a nerve-tissue peculiarity, co-exist in the same person, and modify each other. Further, I believe, that much of what is called rheumatoid arthritis (crippling rheumatism, in which both small and large joints are affected) is the result of inheritance of tendency to both maladies. Whether this creed is true progress, or, as Dr. Garrod thinks, a going backwards, a falling again into the confusion from which we had seemed to emerge, must depend upon whether or not it is true. The decision must be left to time, to the further accumulation of carefully sifted facts.

Eczema and Albuminuria in relation to Gout.

Dr. Alfred Baring Garrod, F.R.S., London.

Before entering upon the subject matter of the present communication, it is necessary to make one or two preliminary remarks.

In the first place, the conclusions arrived at are deduced from nearly 2,500 cases of the diseases under consideration; and appended to this paper are tables containing the more important details of at least 1,900 of them.* All the cases have been seen by myself, during a period of about twenty-two years, at my own house; and I have altogether excluded hospital cases, as well as those seen out of doors or in consultation.

In the second place, I may observe that the necessarily short time allowed for the reading of this paper compels me to avoid almost all discussion on details, however interesting, and to bring my conclusions before the medical section of this Congress in a somewhat dogmatic form.

I will at once proceed to the first part of the subject: "On the state of the system preceding the development of the phenomena which constitute gout."

Before any of the group of symptoms which are regarded as constituting gout appear, there is invariably present a peculiar state of the system, characterized by an altered condition of the blood; this fluid, which in health is practically free from uric or lithic acid, is then loaded with it; and it can be readily crystallized out, either as urate of soda —the form in which it exists in the blood—or as free uric acid. This fact, discovered in the summer of 1847, appeared in the "Transactions of the Medico-Chirurgical Society," 1848; and since this period I know of no observation which has thrown a doubt on the truth of my original statements. How this alteration of the blood is brought about—a question of great interest and importance—or what are the causes in operation during this "pre-gouty" stage, I purposely avoid discussing on the present occasion, satisfying myself with the simple announcement that a change in the blood, of the character above indicated, is invariably present prior to the manifestation of gouty symptoms.

The functional disturbances induced by this state of system are at times referable to the digestive organs, under the name of gouty dyspepsia, with acidity, heartburn, flatulence, and disturbance of the function of the liver; or the muscular and nervous systems may be implicated, and cramp, palpitations, asthma, headache, &c., produced.

The symptoms, attended by visible changes in the organs implicated, are referable to the joints, the kidneys, skin, and, less frequently, the throat, bladder, and testicles.

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* The tables above mentioned were presented to the Congress at the time when the paper was read, but are far too voluminous to be inserted in the Transactions, as they would have occupied at least 200 pages. The deductions from them are given in the paper.

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In the present communication, my principal object is to describe the peculiarities of the kidney disease so common in gouty subjects, and to show the form of skin affection which occurs in connection with, and as the result of, the gouty diathesis; and lastly, from the result of an analysis of a large number of cases, to indicate the frequency both of the renal and skin complication. There is one point about which we must be satisfied before discussing these subjects. Cannot this peculiar state of the blood be present without the occurrence of symptoms? I have no hesitation in answering this question in the affirmative, for in many cases of poisoning with lead I have found this to be so; when, however, this condition of blood is present, the individual is always on the brink of an attack of gout; and in two instances it has been my lot to predict a first fit some weeks before its occurrence, simply from an examination of the blood. It would appear, therefore, that something beyond the mere presence of urate of soda in the blood-serum is required in order that gouty symptoms should be manifested; it is necessary that there should likewise be a deposition of the same in some tissue, which gives rise to inflammatory action, varying in amount according to the susceptibility of the structure to take on such action. If, for example, deposition takes place for the first time in a joint such as the knee, the subsequent inflammation is usually very severe; if, on the other hand, it happens in the cartilages of the external ear, an accumulation of urate of soda may ensue, sufficient to form a small tophus, or chalk-stone, without the occurrence of any appreciable vascular action.

The reason why at one time the salt crystallizes out of the blood more readily than at another, must be sought for in some further alteration in this fluid beyond the accumulation of urate of soda; and, as a clinical fact, I may state that, in chronic gout accompanied with the free formation of chalk-stones, the serum has exhibited a marked diminution of alkalinity, compared with the healthy standard.

As the result of long-continued observation, I feel justified in asserting that the peculiar deposition which takes place in the various structures in gout—I say peculiar, because it is pathognomonic, never occurring in any other disease—is the cause, and not the effect, of the inflammation.

We next proceed to speak of—

1.—*Albuminuria: the cause of its occurrence in gout, and the peculiarity of the kidney disease.*—More than twenty years ago I made a statement to the effect, that in acute gout, especially in its early stages, albumen was seldom found in the urine; but that, later on, a small amount of this principle was frequently present, more particularly during the fit; the urine being quite free from albumen in the intervals; that in chronic gout the presence of albumen was much more common, but only in very small quantities, often requiring some little care in manipulating in order to detect it.

In the present paper I shall endeavour to test the accuracy of this statement, and to supplement it from the result of a somewhat lengthened clinical experience.

When the blood is in the condition we have above described, not only does it deposit its urate of soda in the different structures of the joints, giving rise to articular gout, but it also, not unfrequently, finds a vent for its impurity in the kidney; if the deposit takes place within the tubuli, it leads to the formation of gravel and rudimentary calculi; if in the tissue between the tubes, it leads to the production of albuminuria. The co-existence of gout and calculus has been recognized for centuries. Sydenham, who wrote so vivid a description of gout, was himself a martyr to both diseases; and the frequent occurrence of gravel in members of a gouty family is a fact for a long time fully established; but that form of kidney disease which leads to the production of true gouty albuminuria, although by no means unrecognized, has as yet had but little attention paid to it. The names of those who have made observations upon this subject

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are Castelnau, Charcot, and Cornil in France; and in this country, Todd, Ceeley, and myself.

In a few words, the most important and essential alterations in the kidney may be thus described. A section of the renal structure, cut parallel to the direction of the pyramids, exhibits streaks of perfectly white opaque matter, which, on microscopic examination, is found to be wholly, or in part, between the tubuli; that which is between consists entirely of crystals of urate of soda, often arranged in fan-shaped bundles; whilst that formed within the tubes themselves is for the most part amorphous, consisting of urates of ammonia and soda intermingled with rhombs of uric acid.

This peculiarity in the anatomical changes in the gouty kidney explains satisfactorily why albuminuria resulting from this condition differs from some other forms, not only in the presence of a very small amount of albumen, but in the absence of casts; but, at the same time, it must be remembered that urate of soda deposited in the renal tissue acts as a foreign body, and may, after a time, excite more or less further change in the condition of the kidney, and lead to disease closely resembling some of the more common forms of albuminuria. This peculiarity of the pathological change also explains why the albuminuria in the early stages is intermittent; the mere presence of the crystalline deposit may be insufficient, of itself, to cause the escape of albumen with the urine; but when congestion, or any inflammatory action, ensues around such deposit, albumen is at once thrown out.

In several instances I have discovered the presence of albumen in the urine some days before a fit of articular gout; this has continued during the attack, but has afterwards passed completely away. In such cases, if the urine is frequently examined, it is possible to predict an attack of gout many days before any of the ordinary symptoms manifest themselves; and as a fact, of the truth of which I have convinced myself by clinical observation, the continuous application of heat and moisture to the loins will often relieve an attack of gout more speedily than any other treatment.

In relation to this subject, I may observe that at the time when a gouty patient is flattering himself that his attacks are becoming less frequent, and his urine is assuming what to him seems a more healthy appearance, kidney mischief is often insidiously going on, and laying the foundation of much subsequent misery.

Another fact must not be lost sight of, which is, that albuminuria from any cause may be present in an individual for many years, and that subsequently gout may become established in his system; in such a case, the two diseases may have little or no relation to each other—they are simply co-existent. A peculiarity of gouty albuminuria is the slow development of the mischief in the majority of cases; in one instance, a man having this disease to a very moderate extent went to China and lived there six years; on his return, the analysis of his urine gave the same result, both as to specific gravity and quantity of albumen, as on the occasion of his previous visit. But the development of gouty albuminuria, though slow, is sure.

The last point in connection with this subject is to ascertain the frequency of the presence of albuminuria in gouty cases. For this purpose I will make an analysis of the cases; they are contained in forty volumes, of which I have made ten groups, four in each; they are arranged in order of time, extending from 1860 to 1881—in fact, up to the end of the last month, a period of nearly twenty-two years.

The results are as follows:—1st group: number of cases, 176; percentage of albuminuria = 42·6. 2nd group: number of cases, 176; albuminuria, 43·8 per cent. 3rd group: cases, 135; albuminuria, 33·8 per cent. 4th group: cases, 156; albuminuria, 27·0 per cent. 5th group: cases, 133; albuminuria, 30·8 per cent. 6th group: cases, 130; albuminuria, 30·7 per cent. 7th group: cases, 130; albuminuria, 17·0 per cent. 8th group: cases, 126,

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albuminuria, 22·2 per cent. 9th group: cases, 139; albuminuria, 15·0 per cent. 10th group: cases, 137; albuminuria, 20·4 per cent.

In examining these results, it will be seen that the percentage of cases in which albumen was present in the urine is greater in the earlier than in the later groups. The difference is not difficult to explain; in the first place, from the commencement there has been a gradual addition of observations, as the patients from time to time have come under observation; and many cases, which a few years since were free from albuminuria, have exhibited the symptoms later on; probably many cases in the later volumes, now free from albuminuria, will in the course of time become affected with this malady. In the second place, the proportion of severe cases is greater in the early groups, for at that time patients frequently did not come under my care till they had exhausted the resources of many experienced physicians. Taking all the cases under consideration, we find the percentage as high as 26·5, the number of cases being 1,449. After this, I think we cannot but look upon the kidney affection as a most important one in the history of gout.

The practical value of this fact is very great, both as to prognosis and treatment; and as a rule it will be found that gouty patients, who, as they advance in years, find the disease become less frequent and less severe, are those in whom the renal organs have preserved their integrity.

2.—*Eczema; the skin development of gout.*—There are both external and internal causes of eczema; the external are numerous, and generally easy to ascertain; but up to the present time little progress has been made in determining the nature of the internal causes; my object in the present paper is to endeavour to define one of these causes, and to show that the gouty diathesis is a very frequent cause of eczema, and that eczema is, in fact, the skin-development of gout.

A few words on our present knowledge of this subject, gleaned from the writings of dermatologists, may not be out of place.

Hebra, of Vienna, scarcely alludes to any such internal cause, merely stating that, in his opinion, it is needless to seek for any explanation beyond the skin and subcutaneous fascia. Sir Erasmus Wilson, the late Drs. Anthony Todd Thomson and Neligan, and Dr. Anderson, pass over the subject of the internal causes of eczema in comparative silence; the late Dr. Tilbury Fox referred to gout as a cause of eczema, and Dr. Liveing mentions a gouty, or quasi-gouty constitution, among the predisposing causes, considering that mal-assimilation and defective excretion may induce both diseases; but this last author speaks more of the connection between gout and psoriasis than between gout and eczema. Dr. L. A. Duhring, among American writers, makes this remark: "The presence of the gouty or rheumatic vice in some subjects strongly predisposes to attacks of eczema;" and of psoriasis he says: "The relationship is not, I think, of so close a nature as that which exists between these diseases and eczema."

It will be seen that both gout and rheumatism are looked upon by Duhring as equally the cause of the skin affection. Some French authors, as Hardy, do not allude to the subject; but others, especially Bazin, recognize a close relationship between cutaneous eruptions and certain forms of joint disease. A somewhat remarkable passage is contained in his work—a passage which bears on the present paper; it is as follows: "Que les arthritides procédant de la goutte sont humides, tandis que celles que procédent du rheumatism sont sèches." After comparing these statements from various authors, I think we may safely come to the conclusion that the discrepancies between them are great, and that nothing very definite in relation to the connection between eczema and gout has been clinically made out.

It is only within a comparatively few years that any attention has been particularly directed to the above relationship, but each year has strengthened my conviction that

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the two diseases are often most closely allied, and that when a cutaneous eruption is developed by the presence of a gouty diathesis, the form which it then assumes is that of eczema; of the frequency of the co-existence of the two maladies we shall shortly speak.

From a reference to a large number of my cases, I find that gouty eczema especially affects certain parts of the body, and probably the order of frequency is about the following:—The ears, on the external surface, and within the meatus; also behind the ears, and on the back of the neck; the eyelids and different parts of the face; the groins and internal surface of the flexures of other joints; the scrotum, gland of the penis and prepuce—in the latter it may alone exist, and is then apt to be overlooked. Eczema also occurs on the hands and feet, especially the dorsal surfaces of the fingers and toes, and between them. Eczema may likewise affect the arms and legs, and be scattered over different parts of the trunk.

A very troublesome symptom, pruritus ani, has long been recognized as of frequent occurrence in gouty subjects; and this affection usually partakes of the nature of eczema Eczema in gouty subjects, although at times largely diffused over the body, in the majority of cases is comparatively slight, and limited to a few localities; and this fact often causes it to be overlooked by the physician, as the patient may not mention its existence, not supposing it to be in any way connected with his other ailments. In some of its stages, the vesicular character of gouty eczema can almost always be detected, but it is apt to degenerate into a form of dry eczema.

A question may now be asked; in what relation does eczema stand to the joint affection? It often is present at the same time as the articular disease, and sometimes the intensity of the two alternates, one increasing and the other decreasing; at other times they run a simultaneous course. Not unfrequently, eczema is present many years before the joint disease manifests itself; and I have numerous records of patients suffering from skin disease, whose whole history points to its gouty character, but who have not yet suffered from joint complication. On the other hand, eczema often occurs late in a gouty life, even in extreme old age, when the fits of ordinary gout have become much less frequent and less severe.

During the course of gout, if extended over many years, eczema may come on, and last for weeks, months or years, and afterwards entirely disappear. Eczema, in its acute form, will occasionally occur in place of an ordinary attack of articular gout. Many instances illustrating this fact have come under my notice, and in some the serous discharge has been great. These patients have for many years had an ordinary attack in the spring. This year (possibly in connection with the excessive cold of the winter, and severe dry east winds of the spring) no joint attack has been experienced, but acute eczema appears to have taken its place.

In a gouty family, we occasionally find that the father has gout and eczema; one son, ordinary developed articular gout, and another son, severe eczema but no joint affection.

Women, especially if descended from gouty parents, will often, near the climacteric period, exhibit symptoms of eczema, but the joint manifestations may be delayed till after the complete cessation of the catamenia. Gouty eczema is usually more difficult to treat than ordinary joint gout, especially when it becomes chronic; but when acute and, above all, when it occurs in place of the joint disease, it usually yields readily to the ordinary constitutional treatment of the latter.

Before proceeding further with the subject, it will be well to say a few words about psoriasis, the more so as we have seen that much confusion as to the etiology of eczema and psoriasis exists in the minds of dermatologists. From my own experience, I have come to the conclusion that psoriasis is not connected with gout in the same way that

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eczema is, and that when the two diseases occur in the same subject, it may be looked upon as accidental. If, for example, a man has psoriasis in his youth, it is not impossible that gout may be developed in his system in after life; this, however, only shows that the presence of psoriasis does not prevent the occurrence of gout; the two diseases, in such an instance, may be in no other way associated with each other.

I can assert with some confidence that psoriasis is at times associated with rheumatoid arthritis. I have seen many instances in which the diseases have been co-existent; whether in the relation of cause and effect I should not at present wish to say, but certainly the successful treatment of the joint disease not unfrequently relieved the morbid condition of the skin.

Assuming that eczema is the skin disease which is produced by the presence of a gouty state of the system, we must next endeavour to explain why it should be so; why eczema, rather than psoriasis, should occur under these circumstances. In discussing this subject it is necessary to say a few words about the function of the skin and the character of its secretion. The perspiration in health has a well-marked acid reaction; this is more marked when the skin is acting slowly; in no instances have I found it alkaline, but occasionally almost neutral, under the influence of large doses of acetate of ammonia. The perspiration is free from uric acid, not only in health, but even when the blood is loaded with this principle. The perspiration obtained from a man suffering from acute gout, during the exhibition of a Turkish bath, failed to yield uric acid, but oxalate of lime and urea were found in it. The blood of the same man, taken at the time, was rich in uric acid.

Blister-serum has a strong alkaline reaction; when obtained from a healthy part of the skin in gouty subjects it yields uric acid, but less perfectly than the blood-serum; on the other hand, when taken from a part of the skin which is suffering from gouty inflammation, no uric acid can be obtained from it. From these observations it appears that inflammation exercises a destructive action upon uric acid, and that, even when slight, as in the case of a blister, still its influence is shown in the characters of the secreted fluid.

The statements seen in some works of the occurrence of a white efflorescence of urates on the skin of patients suffering from gout, have been made without sufficient grounds; earthy phosphates have usually been mistaken for urates.

We will now try to apply these facts, in order to explain why eczema rather than many other cutaneous eruptions should be the product of the gouty diathesis.

Eczema, we must bear in mind, is a vesicular disease, and the exudation of the inflamed surface, as far as my observations go, has a strong alkaline reaction, closely resembling the fluid obtained from a blister. In psoriasis, on the other hand, there is no alkaline exudation, as the disease consists of a hypertrophy of the epithelium cells of the skin; such being the case, it would naturally be supposed that if a cutaneous eruption occured in a gouty subject, as an eliminator of the gouty poison, it would be of such a character as to do this most effectually; and under such circumstances, we should expect it to be eczema, which is practically a collection of small blisters, rather than psoriasis, or other forms of dry cutaneous disease.*

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* I find in my note-book, 1849, the following entry appended to the results obtained from the internal exhibition of urate of soda: A gouty man to whom this salt was administered, experienced, while taking it, a great increase of irritation in some eczematous patches, which had been present for a long time; when the salt was omitted the irritation at once subsided. At that time I knew nothing of the relation between gout and eczema.

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Should we find uric acid in eczematous scales? The first answer might possibly be Yes; but on second thoughts we should come to the opposite conclusion; for the eczematous surface is a highly inflamed one, and we have seen that acute inflammation destroys this acid; and we know that even the slight inflammation which occurs in the production of a large blister-bleb much diminishes the amount of uric acid in the serum; how much more, then, must this acid be destroyed in the formation of the very numerous vesicles which accompany an eczematous eruption.

Lastly, we have to determine how frequently eczema is found in gouty subjects, or rather, as far as the accompanying cases illustrate, how often it is seen in those who have already experienced articular gout. For this purpose I will make the same grouping as before, and arrange in the same order of time. The results are as follows: In the 1st group, number of cases, 169; per cent. of eczema equal 10. In the 2nd group, cases = 204; per cent. of eczema = 11·8. In 3rd group, cases = 154; per cent. of eczema = 14·2. In the 4th group, cases = 171; per cent. of eczema = 15·2. In 5th group, cases = 194; eczema per cent. = 10·3. In 6th group, cases = 187; eczema per cent. = 17·0. In 7th group, cases = 156; eczema, per cent. = 17·3. In 8th group, cases = 187; eczema, per cent. = 15·5. In 9th group, cases = 238; eczema per cent. = 19·3. In 10th group, cases = 212; eczema, per cent. = 47·0.

The total number of cases in the ten groups is 1,890, and the percentage in which eczema is stated to have occurred is 18 per cent. Some peculiarities will be discovered in reviewing these numbers; in the first place, there is a gradual increase of the percentage as time advances; arranging the cases in two groups only, the percentage in the first is 12·2, in the second, 23·2; and, secondly, the increase becomes still larger if the last of the groups be compared with the rest. These differences can be accounted for—at least, in a great degree. My attention has only been called to the close connection between gout and eczema within the last few years; before this time, the skin disease was only noted when it was very patent; sufficiently developed to cause its being spoken of by the patients themselves; but lately—that is, for the last two years or so—I have made a definite inquiry as to the presence of eczema or other skin eruption in every case of gout, and by these means have frequently discovered its presence when otherwise it would have been overlooked. If we subdivide the last group, the percentage of eczema is found to be much greater in the latter half; and the cases therein contained have come under observation during the present year, and I cannot help thinking that there has been during that time an unusual development of eczema, possibly depending on some climatic cause. Under these circumstances, it would be hazardous for me to attempt to say what is the real percentage of eczema in gout; but I feel quite sure that it would be very high, probably over 30 per cent. in cases of gout which have been of long duration. In conclusion, I have only to express a hope that I have been successful in drawing your attention to the importance of the subjects treated of in this communication; and that I have not failed to show that in gout there is a great tendency to the development of both kidney and skin disease.

DISCUSSION.

Professor Stokvis, Amsterdam: I will allow myself a very few remarks upon the frequency of true gout in our country (Holland). Cases of true gout are exceedingly rare in our town (Amsterdam), and I only remember two cases of true gout in hospital practice. These cases were observed in poor men (labourers) who lived under very unhealthy conditions. There is no need to say that, as regards the diagnosis of gout, we on the Continent stay wholly on the basis which was laid for us by the beautiful

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investigations of Dr. Garrod. Now, in the classes of easy-living men, I never saw a case of joint disease, where the diagnosis of gout could be presumed, in which there was a deposition of uric acid in the joints, or in which the uric acid in the urine was diminished or was not to be found; so that, as far as respects our country, true gout is an exceedingly rare illness.

Dr. QUINLAN, Dublin: I wish to remark, in reference to what fell from the President, that in Dublin a great social revolution has of late years taken place with regard to the alcoholic beverage of the humble classes. When I was a student, the one employed by them was whisky; and, although a long time an *interne* in hospitals, I saw plenty of rheumatic fever, but not a single case of gout. For some years past, owing to fiscal changes, porter has been their universal ordinary drink, the much more expensive whisky being seldom used. Since this change gout has been common enough in the Dublin hospitals.

Dr. MACLAGAN, London: Gout, rheumatism, and rheumatoid arthritis, are to be regarded as distinct morbid conditions. In Scotland, where I practised for many years, it is rare to meet gout, but rheumatism and rheumatoid arthritis are common; but these occur under different circumstances, and yield to different treatment. The latter was noticed chiefly in young women suffering from uterine disorders, prolonged lactation, or other similar disturbing agency, and yielded only under treatment directed to the rectification of these depressing agencies. The urine in these cases was such as is usually noticed, not in gout, but in low and anæmic states of the system; it is pale in colour, generally of low specific gravity; and if there is any crystalline deposit, it generally consists of oxalates and phosphates. The action of wet and cold in the production of rheumatism is capable of over-estimation. So common is exposure to these agencies in those climates in which rheumatism most prevails, and at that age at which it is most common, that it would be wonderful, indeed, if they did not frequently co-exist. But care must be taken to distinguish between the *post et propter hoc*.

Dr. ROBERTS, Manchester, thought that in London gout was so common that it intervened, more or less frequently, in a large number of chronic ailments. In Manchester and the neighbourhood, where there is a large poor population, the three kinds of arthritic ailments, gout, rheumatism, and rheumatoid arthritis, appear very distinct.

Dr. GRANT, Ottawa: In offering a few remarks on rheumatism as observed in Canada, I may say that it is a disease not of common character, and is largely the result of a want of care in the proper use of flannel, combined with the too free use of alcohol. This disease is not by any means so common in our country as generally supposed. Gout, as a specific disease, is very rarely seen, and when a case arises it partakes more of the rheumatic gouty type. Pericarditis and endocarditis are common associates, particularly in such cases as those associated with debility of the system, the result of excess in the use of alcohol. We find that the cold season is best guarded against by the use of flannel and the absence of spirits. Salicylic acid, and salicylate of soda, are the chief agents at present adopted; and, as a general rule, the best safeguards against rheumatism are careful attention to warmth of clothing, and the regulation of diet, with the non-use of alcohol.

Dr. LONGHURST, London: I consider that, whilst the deposition of lithate of soda, as advanced by Dr. Garrod, may be considered to define what we mean by the expres-

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sion of gout, yet that, as demonstrated by Mr. Hutchinson, the two diseases may exist together. But I should attach importance to the influence of age, for while rheumatism occurs in people of all ages, gout is rarely seen until after the middle period of life, when important degenerative changes have taken place in the system, especially in the kidneys; and that it is often not until the actual fit of gout occurs that there may be any indication of such changes of a degenerative nature in the system.

Dr. Ord, London: In relation to Dr. Garrod's paper I have a few facts to state. Following up an observation which he had commenced some years ago, I obtained the co-operation of my friend, and, till recently, colleague, Dr. Greenfield, in noting the existence of uratic deposits in the joints, among the subjects submitted to post-mortem examination in St. Thomas's Hospital, and in noting, among other points, the presence or absence of renal disease in the subjects presenting the uratic deposits. The results may be briefly stated as follows:—1. The joints were examined in 135 adults. 2. Of these, 96 presented lesions of the kidneys. 3. Urate of soda deposits were found in the joints in 18 cases. Of these, 17, were males; their ages varied between 37 and 64 years. 4. The notes of the state of the kidneys had been lost in one case. In the remaining 17, diseases of the kidneys were found. The lesions were:—contracted granular kidney in 12; kidneys small (5 ounces), cortex atrophied, capsule separable, in 1; kidneys brawny, with cysts, and non-adherent capsule in 1; granular surface, with cysts in 1; kidneys very firm, cysts in cortex, which was not wasted, capsule readily separable, surface not granular (aortic disease) in 1; the left kidney only had the capsule very adherent at parts, and the surface very granular (hypertrophic cirrhosis of liver) in 1. In other words, in two-thirds of these hospital cases of gouty affection of the toe-joints there was a definite co-existence of contracted granular kidney, and in the remaining third there were affections of the kidney closely allied thereto. 5. Among the 96 cases of renal disease were 20 cases of contracted granular kidney, and 4 more cases almost deserving of the title. There were at least 8, and probably 9, cases of contracted granular kidney in which nouratic deposits were found in the joints. The 8 cases may be thus stated in abstract:—Extreme or typical granular contraction, 2; marked contracted granular, 2; slighter granular contraction, 3; granular contraction with acute nephritis (in man aged twenty-eight) 1.

Dr. C. A. Leale, New York, said the very valuable paper of Dr. Garrod—who has probably observed with great accuracy as many cases of gout as any living physician, and whose name is so well-known in America—presents so many points for consideration, that it would be impossible for me in the brief time to do justice to this theme. I will, therefore, with your permission, confine my remarks to the first of Dr. Garrod's problems, —viz., functional symptoms, or those arising from disturbances of different organs or systems of the body, but not accompanied with any known or visible alterations in the implicated parts. The very great importance of closely observing and treating these functional symptoms is not too forcibly illustrated by Dr. Rayner, when he, with his great experience, associates suppressed gout with symptoms of insanity—a combination of symptoms that we, in the large cities of America, are becoming more familiar with as we increase in material wealth, and are surrounded by so many luxuries; where the inclination for active duties decreases in the diminished struggle for existence, and where there is insufficient elimination of effete matter from the muscular exertion, so necessary for the combination of the healthy body and mind. From closely observing these cases of suppressed gout, with functional disturbance, making numerous physical examinations of

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the heart and lungs, many chemical and miscroscopical examinations of the urine, and watching the effects of the gouty element on the cerebro-spinal system, and, more especially, the influence on the extended sympathetic nervous system, also the effects on the distal cutaneous nerves, and irritation of the skin, by the frequently appearing and disappearing of a modified form of eczema, so faintly seen that its presence is often obscure. These observations, Mr. President and gentlemen, incline me to place these functional symptoms among the most important, as Dr. Garrod has so ably done, and to believe that the physician who overcomes them, and keeps his patient even temporarily in a condition of health, has accomplished one of the most difficult tasks in the profession. The treatment, as a rule, in New York has to commence with a stomach prone to a condition of gastric catarrh, where morbid materials form a peculiar fermentation, to end in producing the oxalic diathesis; this can be overcome by hydrochloric acid and the bitter tonics, and special attention to diet. The cerebral congestion may be relieved by the bromides; the functional disturbance of the heart, by either digitalis or hyoscyamus; the bowels, by colocynth and strychnia, and the kidneys, by buchu and uva ursi; while the irritable skin is relieved by warm baths, followed by the application of some oleaginous substance. The general health is improved by massage, twice a week from a vigorous person, while daily mental and physical exercise must follow. To overcome the gout, colchicum and the iodide of potassium are given p. r. n. When thus treated, with copious drinks of pure warm water, we may expect a marked improvement, or possibly a return to perfect health. But, on the other side, when these gouty functional disturbances are ridiculed or neglected by the physician, and the sufferer is permitted to long continue in this irritable nervous condition, under the plea that he is a hypochondriac, and permanent changes are allowed to occur in the cerebral meninges (as we can so clearly follow and see in the skin) then we may have acute mania, ending in incurable insanity, with the remainder of a life spent in a lunatic asylum. Illustrations of all these phases I have personally observed in the city of New York. In regard to the general subject of Dr. Garrod's paper—viz., eczema and albuminuria in relation to gout, conclusions may be reached similar to those when we observe the relationship of effects in the general shedding of the epidermis and the desquamative nephritis of scarlet fever; and as illustration of the association of eczema and albuminuria in a case of gout, I will cite the following instance, when, in the midst of apparent good health, the force of the gout expended itself, at first on the kidneys, then the skin and joints of the great toe, soon followed by convulsions, and finally ending in death. A very wealthy gentleman, aged thirty-seven years, weighing about 240 pounds, a high liver, drinking frequently to excess, accustomed to an open-air occupation, and one who had never had an explosive attack of gout, was suddenly called, during the temporary illness of his partner, to remain, from 7 A.M. until 6 P.M. daily, in his office; which duty, on all previous occasions had been very irksome to him. He continued eating very heartily, and each night to drink largely of wines—especially champagne—until one morning, on awaking, he felt so uncomfortable and alarmed, that he walked a distance of half a mile to my office, and complained of headache, distress of the stomach, pain in loins, and was greatly annoyed at the unusual irritation of the skin. On a thorough physical examination I failed to detect any morbid condition sufficient to cause so much terror, until on examination of the urine by heat and nitric acid, albumen to more than 50 per cent. was revealed, and under the ¼ inch objective, the casts of acute desquamative nephritis were seen in great abundance. His general appearance was that of approaching convulsions, as I so expressed to Dr. Flint at our first consultation. He was sent home, and the following treatment commenced:—elaterium and colocynth to quickly empty the bowels and relieve pressure on the kidneys; dry cups over the loins, hot bath and wrapping in wet blankets

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(kept continually warm); an abundance of hot drinks until a decided hydragogic carthartic effect had been produced, and diaphoresis and diuresis followed; after which he improved so rapidly, that in a week he returned to his place at the office, and although particularly cautioned against the effects of exposure and injudicious diet, soon returned to his former mode of living, and in less than a month had a recurrence of his former symptoms, and before a physician could arrive was in violent convulsions, lasting from 12 to 6 o'clock. He recovered from the effects of the convulsions so rapidly, that on the following day, he said, were it not for his sore tongue (which he had bitten) and his headache, he would return to his office. He again rapidly convalesced, and with great difficulty was kept in the house for ten days; and as he showed such a disinclination to be cautious, the final consultation was agreed to be held in his presence, when the extreme gravity of his case could be presented to himself and brother. Dr. Austin Flint, Dr. Alonzo Clark, and Dr. L. A. Sayre, were then also present, and the patient, who was always exceedingly jovial, was plainly told that he would die if he continued in his careless way of living. He promised to abide by instructions, but soon after, returning to his former company, he resumed his old habits, which in a few days ended in death.

What is the Clinical Value of the Examination of the Urine in Bright's Diseases?

Prof. T. Grainger Stewart, F.R.S.E., Edinburgh.

In considering the clinical value of the examination of the urine in Bright's diseases, I shall speak of—

1.—The quantity;
2.—The specific gravity and solids;
3.—The albumen;
4.—Blood;
5.—Tube casts:

and I shall try to explain what I believe to be the indicated condition.

1.—*The Quantity* is diminished in the inflammatory affection of the tubules during the acute and early stage, and during exacerbation. It is also markedly diminished in glomerular nephritis, and in the combinations of tubular inflammation with glomerular change. This diminution may be to one-half the normal amount, or even much lower. It may, indeed, give place to complete suppression.

Diminution also occurs sometimes at the very end of the cirrhotic process; sometimes as a consequence of the superaddition of inflammation of the tubules, sometimes without obvious additional alteration. It is easy to understand how a diminution should occur when the tubules are blocked up by altered cells and exuded material, for then the blood-pressure within the Malpighian tufts is opposed by a counter-pressure within the tubules. Equally obvious is the explanation of the diminution in glomerular nephritis; for in that disease the circulation in the capillaries of the Malpighian tufts is very greatly diminished, and consequently transudation is rendered almost impossible; but why great diminution, or even suppression of urine, should sometimes occur as the terminal change of a chronic process I cannot at present see. It may, perhaps, be attributable to causes beyond the kidney altogether.

Increase of the quantity is very generally admitted to be characteristic of the cirrhotic interstitial or contracting kidney, and there can be no doubt that often in that disease the urine exceeds 100 ℥, sometimes reaches nearly 200 ℥ in twenty-four hours.

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It is also to be noticed, that the increase is mainly during the night, or at least that the calls to micturition are much more frequent during the night. It will be found, however, that polyuria is not an early symptom of cirrhotic Bright; that, on the contrary, it appears when the process is well advanced, when the heart has become hypertrophied, the pulse tense, and the unmistakable groups of symptoms characteristic of it have been fully developed; but it is not to be found during these earlier months or years of the process, when the patient regards himself as quite well, and is doing arduous work without apparent difficulty.

The explanation of the polyuria in the more advanced stages of this process seems to be found in the increased blood-pressure existing in the arterial system, and within the Malpighian tufts. That increased pressure, being to a large extent unopposed by any blocking of the tubules, must necessarily lead to increased transudation.

An increased flow of urine may also take place in the tubular inflammation as the result of the action of diuretics, or of the rapid absorption of dropsies. I have on some occasions seen an increased discharge in very advanced stages of the inflammatory process, where the general condition of the patient in respect of heart, &c., had come to be very much like that in an ordinary case of cirrhosis; under these conditions the explanation is obvious.

My experience satisfies me that, in the waxy degeneration, polyuria commonly exists throughout the whole course of the disease in pure cases, and is in many instances a very early symptom. In Edinburgh we meet with a very large amount of waxy disease, and I have seen a considerable number of cases perfectly uncomplicated—*i.e.*, cases in which there was no inflammation of the tubules and no marked cirrhosis; and in these typical cases I have found polyuria exist from first to last. In one case, which I watched for nine years, and the details of which are published in my book, the urine considerably exceeded the normal standard before albumen appeared, and being led by the syphilitic history of the patient, by his exhibiting the signs of waxy liver and waxy spleen, and by the observation that polyuria existed in other waxy cases, to surmise that the kidneys were undergoing waxy degeneration, I tested the urine day by day, and after a time found a trace of albumen, which gradually increased, till it became well-marked, and then persisted during the whole nine years that the patient was under observation. This view which I have long maintained, as to the early appearance of polyuria, has been accepted by many of the best observers—Murchison, Dickinson, Bartels, and other authorities, but has been denied by Ebstein, and lately by Wagner.

Ebstein has come to the conclusion that an increase in the quantity of urine at first does not constitute a characteristic symptom, but that where it is observed it is not to be under-estimated. He considers that the figures given in some of my earlier papers do not bear out my statements regarding polyuria. I am at a loss to understand how this may have arisen, unless he has failed to distinguish between the uncomplicated and the complicated forms.

In a recently published paper,* E. Wagner states that he has not been able to observe the early polyuria of which I speak; and further, that, in those cases under his observation, the urine was almost invariably diminished in quantity during the progress of the malady. A statement so strikingly at variance with, not merely my own recorded views, but those of many other observers can, I take it, only be accounted for in one way—viz., that he has made his observations mainly upon mixed cases. The majority of these belonged to the second and third of his subdivisions—that is, the kidneys showed fatty and interstitial changes, in addition to the amyloid degeneration. A kidney which

* *Deutsches Archiv für Klinische Medicin*, Bd. xxviii. 416.

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had undergone waxy change pure and simple, without any complication, had, he says, scarcely been met with by him clinically. From such data I have already observed that no safe conclusion can be drawn. I have often found it difficult to get the history of polyuria, but in, I believe, every instance of uncomplicated waxy disease, and in many of these which afterwards become complicated, I have been able to satisfy myself of the existence of the condition, and I consider it one of the most reliable characteristics of waxy degeneration of the kidney which we possess.

It is true that in many cases where the waxy disease exists in the Malpighian bodies the urine is found to be at or below the normal standard, but that is where inflammation has become superadded to the waxy disease, or where the existence of waxy degeneration of the intestine is leading to very copious diarrhœa, or, more rarely, where other diseases of kidney (embolism, syphilitic disease, or strumous change), complicate the renal malady.

As to the cause of the polyuria in waxy disease, I have not been able to find a better explanation than my hypothetical one, now twenty years old, that the degenerated vessels admit of an unnaturally ready transudation of the watery elements of the blood.

2.—*Specific Gravity and Solids.*—The specific gravity of the urine is determined mainly, on the one hand, by the amount of water excreted; on the other, by the amount of solids. As we have seen that the amount of water is increased in advanced cirrhotic cases, and in waxy disease throughout its course, the specific gravity is naturally found to be low in these conditions; but seeing that in the cirrhotic the proportion of urea excreted becomes greatly diminished, being reduced to one-half, or even less, the specific gravity is particularly low in that disease. In the purely waxy disease, on the other hand, the urea is scarcely, if at all, diminished, and the low specific gravity is mainly due to increase of water. In the later stages of the inflammatory affection of the tubules a low specific gravity is also common, but during the early stage of inflammation the specific gravity varies considerably. I have found it as low as 1012 and as high as 1030. Sometimes it goes considerably higher. Dr. Roberts has found it as high as 1065, and Sir R. Christison, 1046. Sir Robert was able to satisfy himself that the density was in that case partly due to the coagulable solids, the sp. gr. having fallen after filtration from 1046 to 1037.

It thus appears that in inflammatory Bright's disease the sp. gr. may be influenced by the amount of water, the amount of urea, of albumen, or of blood.

3.—*Albumen.*—There are various albuminoid bodies which appear in the urine under pathological conditions, viz.:—

(1.)—Serum-albumin.
(2.)—Paraglobulin.
(3.)—Pepton.
(4.)—Fibrinogen.
(5.)—Propepton.

(1.) Of these, by far the most important is serum-albumin, the albumin which most frequently occurs, and to which relate the experiments and discussions as to the origin of albuminuria. Before entering on this subject, I must refer briefly to the other substances, because, though at present we know little or nothing as to the significance of their appearance in the urine, they may soon become important (from the progress of research).

(2.) Paraglobulin is distinguished by its precipitation by great excess of magnesium sulphate.

Lehmann* was the first to discover its existence in the urine. Subsequently,

* *Arch. f. Path. Anat.*, Bd. xxxvi. p. 125.

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Edlefsen,* Senator,† Lauder Brunton, and Purvis investigated it. Senator stated that paraglobulin occurred in albuminous urine along with serum-albumin, but in very varying amount. He met with it in the largest quantity in cases of amyloid degeneration; but the accuracy of this observation has been called in question by Heynsin; and therefore, notwithstanding the fact that some others have worked at the problem, no certain conclusion can be arrived at until further careful and extensive inquiries have been conducted with the employment of Stokvis' method for the separation of paraglobulin by means of magnesic sulphate, and with due regard to the caution of Hoppe-Seyler—viz., that albuminous urine is more liable to undergo putrefaction than non-albuminous, and that this process converts albumen into bodies identical in reaction with paraglobulin and peptone.

Peptone is characterized by the pink or rosy red colour given with caustic potash and cupric sulphate. Its occasional presence in the urine was, so far as I am aware, first shown by Gerhardt. Mariner detected its presence in cases of general nutritional disturbances, such as acute phosphorus poisoning, of extensive purulent discharges, croupous pneumonia, &c., but no definite inference can be drawn from the facts hitherto recorded.

Fibrinogen occurs very rarely in urine—only in cases of chyluria—so that we need hardly consider it further.

Propeptone is characterized by its not precipitating by boiling alone, or with nitric acid, until the solution becomes cold. Under this heading I have thought it best to follow somewhat closely the classification of Hoppe-Seyler, grouping several varieties of albumen, which have from time to time been described by various writers, and all of which correspond more or less closely with the product of digestion which Kühne calls "hemialbumose," and which Schmidt-Mülheim has denominated propeptone. The best known of these substances is that which Bence Jones‡ found in the urine of a patient suffering from mollities ossium. Other similar forms of albumen have been found by Virchow,§ by Pavy,‖ by Lassar,¶ and by Salkowski,** but the record is at present unfortunately without much clinical significance. Allied to these, but of more complex constitution, there stands the colouring matter of the blood. Unaltered hæmoglobin is very rarely indeed, if ever, found in the urine, but meta-hæmoglobin invariably appears whenever a solution of the red corpuscles takes place within the blood-vessels.

Having thus briefly referred to these, so far as we at present know, comparatively unimportant substances, I must now consider serum-albumen itself as it occurs in Bright's disease. Serum-albumen is most abundantly met with in the inflammatory affection of the tubules. In the early stages, and during acute exacerbation, being often associated with blood, but persisting often in large quantity throughout the whole course of the disease, even after the disappearance of every other symptom. In the waxy disease it is present in large quantity; still more in the mixed cases, where inflammation of the tubules is superadded to the waxy. In the early stage of the pure waxy condition, the quantity of albumen is much smaller, and I have watched its gradual increase from the slightest trace to a fair moderate amount, say $\frac{1}{12}$ oz. In the cirrhotic form the amount of albumen is very irregular, and it is frequently absent altogether. I think it is

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* *Deutsches Anat. f. Klin. Med.*, Bd. vii. p. 67.
† *Arch. f. Path. Anat.*, Bd. lx.
‡ "Philosophical Transactions," 1848, i. p. 55.
§ *Arch. f. Path. Anat.*, Bd. iv. p. 309.
‖ *Lancet*, May 23, 1863.
¶ *Arch. f. Path. Anat.*, Bd. lxxvi. p. 164.
** *Ibid.* Bd. lxxxi. p. 552.

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probable that cirrhosis may be quite distinct, as an anatomical change, without the occurrence of any albuminuria.

In considering the causation of albuminuria it may be well first to satisfy ourselves as to the particular part of the kidney through which the albumen escapes into the urine. This has been effected in two ways, and from two sides, and as the results of these methods correspond we may safely accept the conclusion that it is from the Malpighian bodies that the albumen escapes. In 1878, Nussbaum, availing himself of the fact that, in the frog, circulation in the glomeruli is from the renal artery, and therefore entirely distinct from that round the uriniferous tubules, which is from the lower parts of the body, by a peculiar venous plexus arranged somewhat like the portal circulation, determined the question in the following manner. Having ligatured the renal artery, he injected into the blood-current a quantity of egg-albumen sufficient in ordinary circumstances to produce albuminuria.

As however no albumen appeared, though the circulation was free round the tubules, it followed conclusively that the absence of blood from the Malpighian tufts was the cause of the absence of albumen from the urine, and that, therefore, the tufts are the seat of the transudation of albumen when it occurs.

This, though a very conclusive, may be regarded as a negative, proof. It is well, therefore, to be able to refer to the admirable positive proof afforded by the recent experiment of Posner, who boiled the kidneys in cases of albuminuria, and, on microscopic examination, discovered the effused albumen coagulated within the capsules of the Malpighian tufts. This beautiful observation has been confirmed by Ribbert and Littin.

As to the causation of the transudation of albumen, there are four elements requiring attention as possible factors—viz., abnormal conditions of the blood, increased blood-pressure, peculiarity of vascular walls, and alterations of the renal epithelium.

As to the blood. To ascertain whether a hydræmic condition of the blood would produce albuminuria, water has been injected into the vessels, and albuminuria has resulted; but the substance effused was hæmoglobin and not serum-albumen. The presence of excess of water cannot be regarded as the cause of albuminuria, especially in view of the improbability of the existence in the human subject of such a degree of hydræmia as would give rise to hæmoglobinuria.

Wundt's idea that the deficiency of common salt in the blood might lead to transudation was disproved by Stokvis's experiments upon animals. The suggestion of Canstatt and Prout, that some chemical change in the composition of the intra-vascular albumen was the cause of its transudation, has been completely set aside, I think, by other experiments by Stokvis.

Thus, an altered composition of the blood cannot, as far as we see at present, be regarded as a causative factor, directly at any rate, though it may have its place in giving rise to the conditions next to be considered.

Blood-pressure has been supposed to influence the process in two quite different ways—viz., by increased and by diminished pressure.

Runeberg published, in the *Archiv für Heilkunde*, Band viii., and subsequently in the *Nordiskt Med. Arkiv.* vol. ix. (1877), certain observations which led him to the conclusion that the permeability of animal membranes for albuminous molecules varies according to pressure, being less when the pressure is greater, and more when it is lowered. A further development and application of this statement to practical medicine is contained in a very elaborate paper by the same author, which appeared in two numbers of the twenty-third volume of the *Deutsche Arch. für Klin. Med.* (1878 and 1879). With very considerable acumen, Runeberg there applies his theory to each pathological condition under which albumen appears in the urine, and traces all to one cause—viz., diminished

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pressure in the Malphigian tufts. But it is not necessary now to enter into any criticism of his views (as Heidenhain has recently done*), for the physical datum on which he rested his theories has been found to be unreliable. Gottwalt, in 1880 (*Zeitschrift f. Phys. Chemie*, Band iv.), and Bamberger, at the beginning of the present year (*Wiener Med. Woch.*), have both shown with the greatest distinctness that the reverse is the case; Gottwalt stating that "the percentage of albumen (as well serum-albumen as globulin) in the filtrate, is the greater the greater the pressure."

On the other hand, it can hardly be held that albuminuria is the result of increased pressure. In very many instances of albuminuria the pressure in the glomeruli is under the normal; even in renal stasis it is very doubtful if there is at any time an increased pressure. Experimentally, it has been repeatedly shown that artificially increased pressure in the kidneys (as by ligature of the aorta below the renal arteries) does not give rise to albuminuria; and it is a matter of common observation, that in many instances great hypertrophy of the left ventricle exists without the appearance of a trace of albumen in the urine.

We are thus compelled to turn to anatomical changes in the renal tissues for the cause of albuminuria. As it has been already indicated that it is through the Malpighian capillaries and into the capsule of Bowman that the albumen escapes, we naturally concentrate our attention to this part of the renal apparatus. That the Malpighian bodies may maintain their normal condition, it is necessary that there should be no diminution, even for a short time, in the quantity of blood which passes through them. Thus, Overbeck found experimentally that a very brief occlusion of the renal arteries was followed by a more or less prolonged albuminuria; and Hermann subsequently observed that even partial occlusion produced the same result. Is is therefore clear that any diminution of this nutritive supply deprives these bodies of the power of preventing the transudation of albumen. It is easy to see how this depraved condition of the Malphigian bodies may arise in many cases of albuminuria. Let there be some delay in the circulation, or deficiency of oxygen, or of nutritive material, and albuminuria will be caused. We know how commonly such conditions occur in diseases. To these causes may be attributed the albuminuria of inflammatory and cirrhotic Bright's disease, of valvular heart disease, and of anæmia.

Is it to the mal-nutrition of the vascular walls of the glomeruli or of their epithelial covering that we are to refer the albuminuria? Perhaps Cohnheim is correct in holding that the capillary walls are always permeable to albumen, which is retained by the epithelium; but there are difficulties in the way of fully accepting that view. In waxy Bright's disease the vascular walls are probably abnormally permeable. As to the condition of the epithelium in such cases we know little, and it is possible that it, too, is affected directly from without, and rendered incapable of preventing albuminous transudation.

4. *Blood.*—Blood may be met with in the urine in any of the forms of Bright's disease, but it is much the most frequently present in inflammation. Blood is most abundant during the early acute stage, although it may recur from time to time during acuter exacerbations. In the waxy disease, hæmorrhage very rarely occurs; but I have known it to do so on some occasions, just as hæmorrhage is occasionally seen in the spleen, the intestine, and the stomach, in association with that degeneration. In advanced cirrhotic cases, blood occasionally appears at a time when other hæmorrhages are not unfrequent.

5. *Tube-casts.*—Various forms of tube-casts are recognized by clinical observers, the

* Hermann's "Handbuch der Phys.," Band v. 1880.

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most important being the hyaline or structureless, the granular, the epithelial, and the fatty. There may also be mentioned mixed forms, bloody casts, purulent casts, and the extremely rare true waxy or amyloid casts. Tube-casts are found most abundantly in inflammation of the tubules, especially in its early stages. They are then epithelial, bloody, or hyaline, the hyaline being much more common in this condition than has generally been supposed. In the later stages, fatty casts are the most characteristic and abundant, while hyaline casts, and casts partly hyaline, continue to be discharged. In the waxy disease of the kidney, tube-casts are very rare, and in pure cases they are simply hyaline. Some observers have met with casts giving a distinct reaction with iodine and other reagents, but I have never met with an instance. In the cirrhotic form of Bright's disease, tube-casts are even rarer than in the waxy, and are for the most part hyaline. With regard to the mode of the production of casts, many views have been suggested. The hyaline casts have been regarded as fibrinous cylinders. They have also been attributed to the coagulation of some other albuminous material, supposed to be poured out from the blood. It has been further suggested that the epithelial cells pour out hyaline material in drops, which run together to form the casts. It appears to me that hyaline casts are the result of transudation through the Malpighian bodies rather than a product of cells, and that the inclusion of the epithelium in various stages of transformation, blood, leucocytes, and other materials, constitutes the bulk of them.

In connection with so important a subject as this we must not omit to allude to those cases in which albumen may appear in the urine independently of any affection of the kidneys. It has been known for some years that albuminuria may occasionally be met with in persons whose kidneys are healthy. I must refer, for a detailed description of this peculiar condition, to articles by Vogel,* Ultzmann,† Leube,‡ Edlefsen,§ Dukes,‖ Gull,¶ Saundby,** Fürbringer, †† and Runeberg.‡‡ This form of albuminuria, which occurs with a certain periodicity, is frequently met with in persons of weakly constitution, as observed by Sir W. Gull; but it also occurs in perfectly healthy persons under the influence of excitement, muscular exertion, &c. It is extremely unlikely that it is caused by any anatomical change of a recognizable character; still less, that it depends on Runeberg's theory of diminished vascular pressure. In regard to its diagnosis, little difficulty can arise when the whole history of the case is taken into account, and when the urine is examined at different periods of the same day.

Ueber verschiedene Formen der Bright'schen Krankheit.

Professor S. Rosenstein, Leyden.

Meine Herren! Aus eigenem Antriebe würde ich grade hier, in London, wo wir uns, was Entstehung und Entwickelung unserer Kenntnisse von den Nierenkrankheiten betrifft, so zu sagen, auf klassischem Boden befinden, kaum gewagt haben, die Discussion einer so schwierigen Frage einzuleiten. Aber das ehrende Ansuchen des Comités, das mich dazu aufforderte, meinte ich um so weniger ablehnen zu sollen, als meine eigne

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* Virchow's "Handbuch der sp. Path." &c., Band vi. 522.

† *Wiener Med. Presse*, 1870.

‡ Virchow's *Arch.*, Band lxxii. 1878.

§ "Mitth. für den Verein Schleswig-Holst. Aerzte," 1879.

‖ *Brit. Md. Journal*, 1878.

¶ *Lancet*, i. 1873, p. 808.

** *Brit. Med. Journal*, 1879.

†† *Zeitschr. f. Klin. Med.*, Band i. 349.

‡‡ *Deutsche Arch. f. Klin. Med.*, Band xxvi.

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Meinung hinsichtlich der verschiedenen Formen der Bright'schen Krankheit von derjenigen stark abweicht, welche grade hier in England am meisten vertreten wird.

Die Entscheidung der Frage, ob und welche verschiedenen Formen der Bright'schen Krankheit anzunehmen sind, ob solche ihre Entstehung ursprünglich verschiedenen Grundprocessen verdanken, oder aber, obgleich der Grundprocess in allen derselbe ist, doch anatomisch differente Producte darstellen, je nachdem das eine oder andre der Gewebselemente der Niere afficirt wird, hängt wesentlich davon ab, wie weit oder eng man den Begriff des M. Brightii fasst. Nicht nur in der ersten Zeit, als Bright eben in der Erkrankung der Niere das Bindeglied zwischen Wassersucht und Albuminurie entdeckt hatte, sondern auch lange nachher war man geneigt, alle Fälle, in denen irgend welche palpablen Veränderungen der Niere neben Albuminurie gefunden wurden, mit dem Namen des M. Brightii zu bestempeln, und einzelne Gruppen, je nach dem ätiologischen Momente, das ihnen zu Grunde lag, zu besonderen Formen zu vereinigen. So finden wir bei dem besten deutschen Bearbeiter dieses Gegenstandes, bei Frerichs, als solche besondere Formen aufgeführt den M. Brightii Cachecticorum, den M. Brightii Gravidarum, den nach Masern, Typhus, Cholera, u. s. w. Weniger consequent, aber im Principe doch nicht abweichend, hat Leyden neuerdings wieder verschiedene Formen aufgestellt, indem er bald die anatomischen Veränderungen, bald das ätiologische Moment als Eintheilungsgrund benutzt und neben der desquamativen Form, die toxische, die septische u. a. m. unterscheidet und alle diese Gruppen wieder in dem Sammelnamen des M. Brightii vereinigt. Ehe man aber diesem Verhalten zustimmt, wird man sich fragen müssen, welches die anatomischen Kriterien sind, die wir zu fordern haben, um von M. Brightii reden zu dürfen. Genügt hierfür jede mit Albuminurie verbundene palpable Veränderung der Niere, oder kommt dieser Name nur streng begrenzten, bestimmten anatomischen Veränderungen zu? Da Bright selbst schon auf dem Standpunkte stand die Entzündung der Niere als Grundlage der von ihm beschriebenen Krankheitsbilder zu betrachten, so werden wir nicht fehl gehen, wenn auch wir nur da von M. Brightii sprechen, wo wirklich entzündliche Veränderungen in den Nieren gefunden werden. Danach hat aber ebensowenig die febrile Albuminurie, als etwa der Nierencatarrh, etwas mit M. Brightii zu thun. Eine ganze Reihe der als besondre Formen unterschiedenen Affectionen ist nicht entzündlicher Natur. Von einer derselben, dem M. Brightii Cachecticorum nehmen gegenwärtig Alle an, dass sie ihren Grund nicht in primärer Entzündung, sondern in der Entartung der Gefässe hat, und obgleich sowohl die Entzündung sich ebenso zur amyloiden Degeneration, wie diese zur Entzündung gesellen kann, so verläuft sie nicht selten doch völlig rein, ganz auf die Gefässe beschränkt, sogar ohne jede Veränderung der Epithelien, und darf deshalb ursprünglich nicht als besondere Form des M. Brightii aufgefasst werden. Weniger Uebereinstimmung herrscht schon betreffs derjenigen Nierenveränderungen, welche im Gefolge von Herz und Lungenkrankheiten auftreten. In der That sind diese auch nicht alle gleichwerthig, so dass sie rundweg in eine Gruppe vereint werden können. Man muss von vornherein zwischen denen unterscheiden, welche bei Klappenaffectionen der arteriellen Ostien, in specie bei Insuff. der Aortenklappen vorkommen und denen, welche die Erkrankungen der venösen Ostien, das Emphysem, und die chron. Bronchitis begleiten! Die ersteren sind wirklich entzündlicher Natur und weichen in nichts vom histologischen Bilde der diffusen Nephritis ab, während die letzteren in der überwiegenden Mehrzahl der Fälle solches nicht zeigen, sondern nur regressive Veränderungen der Epithelien, sclerotische Verdichtung des Bindegewebes und Veränderungen der Glomeruli zeigen, wie solche bei allen Circulationsstörungen in der Niere vorkommen. Wo daher die letzterwähnten Alterationen nicht über die der sogenannten cyanotischen Induration hinausgehen, darf man sie in der That ebenfalls, wie Traube gethan hat, als *affectio sui generis* vom M. Brightii aus-

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scheiden. Nur darf nicht vergessen werden, dass auch die Affectionen der venösen Ostien sich gelegentlich von vornherein mit entzündlichen Affectionen der Nieren combiniren können, wobei dann Herz und Nierenaffection nicht in causaler Verbindung mit einander stehen, sondern Coeffecte der gleichen Ursache sind. Darum bleibt aber im Allgemeinen die Stauungsniere keine besondere Form des M. Brightii, auch da nicht, wo sie bis zur Schrumpfung vorgeschritten ist. Denn diese letztere kann auch ohne entzündliche Alterationen, allein als Folge mangelhafter Ernährung der Gewebselemente zu Stande kommen. Denn grade in solchen Nieren sieht man häufig eine grössere Zahl von Glomerulis geschrumpft, ohne jede gleichzeitige Verdickung der Kapsel, ohne jede zellige Infiltration der Interstitien oder Hyperplasie des Bindegewebes, nur mit Desquamation und Trübung der Epithelien in den Harnkanälchen und auf dem Glomerulus, wo solcher noch nicht verödet ist. Wiederholt habe ich in solchen Fällen, die mit Blut gefüllten Schlingen der Gefässknäuel nackt, ohne dass eine Epithelzelle sie bedeckte, liegen sehen. Der so statthabende und zur Schrumpfung führende Collaps von Glomerulis und Harnkanälchen hat nichts mit der Entzündung gemein. Und ebensowenig als die reine "Stauungsniere" eine Form des M. Brightii ist, ebensowenig ist es die durch die Schwangerschaft veranlasste Veränderung des Nierengewebes. Schon 1863 habe ich auf die Nothwendigkeit hingewiesen, streng zu scheiden zwischen Gravidæ, die nierenkrank waren, ehe sie schwanger wurden, und solchen, die zwar während der Gravidität an den Nieren erkranken, aber nicht durch dieselbe, und solchen endlich, bei denen kein andres ätiologisches Moment gefunden werden kann, als die Schwangerschaft. Die letztere Gruppe bildet schon an sich ein recht kleines Contingent und von dieser kleinen Zahl kommen glücklicherweise nur Wenige zur Obduction, so dass wir für die Beurtheilung der Art des fraglichen Leidens hauptsächlich auf die klinische Beobachtung angewiesen sind. Diese nun zeigt uns Symptome, welche mit beinahe zwingender Nothwendigkeit auf Circulationsveränderungen im Abdomen als Ursache der Nierenerkrankung hinweisen. Das überwiegende Auftreten der Affection bei Primiparae, und unter diesen insbesondere bei Zwillingsschwangerschaften, die Zeit der Erkrankung hauptsächlich in den letzten Monaten der Gravidität, das häufige Vorangehen von Anasarca der unteren Extremitäten vor der Albuminurie, der sparsame, trübe, schwere Harn mit hohem Eiweissgehalte, der steigernde Einfluss der Wehenthätigkeit auf diesen und der vermindernde der Entbindung—alle diese Momente zeigen, dass es sich um eine verhältnissmässig schnell sich entwickelnde Circulationsstörung handelt, welche zu veränderter Ernährung der Epithelien sowohl der Glomeruli, als der Harnkanälchen, führt, die entweder nur albuminös sich trüben, oder selbst verfetten können, jedenfalls aber auch schneller Reparation zugängig bleiben. Und hiemit stimmen die Ergebnisse der Autopsie, welche in den Nieren eigentlich entzündliche Veränderungen vermissen und nur epitheliale Ernährungsstörungen finden lassen. Die Schwangerschaftsniere stellt keine wahre Nephritis dar, und ist auch keine besondere Form des M. Brightii.

Wenn wir danach bei Besprechung des M. Brightii in strictem Sinne die reine amyloide Degeneration "die cyanotische Induration" der Nieren und die "Schwängerschaftsniere" ausschliessen und nicht als zu M. Brightii gehörige Formen der Nierenaffection betrachten, so kommen wir nun zu denjenigen Alterationen, welche unzweifelhaft entzündlicher Natur sind, und damit das Substrat des M. Brightii bilden. Am typischsten lernen wir diese da kennen, wo sie sich in acuter Weise entwickeln, wie namentlich häufig bei Scarlatina, seltner beim Typhus und noch seltner unter dem Einflusse sogenannter rheumatischer Ursachen. In allen diesen Fällen, ebenso wie da, wo wir künstlich bei Thieren durch Cantharidin, z. B., eine acute Nephritis produciren, nehmen wir als hervorstechendste Veränderungen, bei microscopischer Untersuchung der äusserlich hyperämischen, geschwollenen, auf der Oberfläche oft mit vielen Blutungen besäten, oder

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mehr buntbestreut aussehenden Niere, die des Gefässapparates und der Epithelien wahr. Vom Beginn an sieht man herdweise Anhäufungen von Rundzellen, besonders rings um die Glomeruli, aber auch um andre Gefässe, welche dann innerhalb verbreiterter und mit Lymphkörperchen gefüllter Interstitien liegen. Dabei werden bald die Epithelien der gewundenen Kanälchen körnig, geschwollen, und ihre Kerne nehmen bei Tinction weniger Farbstoff auf, als in den normalgebliebenen Parthien. Stets erfährt auch das Glomerulusepithel Veränderungen; ebenso, wie das der Harnkanälchen, schwillt es auf und desquamirt, so dass, worauf Langhans besonders die Aufmerksamkeit gelenkt hat, desquamirtes Epithel zwischen Glomerulus und Kapsel sich findet, bald ein regelmässiges Stratum bildend, bald ein feinkörniges Exsudat, welches aus Eiweiss besteht, und worin die Kerne besonders sichtbar sind. (Beiläufig bemerkt, ist es von Interesse, dass Becquerel das Vorkommen von Eiweiss im Kapselraum kannte und dasselbe durch Kochen, Erhärtung in Alcohol und Behandlung mit Salpetersäure nachwies). Ob diese desquamirten Epithelzellen von der Kapsel und dem Glomerulus abgestossen sind, oder auch alleinige Wucherung des Kapselepithels vorkommt, wage ich nicht zu entscheiden. Doch möchte ich darauf aufmerksam machen, dass ich im frühesten Stadium von Scarlatinanephritis Anhäufung von Lymphkörperchen um die Glomeruli gesehen habe, und solche Präparate bewahre, in denen noch keine Veränderungen des Glomerulusepithels sichtbar waren, so dass ich auch nicht wie Ribbert die Alteration des Glomerulusepithels überall als erste Einleitung aller geweblichen Veränderungen ansehen kann, wie solche übrigens auch schon von Becquerel angenommen wurden. Ausserdem halte ich die Desquamation des Glomerulusepithels, die beinahe in keiner, wie auch sonst erkrankten Niere gänzlich vermisst wird, nicht für den Ausgang eines entzündlichen Processes. Den Beweis dafür sehe ich darin, dass derselbe Befund nicht bloss bei Herzkranken gefunden wird, bei denen gar keine oder nur sehr kurz dauernde Albuminurie stattgehabt hatte und in deren Nieren andere entzündliche Alterationen fehlen, sondern vor allem darin, dass ich ihn auch in einem Falle sporadischer Cholera traf, der nur drei Tage gedauert hatte und unter absoluter Anurie verlaufen war. Hier bestanden keine weiteren Veränderungen, als dass einzelne Glomerulusschlingen von Epithel entblösst waren. Thatsache bleibt jedenfalls, dass Anhäufung von Lymphkörperchen um Glomeruli und Gefässe, und Desquamation von Epithel die ersten Veränderungen in der entzündeten Niere darstellen. Im weiteren Verlaufe—in den acuten Fällen tritt meist früh der Tod ein, oder was viel häufiger, nach Wochen oder Monaten Genesung—schreiten die Veränderungen der Epithelien rasch vorwärts, sie werden fettig, bröckeln ab, zerfallen zu emulsiven Massen. Je länger der Zustand gedauert hat, um so mehr hat sich aus der "hyperämischen" oder "buntgefleckten" die "weisse Niere" entwickelt. Diese ist das Paradigma geworden, woran die ausschliesslich parenchymatösen Veränderungen gezeigt wurden, um zu beweisen, dass es eine Form des M. Brightii giebt, welche ohne jede Veränderungen der Interstitien verläuft, und die fast nie in Schrumpfung übergeht. Die genauere Forschung hat aber gezeigt, dass auch bei der grossen, weissen Niere, ganz abgesehen noch von denjenigen Fällen, in denen sie sicher oft mit amyloider Degeneration verwechselt ist, neben der ausgebreiteten fettigen Degeneration des Epithels der gewundenen Kanäle, der Glomeruli und oft auch der Gefässe, ebenfalls Infiltration der Interstitien mit Lymphkörperchen gefunden wird. Dickinson giebt sogar an, wo der Process länger gedauert hat, dieselben niemals vermisst zu haben. Ebenso finden sich auch hier geschrumpfte Glomeruli zerstreut durch die ganze Rinde, deren Harnkanälchen untergegangen sind und statt ihrer fibrilläres Bindegewebe. Mit Rücksicht auf alle diese Veränderungen hat man also, auch wenn man die ausgebreitete und starke Verfettung der Epithelien als die gröbste Alteration anerkennt, doch keinen Grund, die "weisse

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Niere" als eine principiell unterschiedene Form hinzustellen, da, wie gesagt, die Zeichen der interstitiellen Veränderungen beinahe niemals fehlen. Auch die weisse Niere gehört zur "diffusen" Nephritis in dem Sinne, dass alle Gewebselemente des erkrankten Organes am Processe der Entzündung Theil nehmen. Zwischen ihr und denjenigen Producten, welche von anderer Seite her als Beispiele ausschlieslich interstitieller Veränderungen betrachtet werden, den "glatten Schrumpfnieren," ist in der That nur ein gradueller Unterschied in der Ausdehnung der Erkrankung der einzelnen Gewebselemente. Diese Nieren sind meist normal gross, selten nur sogar etwas grösser, am häufigsten schon etwas kleiner, von glatter Oberfläche und gescheckten Aussehen. In ihnen findet man ungleich stärker als in der "grossen weissen" die Schrumpfung der Glomeruli, die Verdickung der Kapseln, die zellige Infiltration der Interstitien und die herdweise fibrilläre Hyperplasie, wo die Kanälchen zu Grunde gegangen sind. Aber auch hier fehlt wieder nicht die Verfettung der Epithelien und des Zwischengewebes. Das buntscheckige Aussehen der Oberfläche rührt eben daher, dass die mehr fettigen Parthien in Form weisser Flecke, sich vorthun, oder auch, wo die Verfettung, namentlich die Glomeruli betrifft, in Form von Sprenkeln (*spicules*), welche der Anlass geworden sind, die "gesprenkelte Niere" wieder als besondere Form zu statuiren. Nur in der Gruppirung und Ausbreitung der Erkrankung der einzelnen Gewebselemente differiren also diese Zustände. In denjenigen Nieren, welche man als "interstitielle Nephritis" par excellence bezeichnet, fehlen ebenso wenig die parenchymatösen Veränderungen, als die interstitiellen in den grossen weissen vermisst werden. Das eine Mal überwiegen die einen, das andre Mal die andren Alterationen. Wie es scheint, ist namentlich die Dauer des ursprünglich in beiden gleichen Grundprocesses der Entzündung von Einfluss auf das überwiegende Ergriffensein des einen oder anderen Gewebselementes. Wo wir artificiell z. B. durch Cantharidin die Nephritis produciren, sehen wir in den Interstitien nur zellige Infiltration, ausgebreitetere Veränderung der Epithelien in den Kanälchen, weil der Eingriff nicht lange vom Versuchsthier überlebt wird, wo aber, wie bei einseitiger Ureterenunterbindung, die, lange überlebt wird der Entzündungsprocess mehr chronisch verläuft, da treten die Reizungszustände des interstitiellen Gewebes, je länger je mehr in voller Entwickelung auf. Und auch bei den Menschen sehen wir die eigentliche "granulöse Schrumpfniere" in der die Veränderungen des Zwischengewebes den höchsten Grad erreicht haben, nur da auftreten, wo die Krankheit von Jahre langer Dauer gewesen ist. Die beiden Arten der Schrumpfniere, welche man "als kleine weisse" und "kleine rothe" nach ihrem äusseren Verhalten unterscheidet, sind hart von Consistenz, klein von Volumen, leicht von Gewicht, uneben auf der Oberfläche, häufig von kleineren Cysten durchsetzt. In der weissen sind die Granula meist noch grösser als in der rothen, schliessen noch mehr glatte Partien zwischen sich und zeigen weniger Cysten an ihrer Oberfläche als die rothe. Doch sind diese Unterschiede sehr *cum grano salis* aufzunehmen. Auch kleine weisse Nieren kommen vor deren Oberfläche mit feinsten, kaum Stecknadelkopf grossen Höckerchen besät ist, die dicht aneinander stehen und durch keine breiteren Bindegewebszüge von einander geschieden sind. Cysten findet man nicht selten die Niere in grösserer Zahl durchsetzen, wenn ihr Volumen noch lange nicht hochgradig verringert ist. Die histologische Untersuchung aller Schrumpfnieren—weisser und rother—zeigt auch in gleicher Weise den ausgebreiteten Parenchymdefect, der durch fibrilläres Bindgewebe ersetzt ist; da, wo die Kanälchen noch erhalten sind, deren Epithelien zum Theil wohl bewahrt, zum Theil fettig und die sie begrenzenden Interstitien mit zelligen Elementen erfüllt; die Glomeruli sind vielfach in fibröse Kugeln umgewandelt, in denen von den Schlingen keine Spur mehr zu sehen ist, und daneben wieder andere, die entweder noch intact, oder ebenfalls schon verkleinert in stark verdickten und geschichteten

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Kapseln liegen. Gerade in diesen Nieren kann man oft nebeneinander alle Stadien der Glomerulusveränderungen beobachten. Nicht selten sieht man an dem noch normal grossen Glomerulus einen Theil der Gefässschlingen schon ohne bedeckendes Epithel in homogene Masse verwandelt, während die andre Hälfte desselben Knäuels noch intact ist, und in andren wieder die Verdickung der Kapsel schichtweise im Verhältniss zur Verkleinerung des Gefässknäuels fortschreitend. Ausser den genannten Veränderungen sieht man aber in allen Schrumpfnieren die mittleren und kleinen Arterien sehr auffällig am Processe Theil nehmen. Die von Johnson zuerst beschriebene Hypertrophie der Muscularis der Gefässe ist unstreitig eine beinahe nie fehlende Alteration—nur in den kleinen rothen sieht man auch die Verdickungen der Intima so in den Vorgrund treten, dass die Muscularis selbst atrophisch erscheinen kann. Ob die von Gull and Sutton beschriebene Gefässaffection, welche in der Ablagerung einer hyalinfibroiden Substanz nach aussen von der Muscularis bestehen soll, nur, wie Johnson will, ein Kunstproduct ist, kann ich auf Grund eigner Versuche noch nicht entscheiden. Dass aber muss ich betonen, dass, wenn diese Veränderung vorkommt, sie äusserst selten sein muss, da ich wohl nicht selten Vermehrung von Bindegewebe sehr reichlich um die Gefässe gesehen habe, nicht aber eigentlich was die genannten Autoren beschreiben, namentlich die hyaline Substanz in und um die Gefässe. Mag sie also auch, da ein so genauer Beobachter wie Leyden ihr Vorkommen zugiebt, zuweilen beobachtet werden; sie ist keinesfalls so häufig, um mit ihr als einem, den Schrumpfnieren zukommenden gewöhnlichen Vorkommniss zu rechnen.

Auf Grund der histologischen Veränderungen wird man, da die gleichen Alterationen aller einzelnen Gewebselemente, nur in verschiedner Vertheilung der Ausbreitung, auch in der grossen weissen und der glatten gefleckten Niere vorkommen, principiell auch die Schrumpfniere nicht von diesen trennen, sondern in ihr nur das meist vorgeschrittene Product desselben Processes der Entzündung sehen, der in seiner langen Dauer die höchste Entwicklung in der sogenannten "gemeinen Schrumpfniere" erreicht hat. Denn principiell differente anatomische Veränderungen zwischen der sogenannten "secundären" und "gemeinen" Schrumpfniere kann ich nicht erkennen. Nur ein Moment ist vorhanden, welches allerdings für die genetische Verschiedenheit einer Form von Schrumpfniere spricht, ich meine, die endarteritischen Veränderungen. Zwar habe ich solche im höchsten Grade der Entwicklung, beinahe bis zum Verschlusse des Gefässlumens führend, in einem Falle gesehen, wo nur eine sehr geringe zellige Infiltration der Interstitien und trotz reichlicher Schrumpfung von Glomeruli der Grad der allgemeinen Atrophie nicht sehr erheblich war, so dass also nicht immer zwischen der Intensität der Gefässalteration und der Ausbreitung der interstitiellen Veränderungen ein bestimmtes Verhältniss Statt hat. Dennoch ist Ziegler wohl im Rechte, wenn er ausspricht, dass in einem nicht geringen Theile der kleinen rothen Nieren die Schrumpfung die Folge ursprünglicher Arteriosclerose ist. Und es ist gewiss eine treffende Bezeichnung, die Leyden neuerdings eingeführt hat, wenn er von Nierensclerose spricht. Man kann also der Genese nach zwei Formen der Schrumpfniere unterscheiden, von denen die weisse als Endproduct der diffusen Nephritis, die rothe der Arteriosclerose zuzuschreiben ist. Dass auch hier wieder Uebergänge vorkommen, darf nicht übersehen werden, und namentlich die senile Niere steht der letzteren Form so nahe, dass für sie keine besondere Scheidung nöthig ist. Eine amyloide Schrumpfniere als besondere Form zu unterscheiden, sehe ich gar keinen Grund. Denn die Complication der amyloiden Degeneration—es handelt sich nämlich meistens um geschrumpfte Nieren, zu denen sich diese gesellt — beeinflusst nicht das Wesen des Grundprocesses, der zur Schrumpfung Anlass gegeben hat. Sehen wir doch höchste Grade der Gefässentartung ohne Schrumpfung verlaufen, und bei gleichzeitiger Schrumpfung umgekehrt wieder die amyl-

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Entartung in so geringem Masse, dass die Gefässdegeneration nicht als bedingender Factor in der Genese der entzündlichen Alterationen betrachtet werden kann.

Auf Grund der histologischen Betrachtungen komme ich zu dem Schlusse, als anatomische Grundlage des Morbus Brightii die "diffuse Entzündung" der Niere anzusehen, welche im Anfange, gerade wie die Entzündung jedes anderen Organes, mit Veränderungene der Circulation beginnt, und sich durch Austritt von Lymphkörperchen characterisirt. Gleichzeitig werden vom entzündlichen Reize—wahrscheinlich eines veränderten Blutes—auch die epithelialen Elemente der Glomeruli und Harnkanälchen getroffen. Während beim Beginne noch kein Beweis für eine vom interstitiellen Gewebe selbst ausgehende Kernproliferation zu erbringen ist, wird im weiteren Verlaufe solche allerdings sowohl an den Kapseln der Glomeruli, als auch zwischen den Harnkanälchen gesehen, so dass die schliesslich zu unserer Beobachtung kommenden Nieren der Brightiker stets Veränderungen aller Gewebselemente zeigen und man darum, auch wenn das eine Element in seiner Alteration mehr in den Vorgrund tritt, als das andere, doch nicht von "parenchymatöser" oder "interstitieller" sondern nur von "diffuser Nephritis" reden darf. Nur ein "Mehr" oder "Minder," ein Ueberwiegen des Grades der Veränderung des einen oder anderen Gewebselementes unterscheidet die äusserlich so verschiedenen Formen der "rothen geschwollenen" "grossen weissen" der "gefleckten" und "gesprengelten" sowie der weissen "Schrumpfniere." Nur eine Form der Schrumpfniere ist als genetisch verschiedene aufzufassen; nämlich die arteriosclerotische, in der die Schrumpfung von der Sclerose der Gefässe ihren Ausgang nimmt.

Die Frage, die sich nun unmittelbar an diese Erörterungen knüpft, ist die, ob etwa die klinische Wahrnehmung Zeichen an die Hand giebt, vermöge deren wir im Stande wären zu erkennen, ob im Einzelfalle die Entzündung sich auf die Epithelien oder das interstitielle Gewebe erstreckt; oder ob wir vielleicht im Stande wären nach den Symptomen, ohne vorangehende Kenntniss des ätiologischen Momentes, eine dieses verrathende bestimmte Form des M. Brightii zu erkennen, aus den ersteren letzteres zu schliessen?

Einzelne Beobachter haben gemeint, dass es möglich sei, einerseits diejenige Form zu erkennen, bei der vorwiegend epitheliale Veränderungen anwesend sind und andererseits, dass man namentlich die, aus dieser hervorgehende secundäre Schrumpfung von der s. g. "genuinen Schrumpfung" im Leben unterscheiden könne. Die differentiellen diagnostischen Momente sollen hauptsächlich in der Anwesenheit oder Abwesenheit von Oedemen einerseits, und in dem hohen oder niedrigen Eiweissgehalte des Harns andrerseits, gelegen sein. Eine unbefangene Beobachtung kann dies aber nicht bestätigen. Die exquisitesten, grobkörnigen, weissen Granularnieren habe ich ohne Oedem und mit spärlichem Eiweissgehalte des Harns verlaufen sehen, und ganz feine granuläre Atrophie, deren Beginn mit starkem Hydropsien constatirt war Bilder glatter Schrumpfniere werden wohl jedem vorgekommen sein, deren Symptomecomplex buchstäblich dem Schulbilde der s. g. parenchymatösen Nephritis entsprach.

Abgesehen davon, dass also die Thatsachen nicht für eine solche Scheidung der genannten Formen sprechen, so ist auch von theoretischem Standpunkt aus eine derartige Begründung der An-oder Abwesenheit der Oedeme, als eines werthvollen diagnostischen Zeichens, ganz unmöglich. Das Verhalten der Oedeme ist ein so wechselndes, dass uns vorläufig für ihre Genese und Bedeutung jede Erklärung fehlt. Bei einer grossen Zahl von Brightikern sehen wir so wohl Anasarca als Hydropsien innerer Höhlen sehr bald schwinden, so wie sie nur einige Zeit hindurch das Bett gehütet haben, und bei anderen wieder bleibt das Anasarca, namentlich das Oedema faciei vom Beginne bis zum Ende der Erkrankung bestehen, wie auch das diaetetische Verhalten wechseln möge, wie auch das 24 stündige Harnvolum schwanke,

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und welche Behandlung auch instituirt werde; bei wieder anderen kommen und gehen sie in buntem Wechsel ohne nachweisbaren Grund, höchstens in ganz acuten Fällen dem Umfange der Diurese parallel laufend. Mit diesen Thatsachen stimmt vorläufig keine der gangbaren Erklärungen, auch nicht die von Cohnheim supponirte Veränderung der Hautgefässe als Ursache des Anasarca. Denn weder begreifen wir hierdurch die mit dem Anasarca gleichzeitig schnell sich entwickelnden Hydropsien der serösen Höhlen (Hydrothorax, Hydropericardium, Ascites) die auch bei den acuten Formen fast immer vorhanden sind, noch auch die locale Vertheilung des Anasarca, das bei einzelnen nur die Augenlider, bei anderen das ganze Gesicht, bei wieder anderen den ganzen Körper und ausnahmsweise, wie ich ebenfalls gesehen, nur die Conjunctiva bulbi oder das Präputium befällt. Wenn wir also theoretisch gar nicht begründen können, warum das eine Mal Oedeme vorhanden sein und das andre Mal fehlen müssten, so dürfen wir nur eben die Thatsachen registriren. Diese zeigen aber nur, dass bei Schrumpfungszuständen Oedeme ungleich seltner getroffen werden, als da, wo die Atrophie der Nieren entweder überhaupt noch nicht oder nur in geringem Grade zu Stande gekommen ist. Die An oder Abwesenheit der Oedeme belehrt uns also nicht über eine bestimmte Form der Gewebserkrankung, sondern ebenso wie die Beschaffenheit des Harns und die Zeichen des Circulationsapparates über die schon oder noch nicht zu Stande gekommene Schrumpfung. Wie diese Schrumpfung zu Stande gekommen, dass erfahren wir aus den Zeichen nicht. Zwar will es mir scheinen als ob man, wenn neben den übrigen Zeichen der Schrumpfung in dem geringen staubförmigen Sedimente des Harns reichlich Körnchenzellen gefunden werden, weisse Atrophie annehmen darf, doch ist hinsichtlich der rothen meine Erfahrung nicht gross genug, um die Sicherheit der Unterscheidung aussprechen zu dürfen. Wäre es aber auch sicher möglich, die weisse und rothe Atrophie klinisch zu scheiden, so muss im Allgemeinen festgehalten werden, dass alle Symptome nur die schon oder noch nicht vorhandene Schrumpfung, also das Stadium des Processes, nicht aber die bestimmte Betheiligung eines Gewebselementes erkennen lassen. Ich sage, Stadium. Denn wenn Bright seiner Zeit es als zweifelhaft hinstellte und nur sagte "It may be that the first form of degeneracy never goes much beyond the first stage, and that all the other cases, together with the second series and the third, are to be considered only as modifications, and more or less advanced states of one and the same disease," so betrachte ich diese Vermuthung als der Wirklichkeit entsprechend. Es ist völlig wahr, dass die Beobachtung der Entwicklung der Schrumpfung aus der hyperämisch geschwollenen, oder der "grossen weissen" gewiss zu den Seltenheiten gehört. Doch sind wir bisweilen unzweifelhaft in der Lage, am Leichentische Uebergänge wahrzunehmen, und die klinische Beobachtung hat mich in einzelnen Fällen mit voller Sicherheit die Uebergänge aus der grossen in die geschrumpfte Niere nach den Symptomen verfolgen lassen, indem der sparsame, schwere Harn je länger je reichlicher und leichter wurde, und unter meinen Augen Hypertrophia Cordis und Retinitis sich entwickelte. Ich halte auch den Einwand, dass uns die Kranken mit geschrumpften Nieren fast nie von früheren Oedemen oder Verringerung der Harnausscheidung erzählen, nicht für durchschlagend, denn die oft auch bei geschwollenen grossen Nieren nur geringfügigen Oedeme längs der Tibia und circa malleolos werden oft übersehen und nicht allzu beträchtliche Verringerungen der Diurese entgehen der Aufmerksamkeit des Kranken eher, als der nächtliche Harndrang, der ihn bei geschrumpfter Niere belästigt und dass grosse Harnvolum. Nach dem, was ich auf dem Leichentische und am Krankenbette gesehen habe, betrachte ich, wie das Reinhardt und Frerichs thaten, die weisse granuläre Atrophie als letztes Stadium der diffusen Nephritis. Anders ist es mit Entwicklung der rothen Atrophie, bei der die Arteriosclerose den Ausgangspunkt bildet. Von ihrer Entwicklung ist es im hohen Masse wahrschein-

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lich, dass die Schrumpfung von Anfang an nicht durch primäre Entzündung eingeleitet wird und daher auch nie ein Stadium der Vergrösserung ihr vorangegangen ist.

Betreffs der zweiten Frage, ob die Symptome, je nach der Verschiedenheit des ätiologischen Momentes, so verschieden sind, dass wir danach differente Formen annehmen müssen, so muss ich auch das verneinen. Aus den Symptomen erkennen wir, ob ein M. Brightii acut oder chronisch auftritt, ob er aber acut sich entwickelt nach Scarlatina, Typhus, oder Intermittens, ist aus den Symptomen allein völlig unerkenntlich. Der Zufall hatte mir einmal alle drei neben einander gelegt, die Symptome (Hämaturie, verringerte Diurese, Oedeme, &c. &c., Micrococcen im Harn und auf Cylindern) liessen die acute Nephritis erkennen, nicht aber wodurch bedingt, wenn ich nicht das vorangegangene Leiden selbst beobachtet hätte. Und ebenso ist es mit der chronischen Form; ist die Schrumpfung da, dann erkennen wir diese, aber sehen ihr nicht an, ob ursprünglich Malaria vorangegangen oder Rheumatismus, oder Eczem,—vorausgesetzt natürlich, dass wir es immer nur mit denjenigen Affectionen zu thun haben, die wir M. Brightii nennen, nicht also mit amyloider Degeneration oder käsiger oder suppurativer Nephritis. Auch vom ätiologischen Standpunkte aus sehe ich keine Berechtigung zur Annahme verschiedener Formen des M. Brightii. Nicht also das ätiologische Moment, nicht die Erkrankung eines bestimmten Gewebselementes der Niere (Epithelien, interstitielles Gewebe, Gefässe) bestimmt in erster Reihe die Unterscheidung verschiedener Formen, sondern vor Allem der Verlauf und der Grad bis zu dem der entzündliche Process vorgeschritten ist und danach bereits Schrumpfung des Organes hervorgerufen hat oder nicht. Dem entsprechend möchte ich, alles Gesagte zusammenfassend, vorschlagen, nachdem man eins geworden ist, die amyloide Degeneration, die cyanotische Induration und die Graviditäts-Nephritis aus dem grossen Rahmen des M. Brightii auszuscheiden, nur einen Entzündungsprocess, der alle Elemente der Niere afficirt, als anatomisches Substrat des M. Brightii zu betrachten, und zwei Formen nach dem klinischen Verlaufe zu unterscheiden, die acute und die chronische, —nach dem anatomischen Producte die beiden Zustände der "geschwollenen, grossen" und der "geschrumpften kleinen" zu unterscheiden. Von der letzteren dürfte sich die Abgrenzung der arterio-sclerotischen als besondere Form noch empfehlen, weil sie wahrscheinlich auch der Erkentniss während des Lebens, als von den anderen unterschieden, zugängig sein wird.

On the Diagnosis of the so-called Glomerulo-Nephritis of Klebs.

Dr. George Johnson, F.R.S., London.

Dr. E. Klebs, in his "Handbuch der Pathologischen Anatomie" (pp. 644–647), describes peculiar structural changes affecting the kidney in some cases of scarlatinal nephritis, to which he has given the name of "glomerulo-nephritis."

Klebs states that in the cases in question the intra-tubular changes usually found in cases of scarlatinal nephritis are absent, but he describes the interior of the Malpighian capsules as being filled with a number of small angular nuclei imbedded in a finely granular mass. These nuclei he believes to result from proliferation of the corpuscles of the connective tissue, which binds together the capillaries of the Malpighian tufts. In kidneys affected by glomerulo-nephritis, he describes an enormous increase in the number of these corpuscular bodies, which so completely fill the Malpighian capsules as to compress and even empty the capillaries, thus seriously impeding the circulation and

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secretion, and causing dropsy and uræmia. According to Klebs, this intra-capsular accumulation is the only structural change observable in these cases.

The observations of Klebs have been confirmed in their main points by Dr. Klein,* and by Dr. Bryan Charles Waller;† but both these observers describe and demonstrate, in addition to the intra-capsular accumulations mentioned by Klebs, certain interstitial changes outside the capsules, and in the inter-tubular stroma.

I am indebted to Dr. Waller for the opportunity of showing some stained sections of kidneys, in which all the changes described in his paper may be clearly seen.

Dr. Waller considers that the cell-accumulations within Bowman's capsules have two sources: firstly—from direct proliferation of the connective tissue corpuscles between the Malpighian capillaries; and secondly, from the migration of leucocytes, which have passed from the blood through the walls of the capillaries.

The migratory theory receives confirmation from the fact that corpuscles exactly similar to those within the capsules are found outside the capsules, and around some of the arteries and small veins of the cortex. Further, the lumen of many cortical capillaries is seen to be occupied by numerous leucocytes, which are often so closely aggregated that the circulation through them must have been seriously retarded, if not completely arrested. This cramming of the vessels with leucocytes, apparently ready to migrate, affords, as Dr. Waller maintains, additional evidence of the migratory source of those cells which have accumulated outside the cortical vessels as well as within the Malpighian capsules.

Dr. Waller states that the intra-tubular changes are limited to those portions of tubules which are in the immediate neighbourhood of the Malpighian bodies, and consist chiefly of fatty degeneration of the tubular epithelium, though cloudy swelling and granular degeneration were also observed in a few instances.

I now pass on from the consideration of the morbid anatomy of the kidney to the clinical diagnoses of this class of cases.

During the last quarter of a century, at least, I have often observed and demonstrated microscopical appearances in the urine, which it now appears have an obvious relationship to the structural changes in the kidney, designated glomerulo-nephritis by Klebs. In some cases of acute albuminuria the urine deposits a sediment containing numerous tube-casts crowded with leucocytes. The casts are for the most part of small size, indicating that they have been moulded within the lumen of uriniferous tubes which possess their normal lining of epithelium.

In my "Lectures on Bright's Disease," published in 1873, I give a woodcut illustration of these tube casts, and in explanation of them I state (pp. 34–35) that I had not been able by a microscopic examination of the kidney to ascertain the source of the small round cells thus entangled in the tube-casts; but that "since the publication of Cohnheim's researches it has occurred to me that these exudation cells may probably be white blood-cells—leucocytes—which have migrated through the walls of the Malpighian capillaries, and subsequently have become moulded into small cylindrical casts within the central canal of the uriniferous tubes."

These clinical observations of mine, which were made and published long before I had any knowledge of Klebs's anatomical researches, have evidentally an intimate supplementary relation to them. The white-cell casts in the urine observed during life are the counterparts and the diagnostic clinical indication of the intra-capsular accumulations which are found in the kidneys after death. I have observed the white-cell casts

* *Path. Trans.*, vol. xviii., 1877.

† *Journal of Anatomy and Physiology*, vol. xiv. p. 432.

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in numerous cases, both in hospital and in private practice, during the last quarter of a century. In some cases the white-cell casts have existed alone, while in others they have been associated in variable proportions with the epithelial casts indicative of the intra-tubular changes which constitute acute desquamative nephritis. It is evident, therefore, from clinical observation, that the two conditions designated respectively "glomerulo-nephritis" and "desquamative nephritis" may either occur separately or they may co-exist in the same subject.

In some instances the disease has been fatal with acute dropsy and uræmia, while in others it has passed into a state of chronic albuminuria; but in most of the cases that have come under my observation complete recovery has taken place, and the urine has lost all trace of morbid products. The prognosis in cases of glomerulo-nephritis as indicated by the microscopic character of the urine is, I believe, not more unfavourable than in ordinary cases of acute desquamative nephritis.

Then, as regards etiology, there appears to be no material difference between the intra-capsular and the intra-tubular form of disease. While some of the cases that I have seen have been associated with scarlet fever, others have resulted from exposure to cold and wet, and the other well-known exciting causes of acute desquamative nephritis.

With our present more exact knowledge of the significance of the white-cell casts in the urine, and of their relationship to the intra-capsular accumulations in the kidney, we shall, in future, find it easier to collect precise data relating to the etiology as well as the prognosis of this form of renal disease.

Chronic Bright's Disease without Albuminuria.

Dr. F. A. Mahomed, London.

This paper contains a short account of some observations about to appear at length in the *Guy's Hospital Reports*. The object in recording them has been to show that the condition of high arterial pressure does not follow but precedes the organic changes of chronic Bright's disease; and further, that high arterial pressure, the cardio-vascular changes of Bright's disease, and the red granular kidney, are not usually associated with albuminous urine. Seven years ago the author demonstrated a pre-albuminuric stage of high arterial pressure in the acute nephritis of scarlatina (*Trans. Royal Med. Chirurg. Soc.* 1874); and all his observations since have served to confirm this; it was then also pointed out that many conditions which predisposed to chronic Bright's disease presented this symptom, while the signs of kidney disease were as yet absent. Two years ago he recorded several cases in which high arterial pressure existed in apparently healthy individuals as a chronic condition, and drew the conclusion that the arteries in these cases must eventually thicken and the hearts hypertrophy. Several cases were recorded at the same time (*Guy's Hospital Reports*, 1879), dying from heart failure, who had granular kidneys, but in these cases the urine gave no indications; and in the present paper it will be shown that all the cardio-vascular changes and symptoms which accompany chronic Bright's disease not unfrequently occur in cases in which there are little, if any, kidney changes of an inflammatory nature.

The difficulty then arises, should these cases be described as a part of Bright's disease, or, on the other hand, do they form a distinct pathological entity, and should they be described as another and a new disease? It is true that Bright described certain changes in the kidney associated with albuminous urine, and that neither the kidney changes nor the albuminous urine are present in these cases; yet reasons can be found for believing that inasmuch as these are all stages in one series of morbid processes, that

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as they are interchangeable and dependent on each other, they may rightly be regarded as parts or stages of one great general disease.

The cases on which this paper is founded are sixty-one in number; twenty of them were fatal, and the results of the post-mortem examinations are given.

In addition to their more prominent symptoms for which they sought relief, nearly all these cases presented the following characteristics which led to their diagnosis:—They all had the signs of high arterial pressure; they all had very considerable hypertrophy of the heart; those cases only have been accepted in which the apex beat was in the nipple line, or external to it; in many the arteries were tangibly thickened; in all the cases the urine was free from albumen while they were under observation. In most of the cases it was altogether free; in eleven cases it was present on one or two rare occasions during a long period of careful observations; in these it usually occurred immediately after admission to hospital, and during the time they were severely ill; in three other cases, though absent during long periods of observation, it occurred just previous to death; in three cases of typical chronic Bright's disease, the patients were admitted with albuminuria which disappeared under treatment, and they left without it. Three cases had urine very variable in its character, sometimes albuminous, sometimes not. In the remaining forty-one cases albumen was never discovered in the urine.

The exceptions to well-marked hypertrophy of the heart were seven in number—in three of the fatal cases, in one—a case of phthisis, in which there was great wasting—the heart only weighed 11 oz., but the kidneys were very granular and the arteries thick; in another—a case of severe arterial disease, with much disease about the commencement of the aorta—the heart weighed only 12½ oz., the body was spare and muscles small; in another case of a female, with much wasting, the heart weighed only 12 oz., the kidneys being however markedly granular and the vessels thick. In all the other cases the hearts varied in weight from 15 oz. to 30 oz. Of the cases which were not fatal, in four the hypertrophy was not proven by displacement of the apex beat; in one of these there was renal dropsy and occasional albuminuria; in another bronchitis and emphysema, with severe epistaxis, the arterial pressure was extremely high, and the arteries tortuous, hard, and thick; the other two were ordinary and generally accepted cases of Bright's disease, in which the albumen disappeared while under treatment.

Of the twenty-one fatal cases all had thickening of the arteries visible to the naked eye; all had well-marked hypertrophy of the heart, with the exception of the three cases mentioned above; in ten cases the kidneys were of the ordinary red, wasted, and granular variety. Two were large and granular, but proved on examination to have merely the vascular changes to be mentioned hereafter, with very little, if any, small-celled (inflammatory) growth. In three the kidneys were of the mixed or yellow granular variety. In six cases the kidneys appeared perfectly healthy to the naked eye, of good size, with smooth surfaces and thin capsules; in all of these, however, it is noted by Dr. Fagge and Dr. Goodhart (who made the post-mortem) that the small arteries were thickened and prominent; in three of these the microscope showed thickening of vessels, Malpighian capsules and stroma; the other three were unfortunately not examined microscopically.

These sixty-one cases have been arranged in eight groups, according to their most prominent symptoms; they are as follows:—

1.—Cases of heart failure, ten, of which eight were fatal; all without valvular disease, beyond the thickenings commonly associated with hypertrophy. All of these cases had hypertrophy and dilatation of the left ventricle; in five cases there was severe aortitis deformans.

2.—Cases of lung failure, eleven, of which six were fatal. Eight of these were cases of bronchitis, one of pleurisy, one of gangrene of the lung, and one of phthisis.

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3.—Cases of cerebral disease, nine, of which two were fatal. Of these six were cases of hemiplegia from cerebral hæmorrhage, two cases of softening, and one of headache and vertigo, with attacks of unconsciousness.

4.—Cases of renal dropsy, without albuminuria, nine, of which only one was fatal. Two of these had a trace of albumen on one or two rare occasions. They form a remarkable and very interesting series.

5.—Cases with gout, six, of which none were fatal. Well-marked cases of gout, with all the symptoms of chronic Bright's disease, but with normal urine.

6.—Cases with severe epistaxis, three, of which none were fatal. A very characteristic group.

7.—Cases with various medical and surgical diseases, nine, of which four were fatal. All under observation for other disease, but all found either clinically or at the post-mortem to be suffering from the cardio-vascular changes of Bright's disease.

8.—Cases of typical chronic Bright's disease, with well-marked albuminuria, which was variable or disappeared while under treatment; in other words, cases with undoubtedly bad kidneys passing non-albuminous urine.

Such are the cases. They all came under notice within the brief period of two years. The great lesson to be learned from them appears to be that many cases occur in which the cardio-vascular changes of Bright's disease (arterio-capillary fibrous) are to be recognized clinically or found post-mortem, in which the urine gives no sign, though usually, but not necessarily, there are some changes in the kidneys. Are these cases to be called Bright's disease? I believe that they should be included in this term, though its present somewhat loose and too indefinite use might with advantage be modified. Chronic Bright's disease, when it runs from the commencement an insidious and chronic course, should, I consider, be divided into three stages—first, the "functional stage of Bright's disease," in which high arterial pressure, but no organic changes, such as hypertrophied heart or thickened arteries, can be detected; second, "chronic Bright's disease without nephritis," in which the cardio-vascular signs are to be found, but the urine is normal; third, "chronic Bright's disease with nephritis," the typical well-known condition, which is at present recognized. To believe in this sequence of events, it is necessary to modify Bright's views, and those generally held, in one particular only—namely, to admit that the high arterial pressure, and consequent cardio-vascular changes, is produced by a primary change in the blood or tissues, which interferes with the nutrition of the tissues and impedes the circulation of blood through them, instead of maintaining the view that the disease is primarily renal, that the renal failure induces poisoning of the blood, and this, in its turn, the high arterial pressure. The latter view may be the true one in some cases, notably those in which the disease is acute, though even then, especially when it follows scarlatina, it does not appear always to coincide with the sequence of events. That it is always true in the chronic disease seems completely contradicted by many of these cases in which the great hypertrophy of the heart is out of all proportion to the kidney change. If, then, we allow that altered nutrition, due to gout, dyspepsia, alcohol, nerve troubles, or hereditary tendency, may produce high arterial pressure, then it must follow that its chronic persistency will also produce hypertrophy of the heart and thickening of the arteries, upon both of which increased work is thrown; this will be admitting my first and second stages of the disease; in this second stage the patient may die from any of the accidents common to it, as illustrated in the cases recorded, or an acute congestion of the kidneys may supervene, and the case terminate in a nephritis, one of the most common modes of dying among these people. The case then presents the ordinary symptoms of Bright's disease, and yellow or mixed granular kidney is found.

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Professor STOKVIS, Amsterdam: Allow me to make a very short communication on the eminently interesting question, so well introduced in the very able papers you have heard. My communication does not concern the question if there can be in the blood an albuminous substance, which can be considered foreign to the blood, and which passes into the urine. Yet, my honourable friend Semmola will not take it amiss, I hope, if I think his facts not very convincing. If, really, the albumen in the blood in kidney diseases was to appear on diffusion as peptone, it would be necessary that that same albumen should be found in the urine. And injecting that urine with that diffusible albumen into the blood of animals, you should again find it in their urine. Now, I never was fortunate enough to find an albuminous urine, which injected into the blood, resembled the albumen of the egg and passed into the urine. But I will now call your attention to some experiments made last year, under my direction, by Dr. Kimper, and which seem to prove that there is a possibility of producing nephritis by making a very slight change in the composition of the blood. When you introduce into the system of rabbits, by means of subcutaneous injection, egg-albumen in the quantity of 20 to 30, the albumen passes into the urine; and when you stop the experiments, after some days, in most cases the animals will become ill, lose their appetite, and after four or six weeks they become exceedingly meagre. In all cases, post-mortem, there is to be found a real glomerulo-nephritis, much in the sense as that nomenclature is used by Dr. Johnson. In one case during the time there were symptoms which might be considered as uræmic, and in a great many cases hypertrophy of the heart seemed to be present. These experiments are an attempt to maintain the justness of the opinions, once believed by Canstatt, and now again brought forward by my friend Semmola; and I think there is a way in which the two antagonistic parties may be brought together.

De l'Ataxie Paralytique du Cœur, d'Origine Bulbaire.

Professor SEMMOLA, Naples.

1.—Il existe un type clinique de maladie du cœur capable de se présenter avec tous les effets propres aux maladies organiques du cœur (avec insuffisance valvulaire non compensée) sans altérations primitives ni de l'endocarde ni des muscles cardiaques et se trouvant seulement en rapport avec une altération des centres nerveux ganglionnaires et bulbaires du cœur.

2.—Ce type clinique se développe lentement, très-lentement, à la suite des causes épuisantes du système nerveux, qui agissent surtout entre quarante cinq et soixante ans. Les causes principales sont, les émotions violentes et surtout les frayeurs longues et répétées et les excès véneriens hors de saison.

3.—Les premiers pas de la maladie sont signalés par des symptômes d'oppression passagère qui suivent principalement l'action de la cause, par une accélération plus ou moins permanente dans les battements du cœur et après quelque temps, avec la persistance de la cause, par des disystolies récurrentes. Le développement de ces troubles nerveux du cœur est presque toujours précédé ou accompagné par des symptômes dyspeptiques qui prouvent que l'abaissement fonctionnel du vague et du sympathique n'a pas épargné l'estomac.

4.—Si l'action des causes s'arrête dans ces prodrômes il est encore possible de voir peu à peu tout symptôme disparaître et la reconstitution complète se faire dans les fonctions cardiaques. Si, au contraire, les causes persistent, peu à peu les symptômes s'aggravent jusqu'à la mort en passant par toutes les phases des troubles fonctionnels cardiaques produits par les vices valvulaires non compensés et les dégénérescences cardiaques les plus graves.

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5.—Dans toute l'histoire clinique de ces maladies on peut étudier caractéristiquement les effets de l'innervation cardiaque et les symptômes de paralysie vaso-motrice périphérique. En effet ce qui caractérise cette ataxie paralytique du cœur d'origine bulbaire est l'apparition de taches marbrées, violacées sur les mains, sur l'avant-bras et surtout en correspondance des articulations à une époque des troubles cardiaques dans laquelle n'existent pas encore les stases veineuses dues à l'insuffisance valvulaire non compensée.

En même temps se produit une congestion passive pulmonaire et cela, je le répéte, à une époque dans laquelle l'on ne rencontre pas encore des stases veineuses dans le foie ou ailleurs. Cela provient de ce que ces congestions pulmonaires sont d'une origine neuro-paralytique, c'est-à-dire qu'elles sont produites par le trouble central du pneumo gastrique et des centres bulbaires vaso-moteurs.

6.—La lésion anatomique que j'ai rencontrée dans les cas dont on a pu faire l'autopsie, a été la dégénérescence graisseuse des ganglions cardiaques et l'atrophie avec dégénérescence pigmentaire des noyaux bulbaires du vague et des noyaux vaso-moteurs.

7.—La dégénérescence graisseuse des muscles cardiaques n'est qu'un effet des troubles profonds produits dans leur nutrition à la suite de l'épuisement nerveux. Il arrive dans cette maladie ce qui arrive pour les muscles de la langue dans la paralysie glosso-labio-laryngée.

8.—Comme traitement des troubles fonctionnels les sels de quinine et la digitaline représentent dans ces cas des remèdes d'un effet surprenant, et, comme je l'ai dit plus haut si l'action des causes cesse et que les troubles ne sont pas de longue date, l'on peut compter sur une réhabilitation complète, parce que la nature dans sa haute prévoyance a donné au bulbe une grande force de résistance tout justement parce qu'il tient dans ses mains l'existence de la machine animale. Mais si les symptômes cardiaques datent depuis longtemps et principalement quand on a constaté l'existence de stases périphériques d'origine névro paralytique, l'on ne peut pas avoir d'autre résultat que celui de remettre momentanément l'équilibre dans le mécanisme de la circulation, et, malgré la continuation des méthodes thérapeutiques les plus actives, l'on voit les fonctions du cœur s'éteindre lentement avec celles de la respiration sans aucun espoir de reprise. Une chose dignede remarque, c'est que, dans ces cas, tandis que d'un côté l'effet utile de la digitaline est vraiment étonnant par sa rapidité, d'un autre côté les symptômes d'intolérance surviennent bien plutôt et à des doses bien plus faibles que lorsque on emploie la digitaline pour éloigner la disystolie dans les cas d'insuffisance par maladie valvulaire et avec intégrité des centres nerveux.

9.—J'ai essayé en vain dans ces cas le traitement par le phosphore, par la strichnine et par le courant électrique constant. Je crois même avoir observé que les effets du phosphore et de la strichnine doivent être épargnés, le premier parce qu'il favorise les dégénérescences graisseuses, et la seconde parce qu'elle produit des excitations sur d'autre territoires nerveux, en produisant un désaccord complet avec le degré d'activité possible dans l'innervation cardiaque.

10.—Quant au régime j'ai toujours observé que lorsque les troubles fonctionnels du cœur ont atteint les effets de l'insuffisance non compensée il est absurde d'insister systématiquement sur l'alimentation trop reconstituante et excitante parce qu'elle ne ait qu'augmenter les désordres de la circulation et elle empêche même complètement les effets utiles de la digitaline. Il arrive dans ces cas que cette espèce de régime excitant force avec violence la fonction cardiaque; et l'excitation fonctionelle (c'est-à-dire, un surcroit de travail musculaire) que le cœur doit subir pendant les premiers effets de ce régime dans l'estomac consomme d'avance ce que l'on prétend réparer plus tard, et se fait aux dépens d'une aggravation dans la maladie principale, et paralyse complète-

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ment les effets favorables qu'on présumait pouvoir obtenir sur l'puisement nerveux primitif et sur les troubles nutritifs nerveux qui en dérivent. Dans ces cas le régime toujours préférable est le régime lacté rigoureux, dans la vraie acceptation du mot, et quand on croit arrivé le moment d'ajouter quelques œufs ou quelques morceaux de viande, il faut toujours le faire en fractionnant les repas, c'est-à-dire en donnant un peu d'aliment toutes les trois ou quatre heures pour rendre le travail de la digestion le plus tranquille possible, et atténuer tout retentissement sur les fonctions du cœur qui ressent les minimes impressions.

The Analytical Study of Auscultation and Percussion, with reference to the Distinctive Characters of Pulmonary Signs.

By Professor Austin Flint, M.D., New York.

An offering of homage to the memory of Laennec is a fitting introduction to a paper having for its aim promotion of our knowledge of physical diagnosis.

Laennec was not the first to listen for intra-thoracic sounds. Mention is made in the writings of Hippocrates of at least one auscultatory physical sign; and the prophetic intimation of Robert Hook, in 1705, has been often quoted.* Doubtless hundreds, before the time of Laennec, had applied the ear to the thorax, and heard pulmonary as well as cardiac sounds. But it was reserved for Laennec to study these sounds in order to discover the physical signs of different diseases, and by prosecuting this study to create an important epoch in the history of medicine.

Homage is also due to the author of the work entitled, "A new Method for the Recognition of Internal Diseases of the Chest by the Percussion of this Cavity," which appeared more than half a century before the publication of Laennec's treatise on auscultation. It does not detract from the honour which belongs to Avenbrugger, that an adequate recognition of the value of the method of examination which he originated, followed, and was in a great measure attributable to, the labours of Laennec in behalf of auscultation.

The zeal and the industry as well as the genius of Laennec are evidenced by the accuracy of his descriptions of auscultatory phenomena, and by the fact that the verity of the physical signs which he discovered has, in the main, been confirmed by subsequent observers in all countries. That he should have cultivated this field of study so thoroughly as to gather all the products which it is capable of producing, was not to have been expected. The marvel is that he was able to render it so productive by his own labours during his short life. It is no disparagement to say that he was led into some errors, that his mode of study was in certain respects defective, and that parts of the field were left uncultivated.

Since the time of Laennec much has been added to our knowledge of auscultation and percussion. It must be said, however, that the enlargement of the scope and the increase in the precision of their application to diagnosis, have not been commensurate with the study given to them, and with the space which they have filled in medical literature. A

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* "Who knows but that one may discover the works performed in the several offices and shops of a man's body by the sound they make, and thereby discover what instrument or engine is out of order."

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considerable share of the attention which they have received has been directed to the mechanism of physical signs—a highly interesting branch of inquiry, but not essential to the practical utility, and involving much liability to error. The number of signs has by some writers been needlessly increased. There have been over-refinements of description and of interpretation. The nomenclature has been open to criticism. Names have not been used by different writers with uniformity as regards signification. The names applied to some signs have conveyed not merely imperfect but erroneous ideas. Some writers have even designated signs by the names of authors who have described them. Hence it is that the study of auscultation and percussion, and their practical employment in diagnosis, have seemed to involve peculiar difficulties, and to be necessarily restricted to a few practitioners. It is common enough for physicians to say, without any sense of self-reproach, that they do not profess to be adepts in physical diagnosis, and to consider with complacency that it properly belongs to a specialty. As opposed to this view, I claim that by a simple method of study, which, for the sake of distinction, I have called analytical, the characters distinctive of physical signs are rendered clear, precise, and readily appreciable, so that the practical advantages of auscultation and percussion may be made available in diagnosis with a moderate amount of time and attention on the part of the student and the practitioner.

By the analytical method of study, I mean the analysis and comparison of physical signs in respect of the few obvious points of difference by which, practically, musical and other sounds are commonly discriminated. The most important of these points of difference relate to the intensity, the pitch, and the quality of sounds. It is unnecessary to define these terms, except to say that under the name quality I include all the differences in character which are exclusive of pitch and intensity. The innumerable variations embraced under the name quality, as thus defined, may be illustrated by the diversities of the human voice. Of many thousand persons, few, if any, are to be found with voices so alike as not to be distinguishable from each other, aside from differences relating to pitch and intensity. In the study of the signs furnished by auscultation and percussion, the differential points, in addition to those pertaining to intensity, pitch, and quality, are few and easily appreciated. They relate to apparent distance from, or nearness to, the ear, moisture or dryness, the rhythmical succession and the interruption of the continuity of sounds.

It is to be assumed that morbid physical signs represent morbid physical conditions, and not diseases—that is, they are diagnostic of the latter only in so far as the physical conditions which they represent are characteristic of particular diseases. It is also to be assumed that the sole reliable basis of our knowledge of the significance of the signs is experience. Certain morbid signs denote particular abnormal morbid conditions, because the former are found to be constantly associated with the latter. The only solid foundation of the knowledge which underlies the practical application to diagnosis of auscultation and percussion, therefore, is in clinical and autopsical observations. It is, of course, desirable to ascertain the mechanism of the signs, but it is by no means a *sine quâ non* in order to establish their validity. For example, is the so-called bronchial respiration due to consonance, according to the theory of Skoda; or is it produced by the current of air within the bronchial tubes, as held by Laennec; or is it the laryngo-tracheal respiration conducted by solidified lung? These questions need not be answered in order to appreciate the significance of the sign, or to recognize it by means of its distinctive characters. To infer from the acoustic characters of signs that, according to the laws of physics, certain morbid conditions must exist, or, on the other hand, to determine *d priori* the signs which should be represented by certain conditions, has proved, and will continue to prove, a source of fallacies. The endeavour to make the

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laws of acoustics the basis of the clinical significance of physical signs, has tended, as it seems to me, to retard not a little the advancement and diffusion of the practical knowledge of auscultation and percussion. Basing the significance of signs on experience, the analytical method of study protects against fallacies which must occur if it be assumed that the abnormal sounds contain intrinsic evidence of the nature of the physical conditions which they represent, or if it be considered indispensable to ascertain fully the mechanism of signs. By attempting to deduce the significance of sounds from their acoustic characters, the play of the imagination and the bias of preconceived notions cannot fail to lead to error.

It is a trite statement that the point of departure for the study of morbid physical signs is the study of healthy signs, inasmuch as the former are either deviations from, or additions to, the latter. But it may not be amiss to state, as a conclusion resulting from an experience of more than a quarter of a century in practical teaching, that neglect of a proper study of healthy signs is the secret of the failure of many who undertake to master auscultation and percussion. Moreover, knowledge of the characters of the more important, and the most difficult to master, of the morbid respiratory and vocal signs, is already obtained when a thorough study has been made of the sounds produced by respiration and the voice over the larynx and trachea, over an area on the chest corresponding to the primary and secondary bronchi, and over the remainder of the thorax.

Proceeding, after these preliminary remarks, to consider the physical signs furnished by auscultation and percussion as determined and differentiated by analytical study, a natural division of the auscultatory signs is referable to, 1st, respiration; 2nd, the loud voice and speech; and 3rd, the whispered voice and speech. Having considered the signs belonging to these divisions, it will remain to consider the signs produced by percussion.

Signs Referable to Respiration.

The number of morbid respiratory signs which require nicety of discrimination is not large. They are among the signs grouped as abnormal modifications of the normal sounds. The adventitious sounds or râles are readily discriminated. The chief cause of confusion and difficulty, as regards the latter signs, has been a needless redundancy of them. The list need not extend beyond the crepitant and the sub-crepitant râle, the coarse and fine moist bronchial or bubbling râles, the sibilant and the sonorous dry bronchial râles pleural friction sounds, gurgling and splashing sounds, amphoric respiration, and metallic tinkling. These signs are readily recognized and differentiated; there is no fault to be found with the names, and the significance of each has been sufficiently established. Of the signs belonging to the group of the abnormal modifications of the normal sounds, suppressed, simply weakened, and interrupted respiratory murmur require no analysis. The remainder of the signs in this group claim analytical study. The latter signs are as follows: 1. Bronchial respiration; 2. Gradatory combinations of the bronchial respiration and the normal respiratory or vesicular murmur, which I include under the name broncho-vesicular respiration; 3. Cavernous, broncho-cavernous, and caverno-vesicular respiration; and 4. Prolonged expiration.

Under the name bronchial respiration, Laennec embraced the normal laryngeal and tracheal respiration, together with the morbid respiratory sign representing solidified lung. He considered them all as essentially identical; and that they are so, is easily demonstrated by analysis and comparison. He distinguished the morbid sign from the normal respiratory murmur by the absence of what he called the slight crepitation, which is characteristic of the inspiratory sound in the normal respiratory murmur—the absence, in other words, of its vesicular quality,—by dryness, and by a sensory impression as if the air

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passed into a large empty space. Laennec did not compare auscultatory sounds in respect of pitch. Skoda, Walshe, Barth, and Roger, in the early editions of their works, made mention of pitch in comparing bronchial respiration with the normal respiratory murmur, without apparently attaching to it much importance. With these exceptions, comparisons in respect of pitch had not, so far as I know, entered into the descriptions of respiratory signs by writers in different countries, prior to thirty years ago, when I was led to the analytical study of these signs with special reference to variations in this respect. The results were published in the "Transactions of the American Medical Association" in 1852.* I hope not to incur the charge of having exceeded the bounds of modesty in claiming, by my studies at that time and subsequently, to have established, on the basis of variations in pitch, characters by which these and other respiratory signs may be positively and easily differentiated.

The normal respiratory murmur and the bronchial respiration may be considered as extremes between which are abnormal modifications other than those pertaining to the latter morbid sign. The differential characters of intermediate signs are to be determined by analytical study and comparison with those of the normal respiratory murmur, on the one hand, and, on the other hand, with those of bronchial respiration. As a preliminary step, the normal respiratory murmur and bronchial respiration are to be contrasted in respect of the characters of each as ascertained by analysis.

The inspiratory sound in the normal respiratory murmur is of variable intensity in different persons. Intensity, therefore, does not enter into its characteristics. Its pitch is low, and its quality, for lack of a better term, may be called vesicular. The vesicular quality is *sui generis*. It cannot be described by words, and a distinct apprehension of it cannot be given by any analogy. The expiratory sound is continuous with the inspiratory, in natural breathing; it is still lower in pitch, much shorter, and the quality is neither vesicular nor tubular. Its quality may be expressed by calling it a simple blowing sound.

In contrast to these characters of the normal respiratory murmur, the inspiratory sound in bronchial respiration is high in pitch and tubular in quality, its intensity, like that of the inspiratory sound in the normal respiratory murmur, being variable, and therefore not entering into the distinctive characters of the sign. The expiratory sound, separated from the inspiratory sound by a brief interval, is higher in pitch than the inspiratory sound, tubular in quality, usually more intense, and its duration is equal to or longer than that of the inspiratory sound.

Bronchial respiration is the respiratory sign of complete or considerable solidification of lung. Now, between a degree of solidification sufficient to give rise to bronchial respiration and the normal condition of lung, gradations in solidification are involved in different diseases, and in different stages of the progress of certain diseases. Pneumonia and phthisis are familiar examples of diseases involving these gradations. As regards the abnormal modifications of respiration caused by a slight or a moderate degree of solidification, there is not a little vagueness and confusion; the respiratory sounds have been called rude, rough, harsh, sharp, and dry. These terms convey not only indefinite but erroneous ideas. As an illustration of incongruity, a cardiac bellows murmur is distinguished as soft, whereas a similar sound produced by respiration would be called rude. Supplementary or puerile respiration is harsher in quality than the sound which represents moderate solidification of lung. A late German author of a work on diagnosis, which has been translated into the English and the French languages,

* Prize Essay on Variations of Pitch in Percussion and Respiratory Sounds, and their application to Physical Diagnosis.

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includes, under the name hyper-vesicular, the sign called by others rude, rough, harsh, &c., whereas a distinctive feature of this sign is diminution of the vesicular quality of the inspiratory sound.* The lack of a clear apprehension of the characters distinctive of the sign is implied in the term indeterminate (*unbestimmt*) applied to it by Skoda, and still used by German writers.† I have proposed, as already stated, for the sounds representing gradations of solidification of lung falling short of the degree represented by bronchial respiration, the name broncho-vesicular respiration. This term expresses what analysis teaches—namely, a combination of the characters of bronchial respiration with those of the normal respiratory murmur. In broncho-vesicular respiration the inspiratory sound is both vesicular and tubular. The vesicular quality, always less than in health, is more or less diminished, and the tubular quality is more or less marked in proportion to the degree of solidification. The pitch is raised in proportion as the tubular quality predominates over the vesicular. The intensity is not important. The pitch, tubular quality, and length of the expiratory sound are in correspondence with the characters of the inspiratory sound. If in the inspiratory sound the vesicular quality predominate over the tubular, the expiratory sound is but little prolonged, its tubularity is not marked, and the pitch is but moderately raised; on the other hand, if in the inspiratory sound the tubular quality predominate over the vesicular, the expiratory sound is more prolonged, its tubularity is more marked, and the pitch is higher. According to this description, a broncho-vesicular respiration may approximate closely to the bronchial, the chief distinction consisting in an appreciable vesicular quality in the inspiration; or, it may approximate to the normal respiratory murmur, the distinction consisting in the presence of an appreciable tubular quality. There are gradatory combinations between these extremes, as regards the relative proportions of the bronchial and the vesicular characters. As regards the significance of the sign, the solidification is greater in proportion as the characters of the sign approximate to those of bronchial respiration, and the amount of solidification is small in proportion as the characters approximate to those of the normal respiratory murmur. The intermediate gradatory combinations are exemplified during the stage of resolution in acute lobar pneumonia. The practical value of the sign in that connection is obvious. The sign is still more valuable in cases of phthisis and other pulmonary affections which involve slight or moderate degrees of solidification, either diffused or circumscribed. This sign enables the auscultator, not only to recognize the existence and the limits of solidification when not sufficient to give rise to bronchial respiration, but to ascertain whether the solidification be moderate or slight.

The distinctive characters of the broncho-vesicular respiration may be studied by auscultation of the chest in health. It has been customary to apply to the modifications of the respiratory murmur, as heard over the primary and secondary bronchi, the name normal bronchial respiration. This term is a misnomer. The respiratory sounds in this situation are never purely bronchial, but they have the bronchial and the vesicular characters combined. An appropriate name, therefore, is the normal broncho-vesicular

* Guttmann.

† Guttmann states that as indeterminate respiratory sounds cannot be compared with any other known sounds, it is impossible to describe them. The advantage of the analytical method of study is shown by the facility with which they are described by the characters pertaining to the pitch and quality of the inspiratory and of the expiratory sound. The endeavour to explain the mechanism leads this author into error as regards the significance of the so-called indeterminate sounds. Their significance is rationally understood when it is considered that they are not indeterminate sounds, but sounds intermediate between the normal respiratory murmur and bronchial respiration.

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respiration. I need not add that the characters of this normal broncho-vesicular respiration are more marked on the right than on the left side of the chest; the area within which they are confined being the sternal portion of the infra-clavicular and the upper part of the interscapular regions.

Cavernous respiration was described by Laennec as having essentially the characters of bronchial respiration, the only difference being a sensory impression of air entering a larger space. Subsequent authors have generally held that these two signs are not to be differentiated by intrinsic differences. Skoda affirms that they are absolutely identical, and in this he is followed by the most recent of German publications. The nearest approach to the characters distinctive of the cavernous respiration is the description of Walshe, in the early as well as the late editions of his work on diseases of the lungs.

The analytical study of respiratory signs led me to recognize well-marked and easily-recognizable characters distinctive of cavernous respiration as long ago as 1852.* This sign is to be differentiated, on the one hand, from bronchial respiration, and on the other hand, from the normal respiratory murmur. The differential characters are as follows; The inspiratory sound, as compared with that of bronchial respiration, is low in pitch and non-tubular; as compared with that of the normal respiratory murmur, it is non-vesicular. It has a simple blowing quality. The expiratory sound differs from that of bronchial respiration in being low in pitch and devoid of tubular quality. The pitch is lower than that of the inspiratory sound. In pitch and quality it resembles the expiratory sound in the normal respiratory murmur. These characters, limited to a circumscribed space, without the boundaries of which is often found either bronchial or broncho-vesicular respiration, are readily appreciated, and they point unmistakably to the site of a cavity. The characters are so distinctive that the sign cannot be confounded with either bronchial or broncho-vesicular respiration. The liability to error is in confounding cavernous respiration with simply weakened respiratory murmur, the only essential point of distinction being the presence of vesicular quality in the normal inspiratory sound, and the absence of this quality in the cavernous inspiration; hence, if the respiratory murmur within a circumscribed space be so weak that the vesicular quality is not clearly appreciable, it cannot be distinguished from feeble cavernous respiration. The associated vocal sounds should always prevent this error.

Cavernous respiration is not infrequently modified by solidification of lung surrounding or situated in proximity to cavities. A combination of the bronchial and the cavernous characters is sometimes rendered apparent within a circumscribed area by comparison with a purely bronchial or a broncho-vesicular respiration without the limits of this area. This modification may be distinguished as broncho-cavernous respiration. A cavity without adjacent solidification of lung may furnish a cavernous inspiratory sound combined with more or less of the vesicular quality. This may be distinguished as vesiculo-cavernous respiration. It is recognized by comparison with the respiratory murmur without the limits of a circumscribed area, the latter corresponding to the site of a cavity. The fact of the existence of the cavity may be further established by associated vocal signs.

Other varieties may be mentioned. A cavernous inspiration is sometimes associated with a bronchial expiration. This happens in some cases when lung, completely or considerably solidified, is in contact with, or in close proximity to, a cavity. The bronchial inspiration is not heard over the cavity, but the more intense bronchial expiratory sound extends beyond the solidified lung, and displaces, or, more properly speaking, drowns the

* *Vide* Prize Essay: "Trans. American Med. Association," 1852.

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cavernous expiration over the cavity. In another variety the inspiratory sound is at its beginning either bronchial or broncho-vesicular, and it becomes cavernous before its termination. The probable explanation is that air enters the cavity, not at the beginning of the inspiratory act, but before the conclusion of the act; hence, prior to the development of the cavernous respiration the sound represents adjacent solidification of lung.*

As is well known, Laennec gave very little attention to the sounds produced by the expiratory act. A young American physician (James Jackson the younger) was the first to study these sounds, twenty years after the discovery of auscultation. Jackson ascertained the importance of a prolonged expiration, having something of the bronchial character, as a diagnostic sign in the early stage of pulmonary phthisis. The characters which a prolonged expiration may have are of importance when an inspiratory sound is present, but especially so when an inspiratory sound is either wanting or too weak for its characters to be appreciated.

The significance of a prolonged expiration depends on the characters pertaining to pitch and quality. If the pitch be high and the quality tubular, it denotes solidification of lung as if the respiration were completely either bronchial or broncho-vesicular; in other words, as if associated with either a high-pitched tubular or a vesiculo-tubular inspiration. If the pitch be low and the quality non-tubular or simply blowing, it is either a cavernous sign or it denotes delay and hindrance to the free exit of air in the expiratory act, as in cases of emphysema. The prolonged expiration in emphysema is always low and blowing, not high and tubular, at least without the areas in which a normal broncho-vesicular respiration may be present. A prolonged expiration is not a sign of phthisis (exclusive of cavity), unless the pitch be raised and the quality more or less tubular; or, as stated by Jackson, unless it have something of a bronchial character.

I pass by adventitious sounds, simply remarking that my experience corroborates a statement made by Skoda—namely, the pitch of moist bronchial râles, or coarse and fine bubbling, and of the sub-crepitant râle, denotes either, on the one hand, solidification around the tubes in which the râles are produced, or, on the other hand, absence of solidification. The pitch is more or less raised when these râles occur in connection with pneumonia, phthisis, or other affections involving solidification. The pitch is not raised when they occur in bronchitis, in pulmonary œdema, or in other morbid conditions which do not involve solidification of lung.

Signs Referable to the Loud Voice and Speech.

The analytical study of transmitted voice-sounds is simpler than that of the respiratory sounds, but not less important with reference to clearness and precision as regards the distinctive characters of vocal signs. Suppression of vocal resonance, and simple diminution of the normal intensity, are signs which do not call for analysis. It is not so with the remaining signs referable to voice and speech—namely, bronchophony, increased vocal resonance, ægophony and pectoriloquy.

Bronchophony, the sign correlative to bronchial respiration, is characterized by concentration of the transmitted voice, nearness to the ear, and elevation of pitch, as compared with the diffusion, distance, and lowness of pitch, which are the characteristics of

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* Under the name metamorphosing respiratory murmur (*metamorphosirendes Athmungsgeräusch*) Seitz has described a variety of broncho-cavernous respiration in which, using his terms, the first part of the respiratory sound is rude, and the latter part bronchial in character. Inasmuch as by German writers the cavernous and the bronchial respiration are considered as identical, the latter part of the respiratory sound, in the variety described by him, is probably cavernous.

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the normal vocal resonance. It is important to note that intensity is not an element of bronchophony; the distinctive characters of this sign may be not less marked with a feeble as with a loud vocal resonance.

An abnormal loudness of the transmitted voice-sounds, without the characteristics of bronchophony—that is, the characters of the normal resonance preserved exclusive of intensity—is to be distinguished as increased vocal resonance. This sign signifies either a degree of solidification falling short of that requisite for bronchophony, or the transmission of a voice through a cavity.* It seems an incongruity, but clinical experience shows it to be true, that a moderate degree of solidification of lung may give rise to more intensity of resonance than a greater degree of solidification, the lesser resonance having the characters of bronchophony, and the greater resonance retaining the characters of the normal resonance exclusive of intensity. A cavity not surrounded by solidified lung may be represented by notable intensity of vocal resonance, but without the bronchophonic characters.

Normal bronchophony is sometimes found within the area in which the respiration may be normally broncho-vesicular. In general, however, within this area—that is, over the primary and secondary bronchi, the resonance is simply more intense than in the other thoracic regions.

The opinion held by Laennec, that pectoriloquy is exclusively a cavernous sign, has long since been disproved. Articulated words, or the speech, in addition to the voice, may be transmitted by solidified lung as well as through a cavity. The characters pertaining to the transmitted voice, associated with the speech, however, enable the auscultator to decide, in individual cases, whether the pectoriloquy be, or be not, a cavernous sign. If pectoriloquy be accompanied by the characters distinctive of bronchophony (nearness to the ear, and elevation of pitch), the transmission is by solidified lung; if, on the other hand, speech be transmitted, and the characters of bronchophony be wanting, the inference is that the pectoriloquy denotes a cavity. Two varieties of pectoriloquy, therefore, may be recognized—namely, bronchophonic and cavernous. This statement conflicts with the opinion of Skoda and others, who hold that pectoriloquy is simply an exaggeration of bronchophony.

I would remark that pectoriloquy, which may be defined the transmission of speech, is often not sharply discriminated by writers on auscultation, as well as by practical auscultators, from bronchophony—the latter being the transmission simply of the voice; and it is evident that the discrimination was not clearly made by Laennec. Laennec seems to have been biased by a desire to establish pectoriloquy as exclusively a cavernous sign. That pectoriloquy is entitled to be considered as a sign distinct from bronchophony, is shown by the fact that it may exist without any of the characters of the latter. Under these circumstances, in accordance with what has been stated, it is always a cavernous sign.

To the vocal sign called ægophony, Laennec, in his treatise on auscultation, devoted more space than to any other physical sign; and perhaps there is no sign which has been more discussed than this by subsequent writers, although it is a sign of comparatively small practical importance, inasmuch as other well-marked and readily available signs suffice for the diagnosis of pleural effusion. Laennec confessed that he encountered

* I dissent from the statement made by some writers that bronchophony is a cavernous sign. Clinical study, as I believe, shows that merely intensification of the resonance is the sign when the voice is transmitted through a cavity. The voice may be bronchophonic over a cavity surrounded by solidified lung, but the sign then represents the latter condition, and not the cavity.

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much difficulty in the explanation of this sign. That, as a rule, if not invariably, the sign represents pleural effusion, I do not doubt, notwithstanding the opinion of Skoda and others to the contrary.

Here, as in other instances, Laennec naturally sought to give an idea of the sign by comparisons. The name which he gave to it applies resemblance to the cry of the goat. He also compared it to the voice when a counter is placed between the teeth and the lips, to the voice transmitted through a metallic speaking-trumpet, and to the nasal intonation which is assumed in the performances of Punch. Studied analytically, it has the concentration and the high pitch of bronchophony. It differs from the latter sign in being distant, and in its tremulous or bleating character.*

Signs Referable to the Whispered Voice.

The sounds heard over the thorax when words are whispered, have not, as yet, been recognized as forming a separate group of auscultatory signs. They seem to me to be entitled to this distinction. It is true that a whisper is almost always an expiratory act, and, therefore, the characters of the sounds thus produced are identical with those of expiration in the respiratory signs. The expiratory effort in a whisper, however, as a rule, has more force and emphasis than in the acts of respiration; hence, the characters of the sounds heard over the thorax are more marked; and, moreover, there is sometimes an advantage in listening to these sounds disconnected from the inspiratory sounds. Practically, the whispered voice will be found useful, especially in the diagnosis of incipient pulmonary phthisis.

The whispered voice, as heard over the healthy chest, may be called the normal bronchial whisper, inasmuch as the same is conducted by the bronchial tubes. The normal bronchial whisper is low in pitch, its quality is blowing, and its intensity in different persons variable, these characters corresponding to those of the expiratory sound in the normal respiratory murmur. The characters are normally modified over the primary and secondary bronchi, especially on the right side of the chest, in the same way as the expiratory sound in normal broncho-vesicular respiration. The abnormal modifications may be named so as to correspond with the signs referable to the loud voice, as follows: 1. Increased bronchial whisper; 2. Bronchophonic whisper, or whispering bronchophony; 3. Cavernous whisper; and, 4. Whispering pectoriloquy.

The whispered, as well as the loud voice and the respiration, may be amphoric; but I pass by now, as hitherto, this sign, for the reason that it does not require analytical study, the musical intonation being alone sufficient for its recognition.

The bronchophonic whisper is correlative to bronchophony referable to the loud

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* I refrain in this paper from entering into a consideration of the mechanism of signs; but with regard to ægophony I will venture to offer an explanation, which I do not remember to have met with in any work on auscultation. It is that, the sign is produced when, owing to either old adhesions, or recent agglutination by fibrinous exudation, the pleural surfaces adhere in the upper part of the chest, so that the lung resists the pressure of the liquid; consequently, the pressure upon the lung below the adherence condenses it to such a degree as to give rise to bronchophony. The bronchophony, under these circumstances, lacks the nearness to the ear which it has when liquid is not present, and the presence of the liquid causes the goat-like characters of the sound. This explanation tallies with the fact that the sign is generally limited to a narrow strip near the level of the liquid, and also with the fact that the sign is rarely found except when the level of the liquid is at or near the lower angle of the scapula. According to this explanation, as well as to the results of analysis, ægophony is a modification of bronchophony.

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voice, and to bronchial respiration. It is a high-pitched tubular sound, more or less intense.

Increased bronchial whisper is correlative to increased vocal resonance and to broncho-vesicular respiration. It is less high in pitch, less tubular, and less intense than the bronchophonic whisper.

The cavernous whisper is correlative to cavernous respiration. It is low in pitch, blowing in quality (as distinguished from tubularity), and of variable intensity.

In whispering pectoriloquy the speech—that is, articulated words—are conveyed to the ear of the auscultator. Whispered speech is oftener transmitted than words spoken with the loud voice. The whispered words may be transmitted either by solidified lung or through a cavity, and it is easy to determine, in individual cases, whether or not it be a cavernous sign. If the pectoriloquious whisper be also bronchophonic—that is, the sound high in pitch and tubular in quality—the conduction is by solidified lung. If, on the other hand, the whispered words be associated with the characters of the cavernous whisper, the conduction is through a cavity.

Signs Produced by Percussion.

The advantages of the analytical method of study are as marked in its application to percussion as to auscultation. The results of the study, however, will require much less extended consideration.

Taking, as a point of departure, percussion in health, and the characters of the normal resonance as a standard for comparison, the number of morbid signs need not exceed six, and considering, as might be done, three of these as varieties of one sign, the number is reduced to four. Thus, either four or six signs represent the important morbid physical conditions incident to different pulmonary diseases, in so far as these conditions are determinable by percussion. An important result of the analytical method of study is the elimination of such vague terms as full, empty, hard, wooden, tracheal, bandbox resonance, &c.

The normal resonance on percussion varies in different persons and in different parts of the chest, as regards intensity. As compared with all the morbid signs produced by percussion, it is always low in pitch. The quality is *sui generis*, and being due to the air vesicles, it may properly be called vesicular.

One of the signs is characterized by absence of all resonance or flatness. Of course this sign has no characters pertaining to pitch or quality of sound,

Diminished resonance or dulness is another sign. In this sign the vesicular quality of sound is decreased in proportion to the diminution of resonance or the degree of dulness, but more or less of the quality is appreciable. The pitch is always higher than that of the normal resonance of the person examined. The elevation of pitch is of practical value in determining a slight degree of dulness.

A third sign is tympanitic resonance. Intensity should not be considered as an element in the characters distinctive of this sign. A tympanitic resonance may be either more or less intense than the normal resonance of the person examined. The chief characteristic of the sign relates to the quality of sound; the resonance is devoid of vesicular quality. A resonance absolutely non-vesicular is always tympanitic. Tympanitic resonance and non-vesicular resonance are, therefore, convertible terms. The pitch is always higher than a resonance with vesicular quality.

The fourth sign is a resonance in which the vesicular is combined with the tympanitic quality, and the intensity of the resonance abnormally increased. I have proposed to distinguish this sign by the descriptive name, vesiculo-tympanitic resonance. The pitch is always higher than that of the normal resonance of the person examined. This

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vesiculo-tympanitic resonance is a diagnostic sign in cases of vesicular emphysema. It is the resonance found above the level of the liquid in cases of pleuritic effusion, and over the healthy lobe of a lung when another lobe of the same lung is the seat of lobar pneumonia.

In order to illustrate the characters of this sign, and also its practical value, I will state a problem in diagnosis:—

Let it be supposed that a patient having had cough and expectoration for a considerable period, together with deficiency of breath on exercise, is a subject for a physical examination of the chest. Over the upper anterior thoracic regions, on the right side, the resonance on percussion is found to be notably less in degree than over the corresponding regions on the left side. The difference, as regards intensity of resonance, between the two sides in these regions, is distinctly greater than the normal disparity. Now, if the relatively less intense resonance on the right side be considered dulness, this sign, in connection with the symptoms, points to the existence of pulmonary phthisis. But the relatively less intense resonance on the right side may not be dulness—it may be due to an abnormal increase of the resonance on the left side. If this be so, the greater resonance on the left side points to vesicular emphysema. The question whether the difference in the intensity of resonance between the two sides be due to an increase of the resonance on the left side (denoting emphysema), or to dulness on the right side (denoting phthisis), is to be settled by comparing the resonance on the two sides as regards characters other than intensity—that is, by the characters relating to pitch and quality. If the resonance in the hypothetical case which has been stated, be higher in pitch, as well as more intense on the left side than the resonance on the right side, it is a vesiculo-tympanitic resonance, and denotes emphysema. If, on the other hand, the resonance on the right side be higher in pitch than that on the left side, as well as less intense, it is dulness, and denotes a certain degree of solidification of lung.

The error of confounding phthisis and emphysema is not infrequent: two diseases differing widely in respect of gravity, and the latter protective in a great measure against the former. Assuming, in a case offering the problem just stated, the disease to be pulmonary emphysema, the relatively lesser resonance on the right side of the chest is, in fact, an increased or a vesiculo-tympanitic resonance, as may be shown by comparing the resonance on this side in the regions named with that of the resonance over the lower lobe of the lung on the same side. This is in accordance with the rule that vesicular emphysema, when not lobular or vicarious, is bilateral, affecting the upper more than the lower lobes on the two sides, and generally the upper lobe of the left more than the upper lobe of the right lung.

Amphoric and cracked-metal resonance on percussion may be considered as varieties of tympanitic resonance. They are covered by the definition of tympanitic resonance, that is, the resonance is non-vesicular. They are readily enough distinguished by their characteristics. Perhaps, in view of their significance as cavernous signs, it is more convenient to enumerate them as distinct signs.

I have endeavoured in this short paper to give an exposition of the method of study which, as it seems to me, secures for auscultation and percussion simplicity, together with completeness and precision in their application to physical diagnosis, and I have submitted results to which I have been led by pursuing this study as a branch of clinical medicine. If the tone of my paper may have appeared to show undue assurance—or even dogmatism, I would plead in extenuation that in order not to presume too much on the patience of my hearers and readers, I have sought to condense the matter as much as possible. As a further plea, I may add that I have, for many years been a

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student and teacher of auscultation and percussion, and that I have reached the age when some indulgence may be claimed on the score of no longer remaining among the junior members of our profession.

Note on the Value of Baccelli's Sign—Pectoriloquie Aphonique —in the Differential Diagnosis of Pleuritic Effusions.

R. Douglas Powell, London.

My knowledge of Dr. Baccelli's* views on the value of transmitted whisper sound in distinguishing between purulent and serous effusions, is entirely derived from Dr. Noel Gueneau de Mussy's abstract of his paper in the *Union Médicale*,† of January, 1876. Dr. Gueneau de Mussy, in subsequent numbers of the same journal, adds some valuable remarks on the physical signs of pleurisy; but it is as regards the diagnostic value of this one particular sign—*pectoriloquie aphonique*—that I wish to invite discussion to-day. Dr. Baccelli admits of three varieties of pleuritic effusion (he is speaking of inflammatory effusions), 1, serum; 2, pus; 3, sero-pus; or, rather, sero-purulent *débris*, a serous fluid containing granular, pyoid, and epithelial cells. I would here remark that the distinction between pus and sero-purulent fluid, rightly observed, is one of much clinical importance in pleuritic effusions, for whereas pus is material, consisting of corpuscular, living, migratory, and multiplying elements, in the sero-purulent fluid, commonly found in effusions of older date, the pus elements are dead, degenerated, effete; the fluid, save for the mechanical inconvenience caused by it, is innocuous. Such a fluid may exist in the pleura for many years. On some rare occasions, however, a purulent effusion, instead of thus becoming degenerated, rapidly loses vitality and decomposes, giving rise to septic systemic poisoning.‡ The diagnosis between serous and purulent effusions into the pleura is confessedly very difficult. Dr. Baccelli justly maintains that of all the signs that have been regarded as characteristic of empyema no one sign and no combination of signs is really so. Profuse sweatings are as common in the rheumatic form of serous pleurisy as in empyema. The pyrexia may be of hectic type and long duration in serous effusion (Case 2). Chills may be very decided in simple effusion, although positive rigors, followed by sweatings, are very rare; but these latter signs may be trivial in empyema. Dr. Baccelli even maintains that we may have œdema of the affected side in association with simple effusion. It has never occurred within my experience to meet with such a case, but Dr. Baccelli's positive statement must be accepted. Hitherto puncture of the pleura, with removal of a sample of the fluid it contains, has been the only means of arriving at a certain diagnosis.

One would gladly welcome a new sign that would render diagnosis certain without recourse to this somewhat rude, if harmless, procedure, and Dr. Baccelli claims that the presence or absence of whispering pectoriloquy, heard through the thickness of the fluid, affords such a criterion. It is the purpose of my present communication further to test

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* Sulla transmissione dei suoni attraverso i liquidi endopleurici di differente natura: *Archivi di Medicina, Chirurgia ed Igiena* Roma: 1875.

† I have since had the advantage, with the help of my friend Dr. E. Pollock, of perusing Dr. Baccelli's original paper.

‡ A serous effusion may probably, under certain circumstances, undergo decomposition also.

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this view by relating several cases in which the sign has been observed and its value tested, and to elicit further experience from those present. If, in a case of pleuritic effusion, the observer apply his ear directly or by the stethoscope on the affected side in the outer basic region (*e.g.*, below and outside the inferior angle of the scapula), and direct the patient to utter in a distinct whisper some rough words, as "tren-ta-tré" *trente-trois*, "one," "two," "three," the whispered sounds may (1) be well conducted to the ear, constituting *pectoriloquie aphonique*, in which case it may be assumed, in Dr. Baccelli's opinion, that the fluid present is clear serum; or (2) the whispered sounds may be inaudible, or very imperfectly audible, in which case the fluid is opaque, and more or less purulent. In one case in which the sign was present, and the fluid removed was purulent in appearance, Dr. Baccelli was satisfied by finding, on microscopic examination, that it did not contain pus, but myriads of granulo-fatty elements.

It will be needless to detail the physical signs of the cases now to be quoted, since we are only concerned with the facts relating to the presence or absence of Baccelli's sign in each case.

Case 1.—Mr. E. A., aged twenty-three, consulted me on May 1, 1879, for pain in the left side. This illness commenced in January, with repeated hæmoptysis during a week. He recovered this attack and remained "well" until fourteen days before seeing me, when he was taken with pain in the left side, of pleuritic character, with languor, but no positive inability to work. His father died at sixty, of bronchitis; his mother at forty-two, of cancer of the throat. He was a sparely-built man, with very pallid face and mucous membranes. Breath extremely offensive, almost gangrenous in odour. The chest showed the usual signs of a large effusion into the left pleura, displacing the heart to the right of the sternum. Over the region of absolute dulness in front the whispered voice was well conducted to the ear. Over the scapular region the voice sounds were bronchophonic, of nasal quality (imperfect ægophony), and from the spine of the scapula to a little above the base the whisper was well conducted (*pectoriloquie aphonique.*) Two days later, with a needle syringe, a drachm of perfectly clear fluid was removed. The rest became slowly absorbed without further interference, and the patient recovered. The hereditary history of cancer in this case and the gangrenous odour of breath, with preceding severe hæmorrhages and marked present anæmia, threw considerable doubt upon the diagnosis which Baccelli's sign helped to remove.

Case 2.—Miss L., aged thirty, of rheumatic diathesis, inherited from the mother's side, came to me on July 14, 1880, with left pleurisy of two or three days' date. There were friction sounds in the left infra-mammary region, the pulse was 140 and the temperature 102°. After four days the left side was dull to the clavicle with well marked ægophony in the scapular region, and somewhat distant tubular breath sound, with well marked *pectoriloquie aphonique* over the whole back. The temperature ranged between 100° and 103°, and for forty days it rose daily above 100° (*see* Chart). Pulse varied from 100 to 120. On the 10th to the 14th days distinct chills were experienced, in consequence of which, and also of restlessness from increasing dyspnœa, paracentesis was performed by Mr. Nunn on the 15th day, and 45 oz. of perfectly clear serum removed, a considerable portion being left behind. The pleura partially refilled, the fluid then began to subside, but a week after the operation, when there was returning resonance in the sub-clavicular region, *pectoriloquie aphonique* was still distinct posteriorly, and remained so for some time later. The pyrexia did not subside wholly until the 60th day, although Miss L. slowly improved and recovered, but with contraction of lung and side. In this case the character and duration of the pyrexia and the occurrence of chills suggested that the effusion was purulent; the presence of Baccelli's sign negatived this view, and was confirmed by the result of the operation. Tubular breath sound, heard in this and in many

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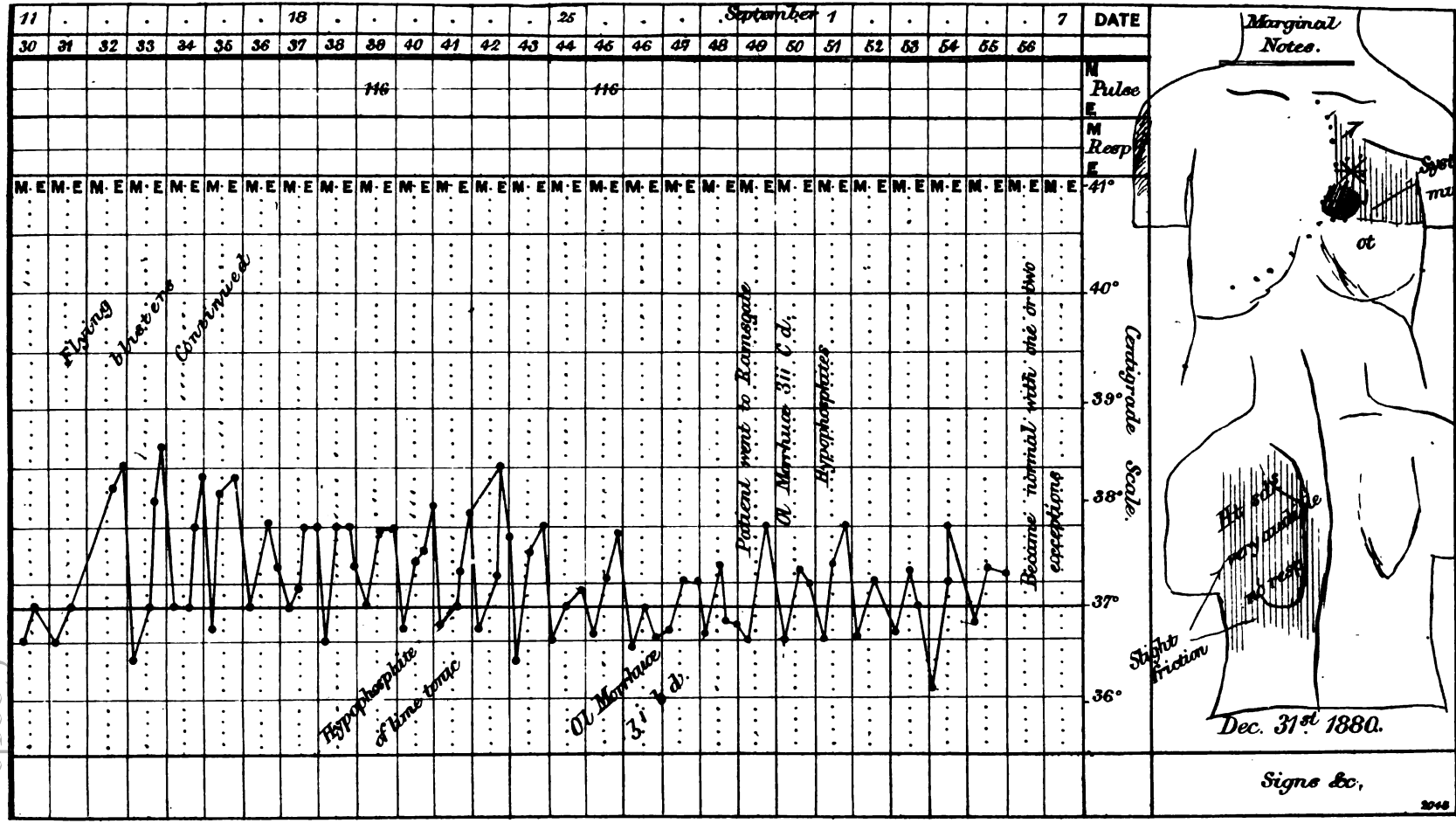

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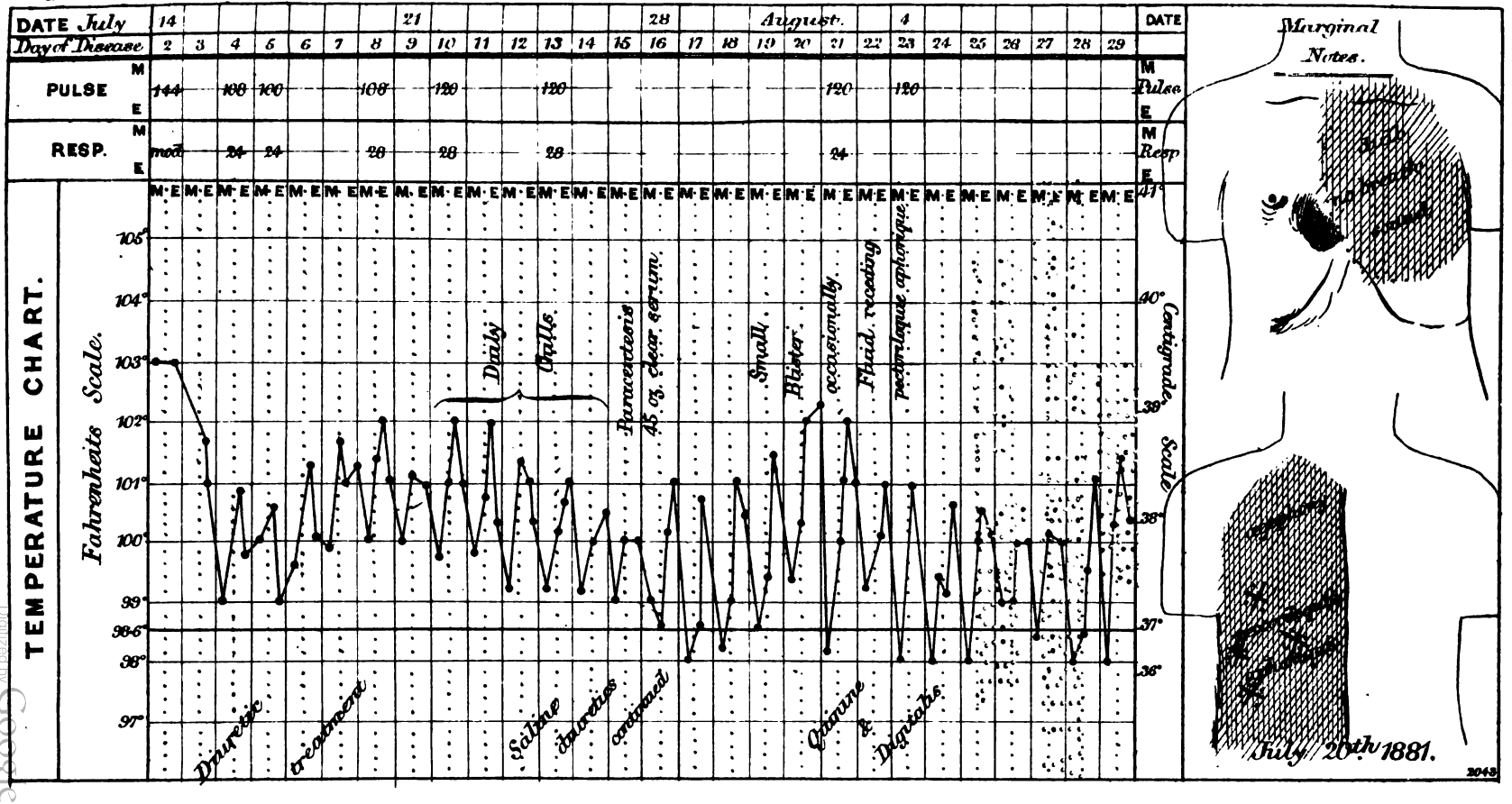
Name Miss L. Disease. Pleuritis.



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such cases, is a sign often calculated to mislead diagnosis, if the displaced heart and other signs be not carefully noted.

Case 3.—On April 26, I saw Miss K., aged about twenty-six, with Dr. Pearson, of Kensington. The illness commenced three weeks previously with pain in the left side and rigors after exposure to cold whilst fatigued. The left side was dull throughout, except at the sterno-clavicular region, where some Skodaic resonance was elicited, the heart beating to the right of the sternum. There was no respiration audible over dulness in front, but over lower two-thirds posteriorly there was distant tubular breath sound, with well-conducted voice and whisper—*pectoriloquie aphonique.* The side was distinctly fuller behind, the inward curve of the flank obliterated. No measurements taken. April 28, trocar, with syphon tube attached, introduced at sixth space anterior, nipple line, and two and half pints of clear serous fluid, very slightly blood-stained, removed; the rest of the fluid became absorbed without further interference.

Case 4.—Henry I., aged twenty-nine, admitted (May, 1881) under my care into the Middlesex Hospital, with acute left pleurisy of eleven days' duration. All the signs of large effusion were present, and *pectoriloquie aphonique* well marked, especially at the inferior angle of the scapula. Four and a half pints of perfectly clear serum were removed. The operation was afterwards repeated, and the rest became gradually absorbed.

Case 5.—June, 1881, a female, aged twenty-six, under my care in the Middlesex Hospital with effusion into right pleura of a few days' duration. The effusion at first rapidly extended, but the symptoms and signs—including Baccelli's sign—rendered the diagnosis of serous effusion pretty certain, and the fluid subsequently abated in the usual way without interference.

Case 6.—Wm. R. C., a clerk, aged twenty-nine, was admitted into the Brompton Hospital under my care in February, 1880. His father was asthmatic and two children had died of phthisis. He had suffered for many years from winter cough, but previous to his present illness had been four years free from it. Twelve months prior to admission he had severe neuralgia for three months, and at the end of that time had bronchitis with increasing breathlessness, weakness, and moderate hectic. On admission he presented the signs of a considerable effusion into the left pleura. At the apex, however, the lung was adherent, cavernous respiration and clicks were then heard. Over the region of dulness, which extended from the second rib in front and the spine of the scapular behind, there were no respiratory sounds, and the whisper was not conducted. On March 15, paracentesis was performed, and 35½ oz. of clear fluid removed. The lung disease further developed, however, and the sputa became fœtid, the temperature, which had been of hectic type, continuing to rise daily above 100°. A free incision was made with antiseptic precautions on April 1, and almost clear serum removed. The patient died some four months later from extension of lung disease. In this case, in which the lung was also diseased, although the fluid present was quite clear, Baccelli's sign failed.

Case 7.—On May 16, 1880, I was asked by Dr. Stamford, of Tunbridge Wells, to see a young lady of consumptive family history, who, six weeks previously, had been seized with symptoms of acute pleurisy, which appeared to subside; but recently hectic symptoms had developed with apthous mouth, and complete intolerance of food by the stomach. The chest signs were those of a large effusion into the left pleura, displacing the heart to the right of the sternum. At about the inferior angle of the scapula there was imperfect ægophony, the voice being heard, but not conducted up the stethoscope, and being attended with a lisp distinctly conducted up to the ear. The whispered voice gave *pectoriloquie aphonique.* Paracentesis was advised, and on the second day Dr. Stamford removed by the aspirator twenty-five ounces of "fœtid purulent matter."

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Note from Dr. Stamford, May 7, 1881:—"Miss N. is doing well, and gaining flesh and strength slowly. I made sufficient opening to admit the tube you kindly sent me, and which was taken out in March. The wound has not quite closed, and there is still a little discharge." In this case the presence of Baccelli's sign contradicted the evidence derived from general symptoms which indicated septic poisoning. If it be urged that the pus had become so altered by breaking up of its elements from decomposition as to correspond acoustically with serum, then Baccelli's sign might be regarded as of especial value in association with septic symptoms, as indicating decomposition of the fluid. I much doubt whether experience will bear out this interpretation, however.

Case 8.—A distiller's drayman, aged forty-one, of good family history, but rather intemperate habits, was admitted into the Middlesex Hospital, under my colleague, Dr. Cayley, in March, 1880. He had a month previously fallen over a beer-cask, and injured the right side of the chest. Pleurisy followed with high fever and delirium for a week, and, subsequently, cough and pain in the side continuing, with dyspnœa and emaciation, he was admitted. The chest yielded the signs of a moderate effusion on the right side, occupying the postero-lateral region, the dulness being absolute below the spine of the scapula, with very faint breath sounds, and ægophony in scapular region, and conducted whisper (*pectoriloquie aphonique*) at base. There were also signs of general bronchitis over the lungs. A fine trocar was inserted, and pus issuing, a free incision was made. The patient died, and, post-mortem, "over the right pleura, the adhesions were moderately firm in front and over the anterior parts, laterally and toward the base; but along a line running obliquely outwards from the angle of the second rib to the mid-axillary line in the ninth interspace the adhesions were almost cartilaginous in density. On cutting through them about 50 oz. of thick light yellow curdy pus flowed out. On removing this the cavity of a localized empyema was found situated between the spine and the line of adhesion just mentioned, and having its anterior wall formed by the posterior surface of the lung. The pleural surfaces of the empyemal cavity were much thickened, and covered with a layer of coagulated lymph hanging in shreds into the cavity. The contained pus was not offensive."—Extract from post-mortem notes by Dr. Fowler.

Dr. Noel Gueneau de Mussy, interpreting Dr. Baccelli's views, observes,* "Les parois de la cavité qui contient le liquide peuvent aussi influer sur la transmissibilité du son; il y a, en effet, une grande différence, au point de vue de la vibratilité, entre une membrane homogène, sèche et tendue, et une autre qui n'est pas homogène qui est pulpeuse humide et rélâchée . . . . . Et voilà pourquoi: dans la collection purulente enkystée qui, pour M. Baccelli, constitue l'empyème vrai, dit-il, l'extinction des vibrations est complète sous l'influence combinée des parois limitantes, de la densité et de la corpusculation du liquide." In the above-related case these conditions were present (it was emphatically *l'empyème vrai* of Dr. Baccelli), but did not serve to extinguish the conduction of the whisper sound.

Case 9.—An ex-soldier, aged thirty-two, in the Brompton Hospital, under my colleague, Dr. Symes Thompson, who was good enough to ask me to see the case. The man, of previous good health, had pleurisy with effusion in the left pleura twelve months ago, and 42 pints of fluid were removed: again in September 110 ounces were removed, the fluid on both occasions being quite clear, "like bitter beer." The patient had lost one stone in flesh since his illness, and had some hereditary tendency to phthisis, but had presented no active symptoms, neither hæmoptysis nor severe cough. On the 16th of April, a few days after admission, he was again aspirated, and 6 ounces of

* *L'Union Médicale, loc. cit.*, p. 51, 52.

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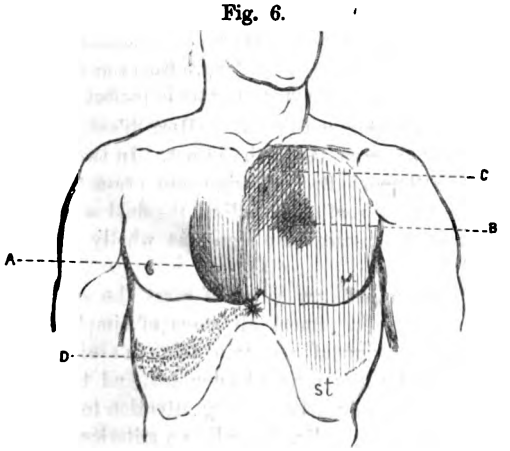
opaque semi-purulent fluid removed; the effusion rapidly increased, and on April 26th the physical signs indicated a large collection, displacing the heart to the right of the sternum, enlarging the side to the extent of one inch of increased semi-circumference. The signs were those usual in effusion, but the whispered voice was nowhere conducted over the region of dulness, and vocal resonance only very incomplete over the upper part posteriorly; respiratory sounds absent. On the opposite side, in the mammary region, coarse inspiratory crepitation was present. The temperature had since admission ranged 101° in the evening. By means of trocar and syphon-tubes 117 ounces of opaque yellowish green fluid were withdrawn. Under the microscope the fluid was found to contain free fat granules and corpuscles in tolerable abundance, each containing several bright refracting granules. No living pus cells or leucocytes were observed.

April 29.—Breath sound with friction audible to the fourth rib in front, with percussion resonance. Resonance posteriorly to junction of upper and mid-third of scapula. Whisper inaudible, no ægophony. Side two inches smaller than the opposite.

Case 10.—On July 29, 1881, I took an opportunity of carefully re-examining a patient, John C., a policeman, aged thirty-eight, who two years ago was under my care in the Brompton Hospital, and who has since been more or less under my observation and that of my colleague, Dr. R. Thompson. When admitted two years ago the man had extensive effusion into the left pleura, which dated from an acute attack eighteen months previously (February, 1878). The presence or absence of *pectoriloquie aphonique* at that time is not recorded in my notes. He was on two occasions tapped, and iodine injections employed, but the fluid reaccumulated, and the general health and physique of the patient being extremely good, and his thoracic parietes strong and rigid, and, further, having regard to the fact that the lung was in all probability permanently collapsed, I decided, after consultation with Dr. R. Thompson, that it was better to let him alone so long as he suffered no active inconvenience rather than to run the risk of the long illness and doubtful result that must necessarily have attended a radical operation in such a case. He has continued on night duty as a policeman ever since.

Fig. 6.



Shading indicates extent of dulness, A heart, apex at *. Respiration generally absent over region of dulness, but over a limited area at third cartilage, B, the respiration was very tubular with pectoriloquial whisper. At C the respiration was bronchial, similar in pitch to that over the trachea; the voice and whisper conducted, but not distinctly pectoriloquial. Along the margin of the thorax on the right side at D a well marked fringe of enlarged venous capillaries extended.

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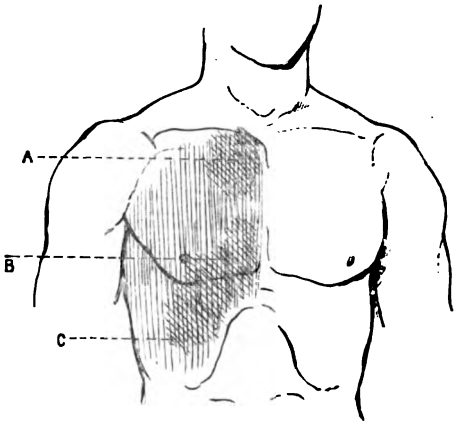
July 29, 1881, the physical signs as shown in the diagrams still indicated great collection of fluid in the pleura.

Fig. 7.



Back view of chest. Vertical shading again indicates complete dulness on percussion, with absence of respiration and fremitus. Voice and whisper sounds generally absent; but along areas of oblique shading, A and B, the whispered voice was distinctly conducted at C, being almost pectoriloquial, whereas above or below this line of shading it became quite lost. This lower line of conducted whisper extended from the 4–5 spinous process to below the angle of the scapula, and corresponded therefore in direction with the main bronchi. The upper line, and also the pectoriloquial spot in front, might correspond with a bronchiectatic condition of lung, covered by densely thickened pleura. The cyrtometer tracing of the circumference of the thorax immediately below the nipple level showed that the measurement of the left side (18⅜ inches) only exceeded that of right side (18⅛ inches) by ⅛ inch; but the tracing on this side was more rounded, and gave a larger capacity—viz., 9¼ inches by 6 inches, as against 8⅝ inches by 6⅜ inches on the opposite side. Vital capacity, 90–100 cubic inches (Lownes' spirometer). The patient suffers from no cough or other symptoms, and, save from some breathlessness on exertion, as from walking quickly or up hill, he is apparently in perfect health.

Of the ten cases above related, with respect to Baccelli's sign, in six cases the fluid was clear, five of them yielded the sign, the sixth did not. In two cases, also acute, the fluid was purulent; yet in both cases Baccelli's sign was present when one would not have expected it. In two cases, both of old standing, the fluid was serous, containing effete pus elements; in one of these cases the sign was wholly absent, in the other it was present in a modified degree.

I have no doubt that in a larger series of cases the sign would hold good in a greater proportion, probably in almost all cases of simple serous effusion. That *pectoriloquie aphonique* is a sign of a certain value in association with other signs in the diagnosis of pleuritic effusions cannot be doubted, and the thanks of the profession are due to Dr. Baccelli for prominently drawing attention to it. But that the sign has the surpassing value contended by Dr. Baccelli as a criterion in the diagnosis of serous from purulent effusions is disproved, I think, even by the meagre experience I have ventured now to contribute. We cannot yet be said to know the precise mechanism of the sign. Homogeneity of fluid does not suffice for its transmission; Case 6 would tend to show that a damaged lung or a thickened pleura would prevent the occurrence of the sign. Yet in Case 2 the sound was perfect, although the continued pyrexia in the absence of cough and the permanent collapse of lung would suggest a thickened

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pleura. Although I have tested the intra-pleural pressure in several cases of pleuritic effusion,* I regret that I have not done so as yet with special reference to the presence of this sign, but in Case 2 the perfection of the sign before and for some time after paracentesis would point to the probability that tension within the sac can have little to do with its occurrence. One cannot doubt, after Dr. Baccelli's observations, that in many cases of purulent effusion the sign would be characteristically wanting, but its presence in the two cases above mentioned (Cases 7 and 8) shows that the sign is too uncertain to be relied upon in diagnosis. Dr. Baccelli regards uniformity and clearness of fluid as very essential to the transmission of the sound, observing that when the fluid is corpuscular the waves of sound are lost by interference and dispersion in passing through a heterogeneous medium. It is difficult to conceive how the breaking down of the corpuscles of a purulent fluid into a granular débris should so alter the acoustic properties of the fluid as to restore the conducting power for whisper sounds, or Case 7 might possibly be explained; but in Case 8 the sound was transmitted through undoubted unchanged pus.

DISCUSSION.

Prof. D'Espine, Geneva: There are two means of distinguishing the bruit of pulmonary solidification (bronchial breathing) from the vesicular murmur. The one arises from the nature itself of the produced sound; the breath begins with an aspirated *h*, the vesicular bruit however harsh it may be, is preceded by an *f* or a *w*. This sign has long ago been given in the German cliniques. The second means is more limited in its utility; it consists in recognizing by the stethoscope the vesicular murmur made by the lung still pervious to air, which separates the ear from the *souffle*.

Dr. Douglas Powell, London: I feel somewhat disappointed that Dr. Austin Flint has not on this occasion given his great authority and experience in the endeavour to place auscultation upon some simpler and broader basis, rather than confining himself to the discussion of particular physical signs. For the time seems to have arrived when the whole subject of auscultation must be reconsidered, and when we shall have perhaps to return to the simpler views advocated by Bean, Spittal and others many years ago. I maintain that the so-called vesicular (healthy) breath sound must be regarded as a compound sound made up of a conducted glottic sound, and the multiple sound produced by the air passing from the bronchioles into the vesicles; and that all morbid breath sounds (harsh, bronchial, cavernous, &c.) are due to the more or less complete suppression of the bronchiole-vesicular part of the normal breath sound, and the better conduction, either simply or with modification of the glottic breath sound. No one of experience can fail to have noted cases of pneumonia and lung excavation, in which the affected side was practically motionless, yet in which bronchial or cavernous respiration, as the case might be, was well marked. This can only be accounted for by the sounds thus heard being conducted glottic sounds, more or less modified by the media through which they are heard. The bronchial tubes themselves, if clear and unchanged, yield no sound, and when obstructed yield certain musical or bubbling sounds called râles. With reference to bronchial breath-sound the experiments of MM. Bondet and Chauveau are conclusive—selecting a horse with pneumonia, in which over the consolidation bronchial breath-sound was distinct, and on the sound side the respiration exaggerated, they performed tracheotomy, and whilst one observer held the wound widely open the other auscultated. The bronchial breath-sound formerly heard over the consolidation was

* *Clinical Society's Transactions*, vol. iii. Jan. 1870. See also Author's work on Consumption, p. 264, in which a continuous tracing of intra-pleural pressure during paracentesis is recorded.

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now extinguished, but the exaggerated healthy breathing on the opposite side remained unchanged. On now introducing a reed-pipe into the wound the musical sound was well heard (bronchophony) over the consolidation, but was feeble on the healthy sides. This experiment completely to my mind settles the question as to the glottic origin of bronchial breath sound.

Prof. EWALD, Berlin: I have to add very few words to the able and clear explanation of our President, but, having had the opportunity of seeing many cases of pleuritic effusion, I wish to express my agreement with Dr. Powell in so far as the so-called Baccelli's phenomena is not at all a true sign of serous or purulent effusion. I believe that Dr. Baccelli started from a wrong point of physical anticipation, inferring that the serous fluid would better conduct the sound than the purulent one. The only way to settle the diagnosis is the puncture with the Pravatz's syringe. This will do no harm at all if antiseptic precautions are taken, and I have done it very often without any damage.

Dr. MAHOMED, London, drew attention to the great necessity which existed for a uniform nomenclature of auscultatory sounds, both for the purposes of instruction, and for ensuring accuracy and comparability of clinical records; several examples were given of various meanings for one word, and of various words with one meaning; great confusion existed among teachers and students in these matters, and he asked the Congress to take steps to define more accurately the terms employed.

Dr. THEODORE WILLIAMS, London, drew attention to the difference of sounds conveyed to the ear by single and double stethoscopes, and suggested that if the latter were largely used the terminology of auscultation must be re-written. He was inclined to agree with the value of Dr. Baccelli's diagnostic point between serous and purulent effusions.

Dr. FLINT, New York: 1. With reference to the remarks by Dr. Powell, these having reference chiefly to the mechanism of signs, the one object of my paper is to advocate the importance of determining the significant signs by clinical and autopsical observations wholly irrespective of the question of mechanism. 2. With reference to bronchial and cavernous respirations, the distinctive characters as stated in the paper are repeated. These characters are based purely on experience. 3. I thoroughly agree with Dr. Mahomed's remarks on the importance of a uniform nomenclature in the literature of auscultation and percussion. 4. I wish to uphold the usefulness of the binaural stethoscope.

Essai de Cardiographie Clinique.

Prof. D'ESPINE, Gèneve.

Conclusions Générales—1.—La cardiographie clinique peut être utile, soit au point de vue du diagnostic, soit au point de vue du pronostic. Les tambours explorateurs de Marey donnent des tracés comparables entr'eux à la condition qu'ils soient bien appliqués. La meilleure vitesse de rotation pour l'enregistrement des tracés est de quatre centimètres par seconde.

2.—Le tracé synchrone de la carotide doit être pris comme la clef du tracé du choc; il remplace chez l'homme le tracé du pouls aortique et donne les deux repaires nécessaires pour reconnaître dans le tracé du choc les parties qui se rapportent aux deux bruits du cœur.

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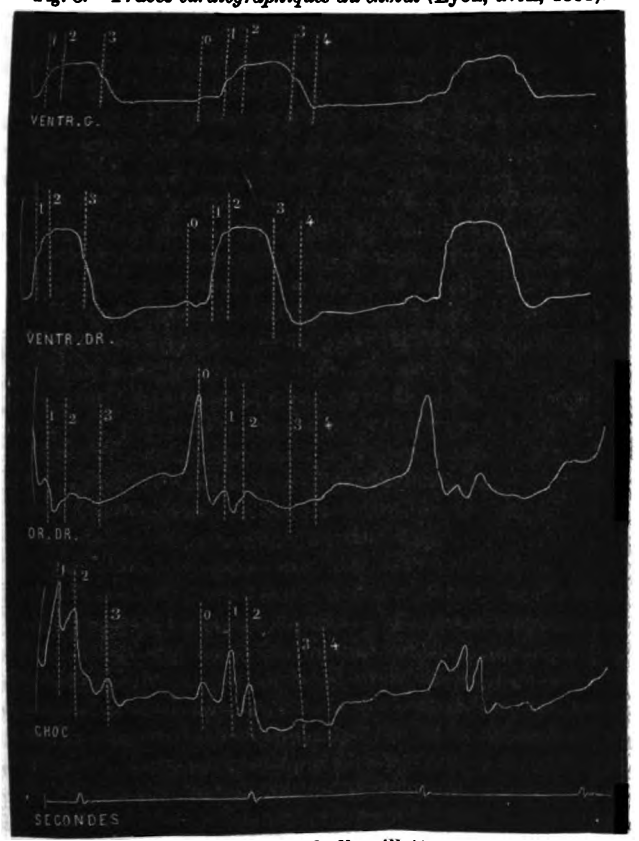
Le premier repaire ou *repaire mitral* est indiqué par le début du pouls carotidien; la distance qui sépare ce repaire du début de l'élévation systolique, du choc, ou le retard carotidien, délimite dans le tracé du cœur le premier effort systolique, celui qui est nécessaire pour soulever les sigmoïdes et vaincre la tension aortique; c'est la partie du choc qui correspond *au premier bruit.*

Le second repaire ou *repaire sigmoïde* (diastolic notch de Burdon Sanderson) qui correspond dans le tracé carotidien à la naissance de l'onde en retour du dicrotisme, indique dans le tracé du choc le point qui correspond au second bruit.

L'erreur qui provient du temps qui sépare le pouls aortique du pouls carotidien est négligeable; elle est au maximum de deux ou trois centièmes de seconde.

Fig. 8.—*Tracés cardiographiques du cheval* (Lyon, avril, 1881).



0.—Contraction de l'oreillette.
1, 2.—Les deux pulsations mitrales.
3.—Fermeture des sigmoïdes.
4.—Fin de la systole ventriculaire gauche.

3.—Les tracés physiologiques de la contraction ventriculaire ainsi que les tracés du choc démontrent que la systole n'est pas une contraction musculaire unique, mais un composé d'efforts successifs, parmi lesquels on peut distinguer deux pulsations principales (*a.*) *La pulsation mitrale* du ventricule qui a pour résultat de faire pénétrer le sang du cœur dans le réservoir aortique. (*b.*) *La pulsation artérielle* qui chasse le sang de l'aorte

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jusqu'aux extrémités de l'arbre artériel. Ces deux pulsations sont marquées dans le pouls carotidien, la seconde seule existe à l'état normal dans les pouls périphériques tels que le pouls radial ou fémoral.

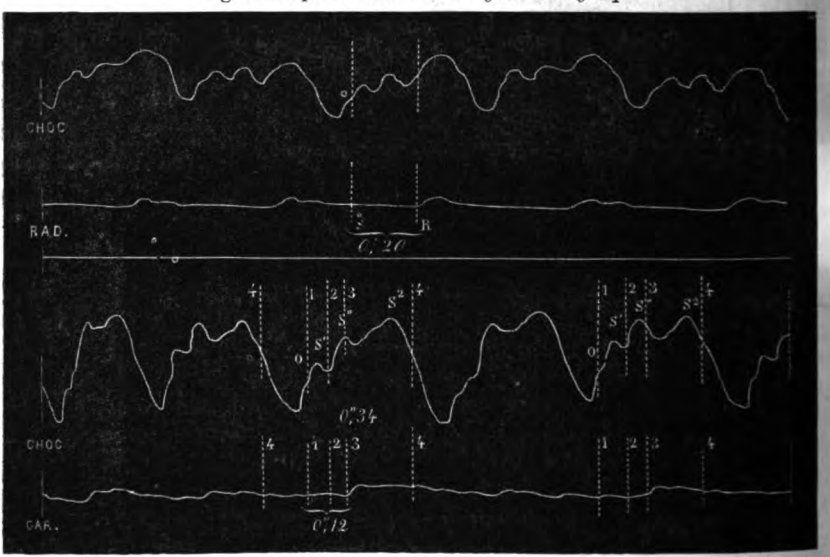
Dans le tracé normal du choc chez l'homme la pulsation mitrale est seule bien marquée et forme le sommet du tracé. Quand la tension aortique s'exagère, la pulsation artérielle apparaît dans le choc et en forme la partie culminante (néphrite interstitielle, certains chocs physiologiques après un effort musculaire exagéré). La forme du tracé rappelle alors celle du tracé intra-ventriculaire obtenu par l'ampoule du ventricule gauche chez le cheval. Quand la tension aortique est abaissée la ligne de descente commence après le sommet mitral et n'est interrompu que par un ressaut dû au choc en retour du ventricule.

4.—La pulsation mitrale est tantôt simple, tantôt double. A une pulsation mitrale simple correspond toujours un premier bruit unique; plus, au contraire, les deux pulsations seront accusées et distantes, plus le redoublement du premier bruit sera net et se rapprochera du vrai bruit de galop.

Chez le chien et chez l'homme la pulsation mitrale est simple à l'état normal. Chez le cheval elle est double et correspond à un double claquement des valvules auriculo-ventriculaires, bien distinct de la présystole. La présystole n'entre pour rien dans la production du bruit de galop.

Il n'y a jamais de dédoublement du premier bruit, parce que les deux ventricules sont parfaitement synchrones dans la première partie de la systole. Il peut y avoir au

Fig. 9.—*Néphrite interstitielle aiguë—vrai galop.*



2a.— { s Début de la systole ventriculaire.
r Début du pouls radial.

2b.—Présystole.

S^1. S^{11}.—Les deux pulsations mitrales.

S^2.—Pulsation ventriculaire artérielle.

1.—Début de la systole ventriculaire.

3.—Repaire carotidien.

4.—Repaire sigmoïde.

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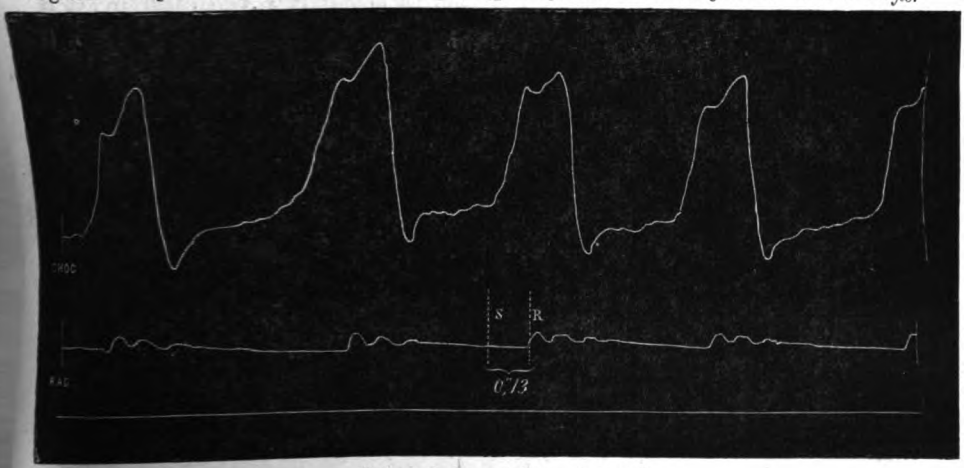
contraire des dédoublements du second bruit, puisque la fermeture des sigmoïdes peut être asynchrone dans l'aorte et l'artère pulmonaire.

5.—Il faut distinguer (*a*) *un bruit de galop à gauche*, dû au double claquement de la valvule mitrale ; il a son maximum à la pointe et s'observe dans certains cas de tension aortique exagérée (bruit de galop néphritique).

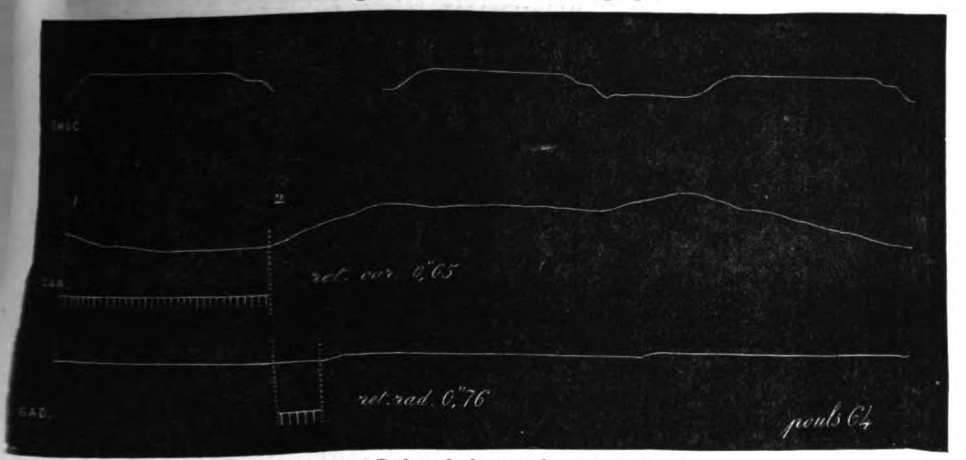
(*b.*) *Un bruit de galop à droite* dû au double claquement de la valvule tricuspide ; il s'observe dans certains cas de tension pulmonaire exagérée, tels que la stase pulmonaire avec faiblesse cardiaque dans le pneumo-typhus, la stase pulmonaire dans les affections *mitrales, &c.* Il a son maximum au sternum, toujours moins sonore et plus fugitif que *le galop gauche* (bruit de galop pulmonaire).

Fig. 10.—*Néphrite interstitielle chronique, hypertrophie du ventricule gauche.—1° bruit renflé.*



s.—Début de la systole ventriculaire.
r.—Début du pouls radial.

Fig. 11.—*Rétrécissement aortique pur.*



1.—Début de la systole ventriculaire.
2.—Début du pouls carotidien.

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6.—L'augmentation de tension artérielle paraît être u ne condition, sine quâ non, pour les productions du bruit de galop, mais ne suffit pas toujours. Le vrai mécanisme du double claquement valvulaire est encore à trouver.

7.—Dans les différentes affections valvulaires du cœur gauche (rétrécissement, insuffisance), le premier bruit est toujours simple et la pulsation mitrale unique, tant qu'il n'y a pas de complications du côté des poumons ou du ventricule droit.

8.—Au point de vue du diagnostic, la cardiographie clinique révèle trois formes pathognomoniques: (1) *Celle du rétrécissement aortique* en large plateau. (2) *Celle de la néphrite interstitielle* en dos de chameau. (3) *Celle de l'insuffisance mitrale* en pain du sucre.

9.—*Au point de vue du pronostic*, la cardiographie donne le rapport exact entre les deux facteurs essentiels de la circulation, la force de *propulsion ventriculaire*, et la *tension artérielle*.

(*a*) La lenteur de la propagation de l'onde ventriculaire au pouls radial (*retard radial*) est un signe de faiblesse cardiaque, surtout si la tension artérielle est forte. La forte tension artérielle favorise en effet la propagation de la vague ventriculaire; dans la néphrite interstitielle avec hypertrophie du cœur, *le pouls radial* présente deux pulsations avant le dicrotisme, qui correspondent aux deux pulsations ventriculaires.

(*b*) *Le retard carotidien est minimum* quand le ventricule gauche est hypertrophié et la tension aortique abaissée (insuffisance aortique). *Il est maximum* dans le rétrécissement aortique. Il est variable dans la néphrite interstitielle; il peut être considérablement augmenté quand le cœur est faible et la tension aortique considérable, il redevient normal à mesure que l'hypertrophie du cœur et, par conséquent, la force réelle augmente. La systole est d'autant plus prolongée et multiple que la disproportion est plus grande entre la résistance à vaincre et la force du muscle cardiaque. *La polysystole du cœur* est dans certains cas (affections mitrales) le précurseur de l'asystolie.

10.—*Le double choc diastolique* ne s'observe que dans deux maladies du cœur.

1. *Dans l'insuffisance* aortique *avec reflux considérable*, le double choc, étant facilité par la dilatation du ventricule, est en général un mauvais signe pronostic.

2. *Dans l'insuffisance mitrale* avec rétrécissement et hypertrophie de *l'oreillette gauche*. C'est un flot de l'oreillette exagéré, une sorte de présystole anticipée.

DISCUSSION.

Dr. James Barr, Liverpool: The subject of reduplication of the cardiac sounds, is one to which I have devoted considerable attention, and I must at once join issue with Professor D'Espine, when he says, "that there is never a reduplication of the first sound, because the two ventricles are perfectly synchronous in the first portion of the systole." On the contrary, I assert that each side of the heart has its own proper muscular fibres, as well as those which are common to both sides; and, by the way, the proper fibres form in great part the deepest layers, and so are first subjected to the stimulating influence of distension in producing contraction; and even in those fibres which are common to both sides it is possible for the wave of contraction to commence as it were at one extremity of the fibre, and be propagated to the other. Each side has more or less independently its own nerve supply, its own peristaltic action; and notwithstanding that both sides are set to the same time, and that they have a complex interlacement of fibres, and are usually observed to act consentaneously, yet it is quite possible, yea, it is an experimental fact, that one side can begin or end contraction before the other. Not only have I seen this in my experiments, but I have clinically demonstrated it in several instances, and have cardiographic tracings of a peculiar case which show complete asynchronism in the systole of the two ventricles. The left

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ventricle was readily felt to contract first, then the right, and this was occasionally followed by a second contraction of the left ventricle which completed the cardiac cycle. What is a *bruit de galop* but rapid reduplication, and in my opinion undoubtedly due to asynchronous valve tension, though Professor D'Espine explains it by an imaginary double click of the left or right auriculo-ventricular valve. It is quite impossible that a double click could occur without an intervening ventricular relaxation, which would be equivalent to abortive systoles. Dr. D'Espine says that "the true mechanism of the double valvular click remains to be discovered." I would rather say that the double valvular click itself remains to be discovered, and is likely to remain so, as it is non-existent, but the mechanism of reduplication is an established fact. Reduplication of the cardiac sounds is due to asynchronous valve tension, which is distinct from, but dependent on, asynchronous ventricular contraction, though it is quite possible that differences in intra-ventricular blood-pressure may be sufficient to give rise to reduplication of the first sound without there being any appreciable ventricular asynchronism. By asynchronous ventricular contraction I mean that one ventricle may begin or end contraction before the other, but not complete asynchronism, and so you may have reduplication of one or other or both sounds. With regard to his explanation of Fig. 9, I believe that what he calls the beginning of ventricular systole is really the commencement of the auricular systole. For, if he be correct, and if the drum of his cardiogram moved at the rate of "four centimètres in a second," then, in the first portion of the figure the radial pulse is delayed two-ninths of a second, and in the second portion the carotid pulse is delayed one-eighth of a second, which is scarcely credible. Even Professor D'Espine previously tells us that "the error which arises from the time which separates the aortic from the carotid pulse may be ignored; it is at the most from 2 to 3-100ths of a second," and we can scarcely believe that it took a vigorous ventricle the remainder of the time to raise the semilunar valves. Dr. D'Espine looks upon high tension as essential in the production of reduplication, but in my opinion it is not the high tension *per se* which causes the reduplication, but it is the effect which it produces in the capacities of the ventricles, and in the uniform distribution of the blood. That ventricle which is best supplied with blood, and which best retains its muscular irritability, first overcomes the inhibitory action of the vagus, first raises its intra-ventricular pressure, and first closes its auriculo-ventricular valve, which gives rise to the first element of a double first sound. It does not necessarily follow that the ventricle which begins will first end contraction, for the period of systole varies. The period of systole depends on the quantity of blood to be driven, the power of the ventricle to drive it, and the resistance in front; *cæteris paribus*, the less the resistance the shorter the systole; and so the ventricle which has the least resistance, in proportion to its power, offered to the issuing current completes its systole before the other, and permits primal recoil of its artery with closure and tension of its semilunar valve, and thus gives rise to the first element of a double second sound.

Recherches Cliniques sur la Thermométrie Locale.

Dr. P. Rédard, Paris.

1.—Température de la peau du thorax à l'état normal et à l'état pathologique. M. Rédard communique les résultats de ses recherches sur les températures de la peau du thorax à l'état physiologique et dans la pleurésie et la pneumonie. Il résulte des recherches de M. Rédard que la température de la peau du thorax est

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soumise à des variations considérables. La moyenne de la température de la peau du thorax est, à l'état normal, de 35·5° à 34·6°. Il y a des différences de trois, quatre, ou cinq dixièmes de degré entre les deux côtés de la poitrine, suivant que l'on place l'extrémité de l'un des membres supérieurs dans l'eau froide ou dans l'eau chaude.

Dans la pleurésie, si l'on compare le côté atteint au côté sain, dans un grand nombre de cas on ne trouve pas de différences. Dans la pneumonie, il y a généralement une légère hyperthermie du côté atteint, mais elle se produit sur tout le côté et non pas seulement au niveau du point malade. Dans aucun cas la température du thorax n'est supérieure à celle de l'aisselle et du rectum.

Toutes ces recherches et ces expérimentations ont été faites à l'aide, non pas de thermomètres, mais bien d'appareils thermo-électriques.

2.—Recherches de thermométrie locale dans la tuberculose. Inconstance des résultats obtenus.

3.—Recherches sur la température locale dans les affections articulaires, etc.

Die klinische Bedeutung der Phosphorsäure des Harns.

Dr. W. Zuelzer, Berlin.

Wie Ihnen bekannt ist, habe ich mich bemüht zu zeigen, dass die Untersuchung des Harns dem Kliniker das Mittel an die Hand giebt, den Stoffwechsel in der Muskelmasse des Körpers von demjenigen der nervösen Organe, des Blutes, der Knochen &c. zu differenziren. Dieser Anschauung gemäss hat sich der Arzt bei der Betrachtung der pathologischen, ebenso wie der physiologischen Lebensvorgänge die Frage vorzulegen: Welche stofflichen Alterationen entsprechen den zur Beobachtung gelangenden funktionellen und anatomischen Störungen?

Die Untersuchungs-Methode, die, wie Sie eben hörten, namentlich von Prof. Lépine (Lyon), und ausserdem von Prof. Edlefsen (Kiel), Prof. Eulenburg (Greifswald), Prof. Stokvis (Amsterdam), u. A. ausgedehnt wurde, ist zuletzt in meinem "Lehrbuch der Harn-Analyse," Berlin, 1880, beschrieben. Erlauben Sie mir, bevor ich einen neuen Beweis zur weiteren Motivirung beibringe, mit einigen Worten auf die Grundlagen der Theorie einzugehen.

Ich habe gefunden, dass in den einzelnen Körpergeweben das Verhältniss der Elementarbestandtheile zu einander durchaus constant ist. (Zur Erläuterung demonstrirt der Vortragende einige Modelle, die inzwischen im Museum aufgestellt sind.)

Denken wir uns die Muskelmasse des Körpers zusammengestellt, in der Weise, dass ihre durch die chemische Analyse ermittelten Elementarbestandtheile auseinander gehalten werden, so finden wir, dass der gesammte, darin enthaltene Stickstoff sich zur gesammten Phosphorsäure verhält wie 100 : 15, und zum Schwefel (als Schwefelsäure ausgedrückt) wie 100 : 24.

Im Blut (total) verhält sich der Stickstoff zur Phosphorsäure wie 100 : 4, und zur Schwefelsäure wie 100 : 2—3.

In der Nervensubstanz ist das Verhältniss von Stickstoff zur Phosphorsäure wie 100 : 45; die Schwefelsäure ist verschwindend gering.

Diese Verhältnisszahlen characterisiren die einzelnen Gewebe vollkommen. Der Kürze wegen bezeichnen wir diese Zahlen als *relative Werthe für die einzelnen Aschenbestandtheile.* Natürlich bleibt der relative Werth der Phosphorsäure, der Schwefelsäure &c. immer derselbe, gleichviel ob eine kleine oder eine grosse Menge eines jener Gewebe in Betracht kommt.

Wenn nun innerhalb des Körpers ein Theil eines dieser Gewebe durch die Prozesse

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des Stoffwechsels zerstört wird, und die Zersetzungsproducte (was bis auf bestimmte Abweichungen zutrifft) in den Harn entleert werden, so müssen wir darin ein Verhältniss zwischen den einzelnen Elementarstoffen wiederfinden, wie es in der Muttersubstanz ermittelt ist.

Die Aufgabe des Klinikers besteht also darin, das Verhältniss der einzelnen Componenten des Harns zu einander festzustellen (100 N : + $P_2 O_5$ &c.), um zu ermitteln, aus welchen Körpergeweben sie herstammen.

Wir haben bis jetzt eine grosse Reihe von Zuständen kennen gelernt, wobei im Harn der relative Werth der Phosphorsäure sehr hoch gefunden wurde und daraus geschlossen, dass hierbei die Zersetzungs-Produkte desjenigen Gewebes vorwalten, welches an Phosphorsäure relativ am reichsten ist, nämlich der Nervensubstanz. Dahin gehört z. B. der Harn nach der Chloroform-Narkose, nach der Intoxikation mit Morphium, der Harn der kritischen Entfieberung (nach Pneumonie) &c.

Ich bin jetzt in der Lage, Ihnen einen weiteren Beweis für die Richtigkeit dieser Anschauung beizubringen. Wie Sie wissen, ist von Hoppe-Seyler inzwischen nachgewiesen, dass—conform unseren Untersuchungen—der Harn im normalen Zustande kleine Mengen von Glycerin-Phosphorsäure enthält. Sie ist als direktes Zersetzungsprodukt der Lecithinhaltigen Gewebe (namentlich der Nerven) anzusehen, weil sie mit Neurin verbunden in diesem Körper enthalten ist.

In meinem Laboratorium wurden eine grössere Reihe von Harn-Untersuchungen in dieser Richtung angestellt. Die Resultate sind, was ich beiläufig bemerke, desshalb nur annähernd, weil die analytische Methode nicht quantitativ genau ist.

Im normalen Zustande beim Manne, im Alter von 20—25 Jahren, enthält der Harn von 24 Stunden nur 1—2 Milligrm. Phosphorsäure in Verbindung mit Glycerin. Auch im Fieberharn wurde nur eine ebenso geringe Menge gefunden.

Dagegen wurde in folgenden Fällen eine ungleich grössere Menge Glycerinphosphorsäure nachgewiesen:

1.—Im Harn nach der Chloroform-Narkose (3—6 Stunden danach). Die Menge, der mit Glycerin verbundenen Phosphorsäure, betrug in einem Falle: in 220 CC. Harn 0·009 Grm.;—in einem anderen: in 290 CC. Harn 0·005 Grm.;—in einem dritten Falle: in 102 CC. Harn 0·003 Grm. &c.

2.—Im 400 CC. Harn nach Application von Morphium (subcutan 0,02 Grm. bei Schlaflosigkeit) wurde 0,01 Grm. gebundene Phosphorsäure ermittelt. In mehreren anderen Fällen, auf die ich anderweitig zurückkomme, schwankte die Menge der im Nachtharn enthaltenen an Glycerin gebundenen Phosphorsäure zwischen 4 und 15 Milligr.

3.—Ausserdem wurde der Harn nach der Krisis bei Pneumonie und Erysipel untersucht; er enthielt in der 24stündigen Menge durchschnittlich 18 Milligramm gebundener Phosphorsäure,—im Minimum 8, im Maximum 35 Milligrm.

Wir können zwar wegen der in quantitativer Beziehung nicht ausreichenden Methode nicht darauf rechnen, dass wir die ganze Menge der an Glycerin gebundenen Phosphorsäure nachzuweisen im Stande sind. Indessen sprechen diese Zahlen unzweifelhaft dafür, dass in den vorliegenden Fällen der gefundene hohe relative Werth für die Phosphorsäure massgebend dafür ist, dass hier Nervensubstanz in grösserer Quantität zerstört werde.

Die vorgerückte Zeit erlaubt mir nicht, weiter auf die Bedeutung der Glycerinphosphorsäure, resp. derjenigen Nahrungsmittel einzugehen, welche Phosphorsäure und daneben Fett enthalten, woraus der Organismus also Glycerinphosphorsäure zu bilden im Stande ist. Ich erlaube mir nur noch anzuführen, dass ich namentlich die *Milch* und den *Ungarwein* auf ihren Gehalt an Glycerinphosphorsäure untersucht und erhebliche Mengen dieser Substanz darin gefunden habe.

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Sur le soufre incomplétement oxydé de l'urine, principalement dans ses rapports avec l'excrétion, l'absorption, &c., de la taurine.

Prof. LÉPINE, Lyon.

On sait que l'urine de l'homme et de plusieurs animaux renferme, à l'état physiologique, outre des sulfates et des acides sulfo-conjugués (dans lesquels le soufre est également à l'état d'acide sulfurique), divers composés sulfurés, où le soufre se trouve à un état d'oxydation moins complète. Ce fait a été mis hors de doute par Ronalds, Voit, Schönbein, Schmiedeberg, Meissner, Sertoli, Löbisch, G. Schleiden, Salkowski, Munk, Kulz et F. Falck. Strumpell et Thudichum l'ont observé chez des typhiques.

Parfois on s'est contenté de produire de l'hydrogène sulfuré en traitant l'urine par l'acide sulfurique et le zinc; mais la plupart des auteurs que je viens de citer ont fait une détermination quantitative du soufre incomplétement oxydé; pour cela, ils l'ont transformé en acide sulfurique au moyen du chlore ou du nitrate de potasse. J'insiste sur le fait que c'est seulement par le dernier procédé qu'on oxyde tout le soufre contenu dans l'urine.

Comme preuve, je citerai l'exemple suivant, qui est celui d'une urine normale: dans un échantillon par litre, on trouve 3 grammes d'acide sulfurique préexistant (c'est-à-dire à l'état de sulfates et d'acides sulfo-conjugués); en faisant bouillir un second échantillon avec du chlorate de potasse et de l'acide nitrique, on trouve 3 gr. 4 d'acide sulfurique (c'est-à-dire qu'on a oxydé une quantité de soufre correspondant à 4 décigrammes d'acide sulfurique); enfin en calcinant le résidu d'un troisième échantillon de la même urine avec du nitrate de potasse (en présence du carbonate de soude), on trouve 3 gr. 6 (c'est-à-dire que dans la dernière opération on a oxydé, de plus que dans la précédente, une quantité de soufre équivalent à 2 décigrammes d'acide sulfurique).

Le fait que le chlore n'oxyde pas tout le soufre est indiqué dans les "Abstracts;" mais ce qui ne s'y trouve pas, car je l'ai tout récemment découvert, c'est qu'il est extrêmement avantageux de faire pour la même urine l'une et l'autre des deux oxydations que je viens d'indiquer; en effet, avec le chlore on oxyde seulement certains composés de l'urine, et on n'attaque pas sensiblement le soufre faisant partie de composés plus stables (taurine, acide tauro-carbonique—Salkowski); par conséquent on a le moyen de distinguer, ce que personne n'a fait jusqu'à présent, le soufre facilement oxydable et le soufre difficilement oxydable, lequel a un grand intérêt au point de vue de l'excrétion de la taurine par le foie, de la résorption de la même substance par l'intestin, &c. &c., et que, pour cette raison, je proposerais d'appeler *soufre biliaire* de l'urine. A l'état pathologique une de ces deux espèces de composés sulfurés de l'urine peut être augmentée à l'exclusion de l'autre.

Ainsi j'ai trouvé que dans certains cas de colique hépatique le soufre facilement oxydable est moins abondant qu'à l'état normal, et que le soufre difficilement oxydable l'est beaucoup plus; la même anomalie peut se rencontrer dans tous les cas où l'excrétion de la bile par les canaux biliaires est gênée; mais j'ai remarqué qu'elle n'est pas durable, alors même que la coloration ictérique de la peau s'accuse davantage, et qu'elle peut même faire place à une diminution du soufre biliaire, ce qui semble prouver qu'un foie ictérique cesse bientôt de sécréter abondamment de l'acide tauro-cholique, ainsi qu'il arrive dans le cas de foie graisseux.

L'exagération de la quantité du soufre biliaire s'observe aussi dans la constipation, qui agit probablement en favorisant la résorption de la taurine dans l'intestin; je l'ai maintes fois constaté dans des coliques saturnines.

Dans ce cas, à la vérité, les choses sont peut-être complexes: on sait que le foie est

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très diminué de volume dans une colique saturnine intense; il se peut qu'il soit frappé temporairement d'insuffisance fonctionnelle. Or, si les vues de Huppert et de Schiff sont fondées (on sait qu'elles ont été vivement combattues par Socoloff), c'est-à-dire, si à l'état normal le foie reprend une partie de la taurine résorbée par l'intestin, on conçoit que, par suite d'une insuffisance fonctionnelle brusque de cet organe, il y ait défaut d'utilisation de la quantité de taurine qui normalement devait être reprise par lui, et par conséquent augmentation de l'excrétion par le rein du soufre biliaire; je reconnais d'ailleurs que ces vues sont encore hypothétiques.

En résumé j'insiste dans cette note sur la distinction entre le soufre facilement oxydable et le soufre difficilement oxydable de l'urine (soufre biliaire), et je montre que les variations relatives, de ce dernier au moins, intéressent dès à présent la pathologie.

Les nombreux dosages (leur chiffre s'élève à plusieurs centaines), sur lesquels sont fondés les faits avancés dans cette note, ont été exécutés par M. Flavard, et surtout par M. Guérin (de la Faculté de Lyon), qui a apporté quelques modifications aux procédés usuels. Ces perfectionnements seront ultérieurement publiés.

On Bacilluria: a Form of Urinary Disorder associated with the Discharge of Rod-shaped Bacteria with the Urine.

Dr. Wm. Roberts, F.R.S., Manchester.

For some years I have occasionally encountered cases in which the freshly voided urine was turbid from the presence of bacteria. The first case of this kind which thoroughly arrested my attention was that of a young German who consulted me some three years ago. He complained of irritation in the urinary passages, marked by frequent and painful micturition—with intercurrent attacks of severe character, resembling acute cystitis. This state of things had continued seven or eight years; he had consulted a number of surgeons in France and Germany, and had had the bladder sounded for stone more than once. No treatment availed; he was sometimes better and sometimes worse, but always troubled more or less with his urine.

He voided urine in my presence; it was opalescent in appearance, and under the microscope it was found to swarm with rod-shaped bacteria. I prescribed for him thirty grains of salicylate of soda three times a day. On his next visit, a fortnight later, I found the urine transparent and free from organisms, and the symptoms gone. He visited me again about a year after for another ailment, and reported that he had not been troubled with his urinary disorder since his previous visit.

Some time after I saw a married lady whose urine exhibited the same characters and similar symptoms, but in a much milder degree. In this case no instrument had ever been passed into the bladder. The patient was suffering from a severe form of rheumatoid arthritis. No special treatment was directed to the urinary trouble, and the case was lost sight of.

During the current year I have had opportunities of observing this class of cases more attentively; and the following examples are offered as a first contribution to the clinical history of an ailment which has not, so far as I know, hitherto received attention, and which I propose to call Non-Septic Bacteruria.

Case 1.—G. Roe, a cabdriver, was admitted into the Manchester Infirmary on the 17th of March, 1881. Thirteen years ago this man was laid up for three months with rheumatic fever. A week before this illness he had contracted a gonorrhœa, which con-

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tinued all the time he was in bed. After his recovery he occasionally suffered from retention of urine, and he has been troubled ever since with frequent and scalding micturition. In the earlier period of the disease he had an instrument passed into the bladder many times, but never within the last ten years.

On the 17th of last March he was seized with a severe rigor, accompanied with prostration, which caused him to seek help in the infirmary. On the evening of admission the temperature was 102·8° F., next morning it was 98·2°, and the same evening 104·2° On the following morning the temperature was found to be normal, and it continued so until his departure from the hospital.

After this brief febrile movement the only symptoms remaining were the old urinary troubles—frequent desire to pass water and scalding micturition. The condition of the urine now attracted attention. It was always found, when examined immediately after emission, to swarm with bacterioid organisms—both micrococci and moving rods. A sample set aside for twenty-four hours deposited a sediment, and the supernatant portions became transparent. On March 22, thirty grains of salicylate of soda three times a day were prescribed. For the succeeding four days bacteria were still seen in the fresh urine; but on March 31 the urine was reported free from bacteria, and it continued so until his discharge on April 7. Meanwhile the urinary symptoms gradually subsided. The patient was seen again three weeks after he left the hospital; the urine was now found to *be* normal, and he declared himself to be relieved from the frequent and painful micturition which had troubled him continuously less or more for thirteen years.

Case 2.—A retired surgeon, about fifty years of age, consulted me on May 12, 1881, for an irritation of the urinary passages. He gave the following account of his case. On February 11, 1881, being then in good health, he fell and hurt his left hip. As the hip continued painful he painted it with a strong solution of iodine. This produced vesication. Four or five days after violent irritation of the bladder came on, with frequent painful micturition both night and day. Under the advice of a medical friend, and with the idea that the bladder trouble was due to congestion of the prostate, solution of cantharides was applied to the perinæum. This blistered the perinæum, and likewise the scrotum, badly. The bladder trouble at once grew much worse; but neither blood, nor albumen, nor any pus to speak of, appeared in the urine. The urine was opalescent, and always acid or neutral, never alkaline. It also had a heavy, disagreeable odour, resembling that of slightly tainted fish—so much so that it had to be removed from the room at once. At no time did the urine possess an ammoniacal odour.

These symptoms persisted with great severity until the end of March. In the early part of April the symptoms began to abate under the influence of morphia suppositories and warm baths, and the improvement has gone on slowly up to the present time. During the height of the disorder two attempts were made to pass a catheter, but on both occasions so great urgency to micturate arose that the instrument was withdrawn without penetrating into the bladder.

When the patient visited me on May 12 the symptoms had greatly moderated. Micturition was now normal in frequency, but there was still considerable uneasiness during the act, and for a short period both before and after. A specimen of urine which the patient brought with him, and which had been voided a few hours before, was found to be crowded with bacteria in active motion. It was opalescent in appearance, acid in reaction, and contained a few pus cells. A fresh sample voided in my presence and examined at once presented exactly the same characters. The moving rods had the appearance of the ordinary vibrios seen in urine, which was beginning to undergo decomposition. They consisted of short rods, varying from eight to fifteen micromillimètres in length, dotted closely along their whole length, and they were agitated with a slow

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vibratory movement. The two specimens of urine were set aside for further examination. On the following day both had deposited a sediment, and the supernatant portions had become perfectly transparent. The transparent condition and acid reaction continued unchanged for six days; and it was evident from the examination of the sediment that no growth or multiplication of the bacteria had taken place since the specimens were voided. On the seventh day both specimens became cloudy from vibrios, the acid reaction gradually declined, and they passed into ordinary decomposition. A third specimen of urine voided in my presence on May 15 behaved exactly like the other two; it became transparent on standing, and maintained its acidity unchanged for several days; it contained fewer rods than the former examples, and these gave no evidence of growth after their emission. The three specimens possessed a high specific gravity—namely, 1025, 1030, and 1028 respectively. It was quite evident that these specimens had no more tendency to ordinary decomposition than healthy urines.

At the patient's first visit I prescribed thirty grains of salicylate of soda twice a day. Three days after this the urine contained many fewer bacteria, and the pain with micturition had nearly gone. On the fifth day from the commencement of the treatment, bacteria could no longer be detected in the urine, and all the urinary symptoms had disappeared.

Case 3.—G. R., a merchant, aged forty, consulted me on June 15, 1881. He complained of severe lumbar pains, and frequent and smarting micturition. Ten years previously I had attended this man for an attack of severe cystitis, with abundant discharge of pus with the urine. In a few weeks the symptoms subsided under the influence of copious potations of decoction of triticum repens, and the patient resumed his ordinary avocations. But the recovery was never quite complete; there remained a slight recurrent irritation of the bladder, which however never seriously interfered with his health and comfort. Some months ago he fell ill with severe symptoms of so-called rheumatic gout. While slowly recovering from this illness, the bladder became irritable, with frequent painful micturition and acute pains in the back. These symptoms had now persisted for six weeks, and had reduced his strength considerably. He had never at any time had an instrument introduced into the urethra.

A specimen of urine made a few hours previously was brought by the patient. This was loaded with pus, acid in reaction, and swarmed with bacteria. A second specimen voided in my presence, and examined immediately, presented similar characters. It was milky from pus, and crowded with rods, and also with micrococci. The reaction was acid, and the odour disagreeable. Both these specimens, on standing twelve hours, deposited a sediment, and became perfectly transparent, and the disagreeable odour passed away. They maintained their transparency and acidity unchanged for a week, though the weather was unusually warm, and then passed into ordinary decomposition.

The patient was ordered to take thirty grains of salicylate of soda twice a day.

On June 29 the patient visited me again. He reported that the pains in the back left him in twenty-four hours after he had commenced taking the medicine, and that the urine lost its milky appearance in two days. A freshly voided sample of urine was found to be almost free from pus; it was acid, and slightly opalescent, and contained numerous rods, but not nearly so many as the previous specimens; it contained no micrococci. The rods in this sample were observed to be, for the most part, longer than those seen previously. Some of them had a length fully equal to twice the diameter of a red blood disk; their dotted or beaded structure was very distinct, and they all exhibited a slow wavy motion. Next day this specimen of urine was turbid from the precipitation of amorphous urates. When warmed it became perfectly transparent, and on boiling it showed a trace of albumen.

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The salicylate had been very badly tolerated by the stomach, and the quantity was reduced to twenty grains once daily.

On July 15 the patient presented himself again. His health was now quite restored, except for a slight recurrence of the lumbar pain. The freshly-made urine still exhibited a good number of rods, but incomparably fewer than at his first and second visit; it was sharply acid, and contained a few leucocytes, and it still had a slight trace of albumen. This specimen remained acid and transparent for nine days. The case is still under observation.

Case 4.—W. D. O., a shopkeeper, aged fifty-three. This man was first seen by me in October, 1875; he was then suffering from recurrent hæmaturia, with painful and frequent micturition. The bladder was sounded for stone, but none was detected. The symptoms persisted for four or five months, and then subsided gradually. Not quite completely, however, for there occurred occasionally in cold weather a slight irritation of the bladder. Last Christmas (after a pause of five years) the symptoms returned; micturition became again painful and excessively frequent. There was also a severe burning sensation about the lower part of the rectum and anus. These symptoms grew worse month by month until at length, in June, he was unable, owing to the pain, to follow his employment. In this second attack the urine has not at any time contained blood; at least not in sufficient quantity to be appreciable to the patient. The general health has not materially suffered. The bladder has been sounded three times since Christmas, with negative results.

I saw the patient for the first time in this second attack on June 22. A freshly made sample of urine was found to be opalescent and sharply acid; it contained numerous short moving rods and myriads of micrococci. A few pus corpuscles were also seen. On the following day this specimen of urine had deposited a sediment, leaving a perfectly transparent supernatant liquid; the micrococci had entirely disappeared, and the rods had not increased in number nor in length. This urine remained transparent and acid for several days. The patient was ordered thirty grains of salicylate of soda twice a day.

On July 6 he was seen again. He stated that for ten days after the commencement of the treatment his sufferings had greatly diminished, but that in the last four days they had returned, and were now as severe as ever, although the medicine had been taken without interruption. He complained especially of the pain and aching about the lower bowel. This led me to make a digital examination of the rectum, but nothing abnormal was detected. A freshly voided sample of urine exhibited comparatively few rods and almost no micrococci. Under the (perhaps fanciful) notion that there might be a colony of bacteria established in the rectum, I ordered thirty grains of salicylate of soda, dissolved in two ounces of water, to be injected twice daily into the bowel. Twenty grains of borax were also ordered to be taken by the mouth twice a day.

On July 13 this patient visited me again. He reported himself as improved, but he complained much of the aching about the anus. The urine was now quite free from ods and micrococci, and, except for the presence of leucocytes, was quite normal. He was ordered to proceed with the treatment.

July 27.—To-day two specimens of urine were very carefully examined, but in neither of them could any bacteria be detected; the urine was still slightly opalescent from pus corpuscles. The painful sensations were almost wholly relieved, and the only thing complained of was a too frequent desire to void urine.

Case 5.—Mrs. B., aged thirty-four, the mother of three children, consulted me on May 30, 1881. She complained of a variety of quasi-hysterical symptoms, and of leucorrhœa. A sample of urine which she had brought with her, and which had been

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passed three or four hours previously, was found to be crowded with bacteria. A freshly voided sample was also obtained, and this was found to be similarly charged, though in a less degree. Both specimens were opalescent and acid, and contained leucocytes and pavement epithelium. There was a slight increase in the frequency of micturition, especially at night; but there was neither pain nor uneasiness in the urinary passages. Both these specimens of urine became transparent on standing some hours, and maintained their transparency and acidity for five days.

She was ordered a mixture of salicylate of soda with iron and quinine.

On June 1 this patient presented herself again. The slight vesical irritation had passed away, and the urine, though slightly clouded with leucocytes and epithelial *débris* was quite free from bacteria. No instrument had ever been passed into the bladder in this case.

These cases are too few to furnish materials for a complete clinical picture, but they are enough, I think, to establish the existence of a form of urinary disorder which is characterized by the discharge of bacteria with the urine, but unassociated with decomposition.

The urine, in these cases, presented certain characters in common. It was opalescent when voided, and acid. It betrayed no undue tendency to decomposition; on the contrary it seemed to resist changes of this order rather more than healthy urine. After standing twenty-four or thirty hours the organisms, and other formed elements contained in the urine, sank slowly to the bottom of the vessel, leaving a clear supernatant liquid which preserved its acidity and transparency many days. The organisms did not multiply in the urine after emission; in one instance only was it thought that the rods had lengthened (although they had not multiplied) after their discharge from the bladder.

The circumstance that the organisms do not multiply in the urine after emission points to the conclusion that their seat of development is not the urine itself,* but some portion of the mucous membrane of the urinary tract.

This form of bacteruria is manifestly a different condition from that in which the urine becomes ammoniacal within the urinary passages, and which is presumably due to the action of the micrococcus ureæ. It is also different from that phase of incipient decomposition through which all urines exposed to the air pass on their way to ammoniacal putrescence. In incipient decomposition, as in non-septic bacteruria, the urine is opalescent, and swarms with bacteria; and I could not distinguish, by their structural characters, the organisms in the two contrasted conditions. Nevertheless, a difference of vital properties evidently exists between them. The sequence of events in the two cases is strikingly different. In a normal urine which passes on to decomposition the secretion is at first transparent and afterwards becomes opalescent; this opalescence, when once established, is permanent; and the organisms to which it is due grow and multiply in the urine. In non-septic bacteruria, on the other hand, the urine is at first opalescent and afterwards becomes transparent; and the organisms which cause the opalescence sink to the bottom of the vessel and cease to multiply.

The precise relations between the organisms and the symptoms in these cases require further elucidation. In women this condition may certainly exist without producing symptoms, or only a slight increase in the frequency of micturition. It is also seen in Case 3, that the urine continued to manifest the presence of bacteria, though not in large numbers, after the symptoms had entirely passed away. If it be assumed that, in these

* One sample was maintained at blood heat for several hours, but the bacilli showed no signs of multiplying in it.

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cases, a colony of bacterioid organisms becomes established in or on some portion of the urinary mucous membrane, it is probable that the symptoms would vary in kind and in intensity, according to the exact seat of the colony and the degree of activity of growth going on in it. I do not think that the above recorded histories support the view that in all instances the irritation of the urinary passages originally arose from the action of bacteria. In some cases, as in Case 2, it appears much more probable that the original irritation arose from other causes, and that the advent of the bacteria-colony was a secondary event, and that this colony, once established, kept up, prolonged, and caused renewal of the irritation.

The mode in which the bacteria, or their germs, obtained access to the urinary passages is likewise a matter of doubt. In the Case of the young German, and in Cases 1 and 4, instruments had been repeatedly passed into the bladder (though at a period remote from the time when the cases came under observation), and the organisms might have been introduced directly from without by this means. But in the three other cases no instrument had ever been introduced into the bladder. In these the organisms must either have crept up along the urethra or have obtained admission through the circulation. In the female, the short and wide urethra provides a ready access into the bladder, and it is easy to conceive that wandering bacteria might thus find their way into that viscus from the external genitals. In the male, the length of the urethra renders this mode of entrance less probable, but not impossible.

In Case 3, where no instrument was ever passed, nor attempted to be passed, into the bladder, it is not unlikely that the organisms effected an entrance through the blood and kidneys. The fact that pain in the back was a prominent symptom in this case lends strength to this supposition. For although it has been amply proved that the urine of healthy persons is free from bacteria and their germs, it is not proved that such organisms may not traverse the kidneys in disease, nor even, as a rare exception, in health. It has been demonstrated, indeed, that when bacteria are introduced into the blood, they are able to traverse the kidneys and appear in the urine.*

The effect of full doses of salicylate of soda, in these cases, appeared certainly very striking. But great caution is necessary in deducing therapeutical conclusions. It seems certain that the disorder is susceptible of spontaneous cure. Two of the cases (2 and 3) were in course of convalescence when they came under treatment, so that the share of the salicylate of soda in the completion of the process was at least doubtful. In the Case of the young German, and in Cases 1, 4, and 5, it was difficult to resist the conclusion that the favourable result was due to the drug; and I leave that conclusion to be affirmed or set aside by future experience.

The group of cases which are here united together under the name of non-septic bacteruria are not the only cases in which I have observed bacterioid organisms in the urine. There are at present under my care in the Manchester Infirmary two cases of this kind which evidently belong to a different category. One is a woman suffering from intractable ulceration of the intestines with profound anæmia. In this patient the urine is always turbid when voided, and swarming with bacteria and micrococci. But it never becomes clear on standing, and the reaction is always neutral, or nearly so. The only urinary symptom is a slight increase in the frequency of micturition. In this case the urine resembles in all respects a urine which has become turbid from incipient decomposition. Salicylate of soda in full doses had not the slightest effect on the condition of the urine in this patient. The other is a well-nourished healthy looking boy, three years of age, who is suffering from recurrent hæmaturia. In this case, the urine is

* Cohnheim, "Allgemeine Path." ii. 197.

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always crowded with micrococci, which either stand singly or grouped in pairs like bacterium termo, or are aggregated into zooglœa-like masses. No bacteria-rods are ever seen in this urine. The reaction is sometimes acid and sometimes alkaline; and there is no pain nor undue frequency in the act of micturition. The boy was suspected to be suffering from stone in the bladder, but sounding yielded negative results.

Tabes ist eine interstitielle Degeneration des Bindegewebes in den Hintersträngen.

Prof. ADAMKIEWICZ, Cracow.

Man fasst bisher die Tabes als eine primäre Degeneration der Nervenfasern in den Hintersträngen auf, und rechnet sie deshalb den sogenannten Systemerkrankungen zu. Diese Auffassung ist daraus hervorgegangen, dass man bisher nur entwickelte Tabesformen untersucht, und in diesen Fällen diffuse Degenerationen der ganzen Hinterstrangsysteme gefunden hat. Diese Auffassung ist falsch. Der Vortragende hatte Gelegenheit, Tabes in ihrem Anfangsstadium zu untersuchen und fand, dass in diesem Fall die Tabesdegeneration nicht diffus ist, sondern aus sehr distincten und gut characterisirten Degenerationszügen besteht. Es schien dem Vortragenden, dass diese Bindegewebszüge dem Verlauf der Blutgefässe correspondiren, und, um diese Ansicht zu begründen, hat er Injectionen von Rückenmarken gemacht und in der That bewiesen, dass jene Degenerationszüge ganz genau dem Verlauf der Blutgefässe entsprechen. Beide, Degenerationszüge wie Blutgefässe, entsprechen:

1) Dem hinteren freien Rande der Hintersträngе;
2) Den hinteren Wurzelbündeln;
3) Der Scheidewand der hinteren Stränge;
4) Der Grenze zwischen den Goll'- und den Burdach'schen Strängen, und endlich
5) Den aus den inneren Rändern der Hinterkörner ausgehenden nach dem hinteren Rand der Hintersträngе convergirenden Zügen.

Mit dem Nachweis, dass die Anfänge der tabischen Degenerationen mit dem Verlauf der Blutgefässe zusammenfällt, ist die Auffassung der Tabes als eine bindegewebigen interstitiellen Degeneration entschieden. Denn nur das Bindegewebe begleitet die Blutgefässe, nicht die Nerven. So schliesst sich also die Tabes der Lebercirrhose an, die ebenfalls eine, von dem Bindegewebe ausgehende, interstitielle Degeneration ist. Dieser Nachweis von der interstitiellen Natur der Tabes ist klinisch unter Anderem desshalb wichtig, weil er die Erscheinung der, mit der Tabes häufig einhergehenden Sehnervenatrophie erklärt. Man hat jetzt nur anzunehmen, dass mit den Gefässen im Rückenmark gleichzeitig oder häufig die Gefässe der Sehnerven erkranken, um den Zusammenhang von Amaurose und Tabes plausibel zu machen. So lange man die Tabes als eine primäre Nervenerkrankung angesehen hat, war eine solche Erklärung unmöglich, weil anatomisch nichts von einem Zusammenhange der Sehnerven mit den Fasern der Hintersträngе bekannt ist. Endlich darf angenommen werden, dass der Nachweis von der interstitiellen Natur der Tabes zur Lösung der Frage beitragen werde, ob sie vorwiegend syphilitischer Natur sei oder nicht. Interstitielle Degenerationen entstehen wenigstens auf syphilitischer Basis.

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The Treatment of Phthisis by Residence at High Altitudes.

Dr. C. Theodore Williams, London.

The subject of the treatment of phthisis by residence at high altitudes has been before the medical profession for many years, and the number of cases of phthisis frequenting mountain sanitaria yearly increases, owing to the important advocacy in this country of Hermann Weber, Clifford Allbutt, and others. At first physicians were satisfied with registering the fact of the improved condition of their patients, but now sufficient time has elapsed for records to accumulate, to furnish evidence not only of the general action of these climates on the human frame, but also of their specific influence on the various organs, and specially on the organs of respiration and circulation.

These influences vary according to the altitude at which people reside, though it cannot be maintained that similar physiological effects are always produced at the same altitude in different parts of the world; the considerations of temperature, moisture, and shelter from winds all exercising an important influence. It is probably on these grounds that an explanation will some day be afforded of the different effects obtained at the same altitudes in the Andes and Himalayas. The *ioroche* or *puña*, or *mal des montagnes*, prevails at great altitudes, generally above 12,000 feet, but this again admits of important varieties, as it does not always affect new visitors to some of the greatest heights. It is well known that on Mont Blanc climbers complain most of the mountain sickness in the couloir, and lose it on arrival at the summit, and in several of the Himalayan passes these symptoms have been more marked at some thousand feet below the Col than at the Col itself.

Our business, however, at present, is not with the results of extreme altitudes varying from 12,000 feet to 29,000 feet on the human body, but with the more moderate and, therefore, more accessible ones, and specially with those of from 4,000 feet to 8,000 feet, where, owing to the absence of the *mal des montagnes*, the presence of suitable accommodation, and means of approach, consumptive patients can be placed, and can remain for long and consecutive periods of time.

It is my purpose in this communication to give an account of cases of phthisis from my own practice, who have passed periods of various lengths at Davos and other high-lying stations, and, comparing the results obtained with those of other observers, to deduce certain conclusions as to the influence of diminished pressure on the human frame.

No attempt will now be made to give an account of the meteorology of high level stations, or to discuss the factors of their climates, as I have done this elsewhere,* as far as it is warrantable to do so from our present information.

Let us, therefore, take a few striking examples of cases, and examine the rest in the form of statistics.

Case 1 (No. 11 of Table).—Mrs. D., aged 30, consulted me May 12, 1879. Two maternal aunts, two brothers, and one sister, had died of phthisis, the three latter of rather rapid forms of the disease. Her father had induration of the left lung, of many years' standing. She had been failing in health and strength for about a year, and during the last six months had been steadily losing weight, the total loss being 1 st. 6½lbs. Cough and night sweats appeared six weeks ago, the latter symptom having lately ceased, the former being slight and attended by expectoration. At present tongue clean, appetite poor, catamenia regular, temperature 98° Fahr., pulse and respiration normal, weight 8 st. 7 lbs. Physical signs: dulness, scattered crepitation audible over

* *Quarterly Journal of the Meteorological Society*, October, 1880.

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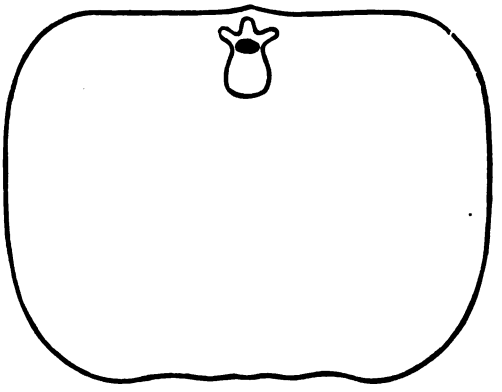
the upper third of the left chest, posteriorly. Ordered cod-liver oil, with hypophosphite of soda, dilute phosphoric acid, and strychnia, and an iodine liniment to the left back; also a dietary in which milk, sugar, and starch preponderated.

June 16.—Oil and tonic have been taken regularly. Appetite better. Cough less. Has gained 1½ lbs. Physical signs worse. Crepitation is now audible below the angle of the scapula, and in front just above the mamma. It is still audible in the upper left back, and the crepitation in the interscapular region has increased. Right chest measures just above the mamma 16½ inches. Left chest 15½ inches. Ordered to Davos until the spring. After three months' stay at Davos, I heard from Dr. Ruedi that she had considerably improved, and gained 7 lbs. in weight. He writes:—"Mrs. D. has little cough, eats and drinks well, and ascends the Schatz Alp without fatigue or shortness of breath. Temperature 98° Fahr., pulse 70, and respirations 16." In October, dreading the ennui of the approaching winter at Davos, she descended to Milan for a change, and was there attacked with violent diarrhœa followed by fever. Dr. Freeman, of San Remo, who happened to be at Milan, examined her, and advised a speedy return to Davos. After being laid up three days at Bellaggio, in consequence of the persistent diarrhœa, she safely reached Davos, and Dr. Ruedi found, in addition to the former physical signs, crepitation under the left clavicle. The diarrhœa subsided, but returned a week later, after the exertion of a long walk, and was accompanied by high temperature. She was confined to bed for six weeks with a fever, apparently of the intermittent type, accompanied by great sleeplessness, and eventually yielding to quinine, given every day, and morphia at night. The diarrhœa ceased, and in January, 1880, she had recovered sufficiently to ascend the Schatz Alp again, to sledge and "toboggin" down the sides of the Davos-Thal. In February Dr. Ruedi wrote to me that the physical signs were nil.

Fig. 12.



She remained at Davos till April, and then, after spending about a month at Berne and Paris, reached England June 29, 1880, where my notes were as follows:—

"Mrs. D. appears in robust health. She has a ruddy aspect, and can walk ten miles at a stretch. Weight, 8 st. 11½ lbs. Has quite lost her cough. Breath not short on exertion. Pulse, 70, strong. Respirations, 16. Chest measurements, just above mamma, right side, 17½ inches; left, 16½, being an increase of one inch for each side of the chest, or a total of two inches in the whole circumference. The subjoined cyrtometrical tracing (Fig. 12), taken at the level of third rib, shows the expansion to have taken place in both antero-posterior and lateral directions. The thorax appears full, but the movements are free. Several veins are conspicuous in the upper portions. The whole chest is hyper-resonant, this being most marked in the posterior portion of the left side. The breathing is harsh on the right side, and at the left posterior apex, especially where the resonance is greatest, emphysematous crackle is heard on deep breath. Below this the breath is harsh anteriorly and posteriorly. No dulness, crepitation, or bronchophony can be detected anywhere; the heart is not displaced."

January 24, 1881.—Last summer yachting, and has spent part of this winter in

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Cornwall and at Ryde. Has gained 6½ lbs., now weighing 9 st. 4 lbs. Continued quite well till Christmas, when in the severe weather cough returned, and since then expectoration has been twice streaked with blood. Careful examination could detect no fresh mischief anywhere, though in the presence of so much emphysema it might well be masked. Heard since that the cough had subsided.

August, 1881.—Patient remains in excellent health.

Remarks.—The result of mountain climate in this case is the more remarkable on account of certain unfavourable elements in the prognosis.

1.—The strong family predisposition.

2.—The distinct increase of local disease previous to reaching Davos.

3.—The occurrence of intermittent fever and its consequent debilitating influence on the constitution. Nevertheless, the arrest of the disease was complete and, so far, permanent.

Case 2 (No. 10 of Table).—Miss C., aged 25. Mother died of phthisis, being consumptive at the time of Miss C.'s birth; one sister and one brother phthisical. Cough and expectoration with wasting came on about three years ago, when consolidation of the right lung was detected by her medical adviser. She had wintered at Pau and Nice twice without any marked benefit. Her symptoms have continued up to the present time.

September 30, 1878.—Physical signs: dulness on the right side from clavicle to the third rib, and above the scapula. Cavernous sounds audible in the first interspace. Weight 7 st. 11½ lbs. Ordered to spend winter at Davos.

December 21, 1878.—Saw her at Davos in consultation with Dr. Ruedi. She had gained appetite and strength, and could walk for several miles. Breath fair, but short on rapid exertion. Pulse 80. Cough slight. Expectoration mucous. Measurements show no shrinking of the right chest, which measures half an inch larger than the left. Physical signs: dulness diminished over first interspace, and chest fairly resonant below. Cavernous sounds as before. Tubular sounds audible from the second to the fourth rib. Dulness with râle on cough above the scapula.

June 25, 1879.—Quitted Davos April 9, having had hæmoptysis to the amount of 1 oz. in March (at the commencement of the snow melting), and descended to Chur, and there had slight hæmoptysis; then passed five weeks at Baden-Baden, where she ascended the hills without difficulty. Has gained flesh (2½ lbs.) Cough and expectoration slight. Pulse 80. Physical signs: resonance on percussion over the whole right front chest, except over the outer third of the first interspace, which is dull; dry tubular sounds (not cavernous) are heard in the first interspace; marked resonance above the scapula.

	Right.	Left.
Measurements at level of third rib . . .	14¾ inches ...	15½ inches
" " mamma	14 " ...	14½ "
" " ensiform cartilage .	13 " ...	12 "

June 2, 1880.—Passed a second winter at Davos with great benefit. Is bronzed and strong. Still has slight cough, but no expectoration.

The chest is of a most peculiar shape, as will be seen by the accompanying cyrtometric tracing taken at the level of the third rib (Fig. 13). This shows great flattening and contraction of the right front chest wall, with some bulging of the lateral and posterior regions. The measurements are:—

	Right.	Left.
At the level of the third rib	15 inches ...	14½ inches
" " mamma	14¼ " ...	13¼ "
" " ensiform cartilage . .	12 " ...	11 "

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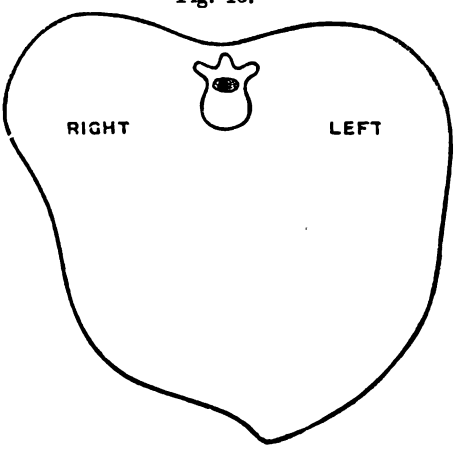
The whole right side is now completely resonant. Tubular sounds audible in the first interspace. On the left side breath is harsh and resonance very marked. The above measurements indicate enlargement of the upper part of the left chest, but the tracing shows the exact distribution of this, and the still more remarkable shrinking of the right.

Fig. 13.



October 27, 1880.—After residing for some time in a damp part of Somersetshire and over-exerting herself, profuse hæmoptysis occurred on August 26, and persisted to a large extent for five days, the amounts varying from 2 to 7 ounces a day; after this the expectoration was streaked up to September 12th, when it became clear again. Dr. Brittain, of Clifton, reports the right lung to have become consolidated throughout, after the hæmorrhage, but to have gradually cleared, and the patient recovered with breath much shortened.

A fortnight later (November 10) I saw her, and found, in addition to the old signs in the right lung, dulness and crepitation over the lower third of the posterior base and crepitation above the left scapula.

Recommended a return to Davos.

1881.—Heard that she had improved very much during the third winter at Davos, walking a great deal, and gaining 11lbs. in weight, and had got through the season without a day's illness. Dr. Ruedi's examination gives: "Dulness almost disappeared from the right lung, but remains round the lower margin of the lung, chiefly in axillary line. Here slight mucous râle is audible. Signs at the apex have much improved, and those heard at the left apex have disappeared."

Remarks.—This case is interesting from the extraordinary changes induced in the thoracic wall by the continued action of diminished atmospheric pressure, and also the variations in the degrees of dulness on the right side. The first winter gave rise to absorption of some of the consolidation at the left apex.

Case 3 (No. 8 of Table).—Mr. C., aged thirty-nine, consulted me, September 24, 1879. He had lived in India 18 years, and had had several attacks of intermittent fever. In 1874, whilst on furlough in England, he contracted a severe cold and cough, which lasted several months, but which disappeared, as his medical man stated, without any permanent damage to the lungs. In April, 1876, had severe chill and cough return, and in September after a thorough wetting during a journey to the Hills, he had inflammatory attacks which confined him to bed for three weeks, at the close of which the right lung was pronounced to be affected. He slowly recovered, and after another attack of intermittent fever returned to England in March, 1879, when on passing from the Suez Canal to the Mediterranean he was seized with a racking cough followed by loss of flesh. On arrival in England he stayed at St. Leonards three months under medical treatment, and gained 6lbs.; then came up to London for six weeks, and lost appetite and strength and weight, and cough became worse, and night-sweats returned. After six weeks at Southsea, some improvement was visible. At present, cough troublesome; weight 9st. 3lbs. Still has night-sweats. Pulse 48, weak. Respirations, 16. Physical

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signs: harsh breathing audible over upper right chest. On the left side cracked-pot sound in the first interspace with loud cavernous sounds, dulness and crepitation to fourth rib; dulness above left scapula. The following measurements were then taken:—

	Right.		Left.
At the level of the second rib	16½ inches	...	16½ inches.
„ „ mamma.	17 „	...	16 „

Patient was ordered cod-liver oil and a tonic, and to winter at Davos.

In March, 1880, Dr. Ruedi writes that Mr. C. improved until the middle of January, having gained several pounds in weight, the cavity having closed, and the cough being reduced; the patient then caught cold, and cough and expectoration increased, night sweats returned, he lost appetite and became feverish. Dulness increased over the left front. He was confined to bed six weeks, and gradually rallied from the attack, and left Davos at the end of March for Biarritz. Before leaving, Dr. Ruedi noted an increase of chest circumference of 3 centimètres, the pulse was 68. Respirations, 12, and the temperature normal. He was at Biarritz seven weeks, and suffered from bilious and gastric symptoms the whole time. He reached St. Leonards at the end of May, and the cough, which had been moderate, became exceedingly troublesome, accompanied by pain in the left side, much relieved by strapping, and night sweats checked by picrotoxine. He then removed to Upper Norwood, and found the climate suited better.

July 20, 1880.—Appearance thin. Pulse 72. Temperature and respiration normal. Tongue furred. Cough troublesome. Physical signs: on left side, hyper-resonance and harsh breathing as far as a vertical line corresponding with the middle of clavicle, and extending as low as third rib. Outside this, in the first interspace, cracked-pot sound and obscure cavernous breathing are audible. Below, breath is harsh and feeble, but over the lower third crepitation and occasional friction are heard. Posteriorly, harsh breathing and prolonged expiration over upper fourth and dulness. Crepitation audible (scattered) over lower third. The heart is drawn up and the impulse is visible between the fourth and fifth ribs, a little to the left of the normal. On the right side there is hyper-resonance and harsh breathing. The following were the measurements:—

	Right.		Left.
At level of second rib . . .	16 inches	...	17½ inches
„ „ mamma . . .	17 „	...	16¼ „

These notes, compared with those taken on the patient's leaving England, reveal a remarkable set of phenomena.

1.—The cavity in the left lung has contracted, and the heart has been drawn up.

2.—Emphysema has been developed over the upper lobe of the left lung posteriorly.

3.—There is pleuropneumonia (of recent origin) taking place in the lower lobe of the left lung.

4.—The right lung is hypertrophied, and has been drawn across the median line in front of the retracted left lung.

5.—Considerable expansion of the thorax has taken place, and this is most marked on the left side, especially in the upper regions. It is usual for a contracting cavity to cause shrinking of the affected side, but here the void has been filled, and over filled, by the development of emphysema behind the cavity and by the encroachment of the hypertrophied right lung.

October 19, 1880.—He has gained 3 lbs. Weight, 9 st. 4 lbs. Cough slight. Aspect greatly improved. Crepitation has diminished at the left base. Physical signs otherwise unchanged. In November he returned to his civil appointment in India in tolerable health.

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Case 4 (No. 7 of Table).—Mr. S., a clerk, aged twenty-three, seen by me April 29, 1879. Had suffered from nervous debility in 1877, and had been a voyage to Australia and back with great benefit. In the last few months he had overworked himself, undertaking, besides his daily duties in his office, shorthand reporting at night. Had been losing flesh steadily, and in last month caught severe cold. Cough came on with expectoration, and followed by night-sweats. Weight, 9 st. 3½ lbs. Physical signs: Loud bronchophony, tubular sound over the upper right front from clavicle to third rib. Slight dulness in the first interspace. Aspect pale and cachectic. Ordered oil with strychnia and phosphoric acid and hypophosphite of soda twice daily, and to pass six weeks at Eastbourne.

June 13, 1879.—Much improved in appearance. Has gained nearly 2 lbs. (9 st. 5¼ lbs.). Still cough and expectoration. Physical signs same. Ordered to Davos.

September 22, 1879.—Just returned from two months' residence at Davos, where he has walked eighteen miles a day, and gained 7 lbs. in weight (9 st. 12¼ lbs.). Was douched daily, and enjoyed the process. Cough and expectoration have ceased entirely. Respiration 12, pulse 72, strong. Aspect, the picture of health. Physical signs: chest hyper-resonant, with harsh breathing everywhere. No dulness, bronchophony, or tubular sound can be detected anywhere. To return to his work in London.

1881.—Has continued steadily at work, with the exception of a summer holiday. Has passed through the last two severe winters without any cold. He seems perfectly well, and about a year ago married.

Remarks.—Here the disease was probably due to over-work; there was general improvement from the stay at Eastbourne, but the state of the lungs was not improved till he reached Davos, the climate of which, combined with the abundant exercise, soon cleared up the rest of the mischief; and as there has been no return of the symptoms it may be regarded as a case of arrest of disease. Unfortunately, owing to some inaccuracies in the record of the second visit, we cannot compare the chest measurements at these different periods.

Case 5 (No. 9 of Table).—Mr. W., aged nineteen, seen in consultation with Mr. E. Parker Young, June 4, 1879, who supplied me with the following history. Mr. W. assists in a paper manufactory, and is there much exposed to inhalation of dust. Cough, with purulent expectoration, in July, 1878, followed shortly by loss of strength and flesh. Two months later he was so weak, and his breath so short, that he could hardly mount the slight ascent to Mr. Young's house in Delamere Crescent. He was at once sent to Davos, and in November had lost his cough entirely, and was able to walk twenty to twenty-five miles daily. He gained 16 lbs. in weight, and returned to England in April free from cough and shortness of breath, and with a ruddy and robust aspect. Physical signs: dulness over clavicle and first interspace on right side. Bronchophony in first interspace. Tubular sound above the scapula. Left chest hyper-resonant. Breathing harsh everywhere. Measurements:—

	Right.		Left.
At level of second rib . . .	16½ inches	...	17 inches
„ „ nipple	16½ „	...	17 „

September 29, 1879.—Has been over-exerting himself riding on a bicycle, and has lost 3 lbs. in weight. A week ago had fresh catarrh, and Mr. Young heard râles in the upper chest, but these have disappeared under vigorous counter-irritation to the chest wall with croton liniment. I measured him again.

	Right.		Left.
At level of second rib . . .	16¼ inches	...	16¼ inches
„ „ nipple	15½ „	...	15 „

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Comparison of these measurements shows a remarkable diminution in size at both levels, most marked at the middle of the thorax and on the left side. This is the more interesting, because the patient took abundant exercise in the form of bicycling since his return, but did not use any special means to keep his lungs in full play and vigour. The expansion arising from the residence at high altitudes was in this case temporary, not permanent as in many other instances, and it is on account of this feature that the case is cited.

Case 7 (No. 19 of Table).—Miss W., aged twenty, consulted me June 18, 1880. Her brother had died of phthisis. She had had cough and expectoration for one year, accompanied by night-sweats and slight hoarseness. In the last four months she had wasted to the extent of 11lbs.; present weight 8 st., aspect very delicate, complexion clear, with veins prominent. Physical signs: on left side of chest crepitation in first interspace, slight dulness above the scapula, with crepitation on cough. Ordered cod-liver oil with hypophosphite of soda and strychnia twice a day, and to spend one month at Malvern.

September 27, 1880.—Gained 2¼lbs. at Malvern, and appearance improved; breath short up-hill; has failed in strength since her return to London three weeks ago; cough worse; pulse, 84; respirations, 28; temperature normal. Physical signs: flattening and hyper-resonance, with slight bronchophony and crepitation from clavicle to third rib on the left side; dulness, bronchophony and crepitation above scapula, and in the left inter-scapular region. The following measurements were then taken:—

	Right.		Left.
At the level of second rib	16 inches	...	15 inches
„ „ mamma	15 „	...	14½ „
„ „ ensiform cartilage	14 „	...	12½ „

Cyrtometric tracing was taken. Patient was ordered to Davos.

April 29, 1881.—Remained at Davos till March 26, then descended to Thusis, and after halting there for ten days returned gradually to England. Dr. Ruedi insisted on her skating daily up to Christmas; after that date, on her ascending the Schatz Alp at least three times a week. Cough and wheezing troublesome the whole winter, and the hoarseness increased, but she grew much stronger, and though she found the ascents difficult, the breathing became easier, and at the present moment she can run upstairs with great facility. She gained altogether 13 lbs., but she has lost 3 lbs. since leaving Davos. Weight at present 8 st. 10 lbs. Her aspect is that of robust health, skin well bronzed, a full face, and a broad chest. Has quite lost her delicate appearance. Pulse 84; respirations 20. There is moderate development of fat and muscle on the thoracic walls. The chest is apparently deeper and also fuller (under the clavicles). The measurements are as follows:—

	Right.		Left.
At level of second rib	16¼ inches	...	15 inches
„ mamma	15¾ „	...	14¾ „
„ ensiform cartilage. . . .	14½ „	...	12½ „

This shows an increase in circumference at all levels, most marked on the right or healthy side. The cyrtometric measurements (Fig. 14) taken at the level of the mamma show that the expansion is entirely antero-posterior, and is arrived at by a diminution of the lateral diameter. Physical signs: hyper-resonance over the whole chest; no dulness or bronchophony anywhere. Crepitation is still audible in the first and second spaces on the left side, but less marked than before, and there is also less above the scapula. Some crackle is audible posteriorly; over the whole right side harsh breathing is to be heard. The cough has improved since she left Davos. Expectoration is yellow;

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voice still hoarse, and the laryngeal mucous membrane, as seen by the laryngoscope, in a chronic catarrhal condition. June 14, 1881. Voice tolerably clear, but high notes weak. The measurements are as follows :—

	Right.		Left.
At level of second rib	15½ inches	...	15½ inches
„ mamma	16 „	...	15¼ „

These show a slight diminution of size at the upper level, and an increase at the mammary level, the latter being due probably, to a further development of the breasts. Physical signs the same.

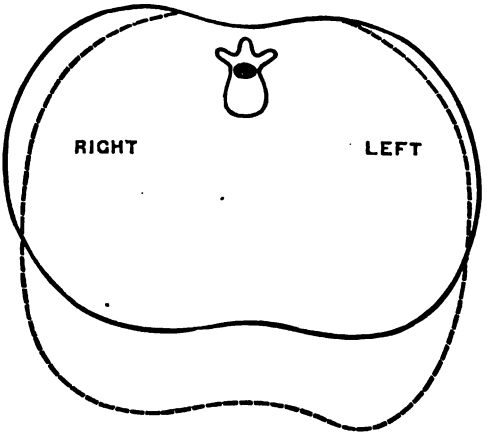
Remarks. — The interesting points in this case were—

1.—The great transformation in the aspect and frame of the patient, caused by the mountain influence in spite of the catarrhal symptoms persisting.

2.—The advance of the disease in England, in spite of the residence at Malvern, and its limitation (probably by the development of emphysema around the tubercular masses) at Davos. The improvement in breathing was probably due to the increased development of the right lung.

3.—The presence of phthisical family history.

Fig. 14.



Before residence at Davos ——
After - - - - - - - - - - - — — —

Case 7 (No. 17 of Table).—Miss Ca., aged 19, seen September 24, 1880. She had had cough and expectoration for two and a half months, accompanied by loss of flesh and shortness of breath, especially on ascents. Catamenia have been irregular and scanty; appetite good, pulse and temperature normal, weight 7 st. 7½ lbs. Physical signs: on the left side cracked-pot sound in first interspace and dulness below to the third rib. Some dulness above the scapula. Croaky crepitation audible in first interspace. Crepitation above the scapula. The following measurements were taken :—

	Right.		Left.
At level of second rib	15 inches	...	14½ inches
„ ensiform cartilage . . .	13½ „	...	13½ „

A cyrtometric tracing was taken at the mammary level as seen below.

Ordered to winter at Davos and to take cod-liver oil and hypophosphite of lime.

June 8, 1881.—Wintered at Davos, where, at first, the cough was very troublesome, but eventually improved. The shortness of breath gradually decreased, and before leaving she could easily mount any of the hills. Quitted Davos at the end of March, and spent two months at Glion (2,050 feet), where the cough still further improved and more flesh was gained, making in all 9½ lbs.—4 lbs. at Davos and 5½ lbs. at Glion—present weight 8 st. 6 lbs. At present there is only morning cough and whitish expectoration. The breath is no longer short on exertion; her aspect is that of robust health. Inspection of the chest shows marked fulness and fair expansion in all parts. Physical signs show some cracked-pot sound in the first interspace on the left side and hyper-resonance below, most marked in second interspace. Some dulness is to be detected over

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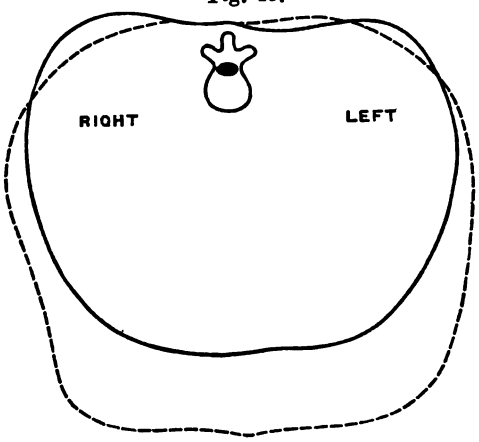
upper third posteriorly. Coarse crackle audible to third rib and above the clavicle. Below the third rib the breathing is good. No cavernous sound audible anywhere. Measurement:—

	Right.		Left.
At level of second rib	16 inches	...	16 inches
„ mamma	15¼ „	...	15½ „
„ ensiform cartilage	13½ „	...	13½ „

Cyrtometric tracings, compared with that taken before repairing to Davos, give an enormous increase in the antero-posterior diameter, and a slight one in the lateral diameter (Fig. 15).

The measurement shows an increase at the second rib level of 1 inch on the healthy side and 1½ inches on the affected, being a total of 2½ inches. At the mammary level the cyrtometer shows a large increase, but there were unfortunately no measurements taken at the first visit, and at the ensiform level no increase at all was observed. The patient was advised to spend a second winter at Davos.

Fig. 15.



Before residence at Davos ——
After - - - - - - - - - - - - - — — —

Remarks.—This case much resembles Case 6, and shows how the ordinary curative process of cavity contraction is modified by the development of emphysema. With the manifest contraction of the cavity at the left apex we should have expected a flattening of the left front, but the reverse took place, and expansion was the result. The influence on the cough is also to be noted, the cough being slow to subside, and, in some instances, it appears to have been irritated by the mountain climate.

Case 8. (No. 16 of Table).—Mr. O. C., a horse-dealer, aged 33, sent to me by Dr. Cremen, of Cork, April 7, 1880, with the following history.

Had broken his leg, riding, some years previously, and had been lame ever since, but enjoyed fair health up to June, 1879, when a tickling cough came on, accompanied by loss of appetite and flesh. Slight hæmoptysis occurred on Christmas Day, followed by slight chills and sweats. Under Dr. Cremen's advice, he relinquished his occupation, and went to reside on high ground outside the city. Here he lost the night-sweats and chills, the cough gradually diminished, and he gained 6 lbs. in weight. At present, cough slight, expectoration whitish, pulse 84, temperature 98° Fahr., respirations normal. Physical signs: on right side slight dulness, bronchophony to third rib, crepitation on cough, in first interspace, and in the supra- and infra-scapular regions. The following measurements were taken:—

	Right.		Left.
Opposite the second rib	15½ inches	...	15½ inches
At level of mamma	16 „	...	15½ „

He was ordered to take cod-liver oil with hypophosphite of lime, and to pass the summer Davos.

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October 14, 1880, spent 3½ months at Davos, and during the first month suffered much from sore throat, bnt took no medicine, and, though lame, managed to walk three miles a day, but did not make many ascents. Appears in excellent health and free from cough; has gained flesh. Since his return to London he has suffered from nocturnal dyspnœa. Physical signs: on right side slight flattening and deficiency of movement from the clavicle to the third rib. Slight crackle or cough in the first interspace. Slight bronchophony audible above the scapula. Below, breath excellent. No dulness or crepitation anywhere. On left side hyper-resonance and harsh breathing. Measurements:—

	Right.		Left.
At level of second rib	16 inches	...	16½ inches
„ mamma . . .	15¼ „	...	16 „

Showing an increase of 1½ inches at the upper level, but none at the lower. Weight 9 st. 10 lbs.

December 17.—Returned to Cork, and began to fall off in health at once. Cough and expectoration returned, followed by night sweats and loss of flesh (4 lb.). Crepitation audible in the upper right front, and some friction at the posterior base. The measurements show a distinct diminution of all parts of the chest during the residence of two months at lower levels amounting to one inch at the upper portion.

Ordered to Hastings, where wintered.

April 13, 1881.—Improved greatly, and gained 11 lbs. at Hastings; still has cough and expectoration. Breath short on exertion. Physical signs: show more development of emphysema.

Remarks.—This case is interesting, because, owing to the lameness of the patient, he could not undergo the large amount of exercise generally taken at Davos. Nevertheless, his thorax expanded, showing that the cause of the expansion was not exercise, but the respiration of rarefied air.

Case 9 (No. 15 in Table).—Miss M., aged 22, seen first March 17, 1880. She had congestion of the lungs in November, 1879, and since that attack cough has persisted, and been accompanied by shortness of breath and some loss of weight. Weight, 9 st. 10 lbs Physical signs: some dulness in the left chest in the first interspace, more marked in the scapular and supra-scapular regions, where there is also bronchophony. Ordered cod-liver oil, with dilute phosphoric acid and quassia, counter irritation with an iodine liniment, and a morphia and squill linctus, also a very liberal dietary.

May 31.—Improved first and gained 2 lb., but went to Brighton and cough became more convulsive. Dulness however lessened in upper left front, but remains exactly the same in amount in the posterior regions. The following measurements were then taken:—

	Right.		Left.
At the level of the second rib . .	15½ inches	...	15½ inches
„ „ mamma . .	15 „	...	15 „

A cyrtometric tracing of the chest was then taken. Ordered to Davos for two months.

August 17.—Has passed two months at Davos walking and climbing largely. Has ascended the Schiahorn and the Swartzhorn (10,000 ft.) and several times walked twenty miles in a day. Cough disappeared at the end of the first month, and the appetite increasing, she gained flesh, all shortness of breath on exertion also disappeared. Aspect well bronzed and very robust. Weight 10 st. 5¾lbs. (a gain of nearly 8lbs.). Respirations 19; pulse 96; temperature normal.

Physical signs: both sides of chest hyper-resonant. Breathing harsh everywhere. No displacement of heart. All dulness and bronchophony have entirely disappeared.

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The thorax is evidently much enlarged, and the breasts are also more developed, making measurements difficult. The following, however, are tolerably correct :—

	Right.		Left.
At the level of second rib	16 inches	...	15¾ inches
Just below mamma	16½ „	...	16½ „

The measurements show an increase amounting to 3 inches at the mammary level, and ¾ inch at second rib.

May, 1881.—Heard that she had passed the winter well, in Hertfordshire, and remained quite free from cough.

August, 1881.—Well, except a slight cough.

This case was one of slight consolidation of one apex of some months' standing, which did not disappear under treatment of several months in England. The effect of the vigorous exercise at a high altitude caused absorption of the pneumonic products, or emphysema around them and hypertrophy of therestof the lung tissue. The mountain climbing was carried out more vigorously than in any of the cases, and with the best results.

Case 10 (No. 18 of Table).—Mr. G. M. G., aged nineteen, sent to me by Dr. Watson of Montrose, September 27, 1880. Grandfather (maternal) and uncle (maternal) had died of phthisis. Had severe bronchitis two years ago, and confined to his bed for fourteen weeks. Since that date he has had cough and expectoration and occasional attacks of asthmatic breathing, but no great loss of flesh. In the autumn of 1879, Dr. Watson detected some consolidation at the apex of the left lung, and ordered the patient to winter at Cannes. There he spent three months, and then passed six weeks at Rome, where he caught intermittent fever, and six weeks at Florence, where bronchitis came on; still he recovered and returned to Scotland improved, and Dr. Watson found the physical signs diminished. At present he appears overgrown, and has a very delicate aspect. Pulse and respiration normal.

Physical signs: slight dulness and tubular breath sound in first interspace on the right side. Some dulness above the right scapula. Measurements :—

	Right.		Left.
At the level of second rib	15½ inches	...	16 inches
„ „ mamma	15 „	...	15½ „

Ordered to take cod-liver oil and arsenic, and to winter at Davos.

May 28, 1881.—Wintered well at Davos, and lost cough soon after arrival. Spent his time in skating, walking, and tobogginning, and gained 2½lbs. in weight, and was quite free from asthma.

Quitted Davos at the end of March, and descended to Thusis, there halting a week proceeded to Clarens (Lake of Geneva), where he passed three weeks, and had a severe attack of asthma; thence to Baden, where he had another attack. Is now quite free from cough or dyspnœa, and appears in excellent health. The chest is largely developed, chiefly in the upper portions. Cyrtometric measurements show great enlargement between first and third ribs, but more at mammary level. The following measurements confirm this :—

	Right.		Left.
At the level of second rib	16¾ inches	...	17 inches
„ „ mamma	15½ „	...	15¼ „

Physical signs: tubular sounds in the first interspace; hyper-resonance and puerile breathing everywhere else.

Remarks.—This case was complicated with asthma, and therefore might be confused with it, but the signs of consolidation at the right apex were unquestionable, and it may be considered to be a case of catarrhal origin. The benefit the patient reaped as regards

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his asthma was very striking. The usual dilatation of the chest took place, but chiefly at the upper regions.

Let us now consider the whole series of cases as embodied in the table.

This table shows that the patients* were twenty-two in number, sixteen males, and six females. Their ages varied from eighteen to forty-eight, the average amongst the males being twenty-eight; among the females twenty-four.

Disease.—The disease in all these cases was phthisis, and generally of the ordinary chronic kind. There were two examples of hæmorrhagic phthisis—*i.e.*, of the variety distinguished by large hæmoptyses and limited consolidations—and in two instances the disease had a distinctly inflammatory origin. Nineteen patients had one lung affected; three had both lungs affected; seven had cavities, and the rest consolidations which were, as a rule, limited to one lung, and this fact should be borne in mind when judging of these statistics.

Length of illness.—This varied, with one exception, from three months to five years previous to the patient undergoing high altitude treatment. In the majority of cases the disease was of two years' standing. One remarkable case had a history of twenty years' illness of a very chronic description.

Family predisposition was present in eight cases; very strongly in two (Nos. 10 and 11) of the patients. Eight had been under the influence of other forms of climatic treatment, previous to trying high altitudes, such as sea voyages, Egypt, Algiers, and the Riviera, for one or more winters. The rest made trial of high altitude treatment only.

Length of Residence at High Altitudes.—This varied greatly in different cases. One patient resided for six months at Bloemfontein, South Africa, another for ten months in the Transvaal, the rest spent periods of from two to eighteen months at Davos, the average of stay being six months.

It is curious to note that some of the patients, who passed only two to three months of the summer, reaped great benefit. In eleven cases, periods varying from two to nine months sufficed to ensure complete arrest of the disease. For the others, longer periods were necessary.

Results.—These are classed as general and local; the former indicating the effect on the constitutional strength on the colour and weight of the patient; the latter, the special result on the lungs.

Of the twenty-two patients, two deteriorated while at Davos, and died three months after leaving the place. In each instance a considerable amount of lung surface was involved. In one a cavity was undergoing contraction, but the whole lung was in a state of fibrosis, and the pulmonary area was greatly contracted; in the other case a cavity existed in one lung, and consolidation in the other. These two patients suffered greatly from the rarefaction of the air, and displayed a certain degree of lividity of countenance at high altitudes. They were a valuable warning that, in our selection, we must take as much note of extent of disease as of advance or of activity. Of the other twenty patients, sixteen showed "great improvement," and four "improvement." Of these last, one relapsed on returning to England, but regained her strength again after another sojourn at Davos.

The cases already cited give a fair sample of the kind of improvement. Of the twenty-two patients no less a number than ten returned sufficiently restored to health to resume their ordinary occupation in business or society, and they may be fairly called instances of arrested phthisis.

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* The first six cases were published in full in the *Lancet*, 1879, and are now only reproduced in abstract for statistical purposes.

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Name.	Sex.	Age.	Disease.	Length of Illness.	Hereditary Predisposition.	Length of residence at high altitudes.	Result. General.	Result. Local.	—
1 Mr. R. S. G. (Published in *Lancet*, 1879.)	M	20	Phthisis (Hæmorrhagic.)	8 months.	None.	5 months at Davos.	Great improvement.	Arrest of disease; development of Emphysema.	Patient gained 21 lbs. till snow melting, when lost 9 lbs.
2 Mr. E.	M	22	Phthisis III.	13 months.	None.	6 months at Davos.	Great improvement.	Arrest of disease; cavity contracted; Emphysema developed.	
3 Mr. H. S.	M	40	Phthisis III. rt. 1st lt.	3 years.	None.	6 months at Davos.	Deterioration.	Increase of disease.	Died 3 months after leaving Davos.
4 Mr. W.	M	18	Phthisis III. fibrosis of whole lung.	4 years.	None.	6 months at Davos.	Deterioration.	Increase of disease.	Died 2 months after leaving Davos.
5 Mr. J. N.	M	28	Phthisis III. whole of left lung consolidated.	2 years.	None.	Davos, 2 months; South African highlands, 10 months; Davos, 4 months.	Great improvement.	Partial arrest; contraction of cavity; Emphysema developed.	Returned to South Africa to practise profession there.
6 Miss B.	F	30	Phthisis I. both.	5 years.	Father, brother, and sister died, phth., and 2 aunts.	Bloemfontein and South African highlands for 6 months.	Great improvement.	Complete arrest.	Lately subject to epileptic fits.
7 Mr. S. . .	M	23	Phthisis I.	6 months.	None.	Davos 2 months.	Great improvement.	Complete arrest; Emphysema developed.	
8 Mr. C. . .	M	39	Phthisis III. lt. 1st rt.	3½ years.	None.	Davos 6 months.	Great improvement.	Partial arrest; contraction of cavity; Emphysema developed.	Sufficiently recovered to return to his appointment in India.
9 Mr. W. . .	M	19	Phthisis I.	4 months.	Brother and sister, phth.	Davos 6 months.	Great improvement.	Complete arrest; Emphysema developed.	
10 Miss C. . .	F	25	Phthisis III. right.	3 years.	Mother, 1 sister died, phth. 1 brother, phth.	Davos 3 months. Davos 18 months.	Improvement with relapse on return to England.	Partial arrest; Emphysema developed.	
11 Mrs. D. . .	F	30	Phthisis I.	1 year.	2 brothers, 1 sister, died, acute phth.; 2 mat. aunts died phth.	Davos 9 months.	Great improvement.	Complete arrest; Emphysema developed.	
12 Mr. B. . .	M	25	Phthisis I. b.	6 months.	None.	Davos 6 months.	Great improvement.	Complete arrest.	
13 Capt. R. . .	M	44	Phthisis I.	20 years.	Pat. cousin died, phth.	Davos 6 months.	Improvement.	Complete arrest; Emphysema developed.	
14 Mr. P. . .	M	48	Phthisis I.	6 months.	None.	Davos 5 months.	Improvemnnt.	Partial arrest.	
15 Miss M. . .	F	22	Phthisis I.	6 months.	None.	Davos 2 months.	Great improvement.	Complete arrest; Emphysema developed largely.	
16 Mr. O'C. .	M	33	Phthisis I.	10 months.	None.	Davos 3½ months.	Great improvement.	Partial arrest; Emphysema largely developed.	Relapse after return to Cork.
17 Miss Ca. . .	F	19	Phthisis III.	3 months.	None.	Davos 6 months.	Great improvement.	Partial arrest; large amount of Emphysema.	
18 Mr. G. M. G. .	M	19	Phthisis I.	2 years.	None.	Davos 6 months.	Great improvement.	Complete arrest; Emphysema as above.	
19 Miss W. . .	F	20	Phthisis I.	1 year.	1 brother died phth.	Davos 5 months.	Great improvement.	Partial arrest; Emphysema	
20 Mr. Cu. . .	M	29	Phthisis I.	2 years.	None.	Davos 6 months.	Great improvement.	Complete arrest; Emphysema.	
21 Mr. B. . .	M	23	Phthisis I.	5 years.	None.	Davos 14 months.	Great improvement.	Partial arrest.	
22 Mr. P. . .	M	25	Phthisis (Hæmorrhagic.)	1½ years.	Brother, phth.	Davos 6 months.	Great improvement.	Partial arrest; Emphysema.	

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The local results, as shown by measurements and physical signs, are very interesting, and will be dwelt on at greater length presently. Excepting the two fatal cases all the patients showed great local improvement. In no less than eleven, or in one half, complete arrest took place; in nine partial arrest was noted; in no less than sixteen development of emphysema round the seat of disease was detected, accompanied generally by hypertrophy of the healthy lung. Let us now consider the effect of mountain climates on some of the organs and functions of the body. The tanning of the skin from the sun's rays is as well marked in winter as in summer, which is explained by Messrs. Volland and Krieger's* observation on the solar radiation thermometers at Strasbourg and at Davos (5,200 ft.), these showing that while the sun power is greater in July at Strasbourg, it is greater in October at Davos. This is due to the wonderful diathermancy of the rarefied mountain atmosphere, specially when the cold has condensed or frozen any floating vapours.

The effect on the sudoriferous glands is of a tonic description, and the night-sweats of phthisis are decidedly reduced.

Appetite.—Increase of appetite generally appears early, and shows itself in the large meals taken by invalids of habitually capricious tastes, though the food, and particularly the meat, is by no means always of the best kind. Gain of weight generally follows. In 119 phthisical patients who passed last winter (1880-1) at Davos, the numbers, including a certain proportion of serious—it might be said hopeless—cases, 87, or 73 per cent., gained weight; six, or 5 per cent., remained stationary; and twenty-six, or 21 per cent., lost weight.

In my twenty-two tabulated cases, eighteen gained weight, two were stationary, and two lost weight. The gain of weight varies in different cases, and in one instance reached 25lbs. We may, however, conclude that, as a rule, such gain of weight takes place, and that it is generally accompanied by gain of colour and strength.

Circulation.—The effect on the circulation has been largely discussed, and great difference of opinion exists as to its extent. Careful sifting of the evidence shows that the differences depend partly on the altitude at which the observations were taken, and partly on whether the subjects of observation are natives of the mountains or strangers from the lowlands. Many observations have been made on strangers ascending for the first time to high altitudes, the period selected being shortly after the ascent. In Sir Douglas Forsyth's expedition to Yarkand in 1870†, the pass of Changla was traversed at an elevation of 18,000 feet. Henderson's observations, made on members of the expedition after half an hour's rest at the summit, give the following results, the barometer being 15·73 inches, the thermometer 61° Fahr., and water boiling at 181° Fahr.:—

Mr. Henderson,	who had	walked to	summit,	pulse	80	respirations	26
Sir D. Forsyth,	„	ridden	„	„	100	„	22
Mr. Shaw,	„	ridden	„	„	94	„	
A Punjabee,	„	ridden	„	„	92	„	
Another Punjabee,	„	walked	„	„	93	„	
A Tibetian,	„	walked	„	„	78	„	

All the party, with one exception, appear to have been dwellers in the plains and unaccustomed to the mountains; and all Punjabees and English, walkers and riders, had their pulse rate increased. The Tibetian on the other hand, though he walked to the summit, did not appear in the least affected. Quickening of the pulse has been

* H. Weber, "Klimatotherapie Allgemeine Therapie" (Ziemssen), p. 129.

† Henderson and Hume, "Lahore to Yarkand, under T. D. Forsyth." 1875.

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noted by travellers in all great ascents; by De Saussure* in the high Alps, by Kellet† in the Himalayas, by Dennison‡ in the Rocky Mountains, who lays down a law of an increase of 2 per cent. for each 1,000 feet ascended. That this, however, is not only want of habit and of temporary character, is shown by the instance of those inhabiting very high altitudes. Zapater§ found the natives of Janja (10,000 feet) had a higher rate of pulse and respiration than the normal. Dr. Kellet‖ found that the Paharees or Hillmen of the Himalayas in the neighbourhood of Landour (7,000 ft.), had quickened pulse and respiration.

Coming to lower levels, Armieux¶ found at Barèges, in fourteen soldiers who had removed from Toulouse, that after a thirty-five days' residence the pulse had diminished on an average four beats.

Hermann Weber's observations** on forty-eight healthy inhabitants of the Swiss mountains, such as guides, doctors, and their families, give about the same pulse rate as that of the dwellers in the plains.

In forty-four visitors to the mountains, remaining there for periods of from twelve days to six months, thirty-two showed no perceptible difference in pulse rate; ten showed an increase of from 5 to 18 per cent., and two a decrease of from 8 to 15 per cent. Unfortunately, not enough is told us of these persons to judge of their condition of health and disease. Dr. Ruedi's observations, made at my request on 117 phthisical patients who had spent six months at Davos showed an increase of pulse in 18 (15 per cent.) a decrease in 90 (77 per cent.) and a stationary condition in nine cases (7½ per cent.).

The increase was, as a rule, from two to ten beats, the decrease on the other hand, was more marked and often from 15 to 30 per cent., a fall from 120 to 92 not being uncommon. Dr. Ruedi's experience at Davos is that the pulse is the first symptom to show a change and to lessen in frequency. My own notes, taken from a small number of consumptives seen before wintering at Davos, in the middle of their stay there, and again on their return to England, lead me to believe that for the first few weeks the pulse rate is slightly increased, but that later on the heart beat becomes more powerful and the pulse rate diminishes with stronger volume. I could not help noticing how common it was to see the cardiac impulse even in pallid young women, where it had only been felt with difficulty. These records and experiences indicate that for elevations not exceeding 6,000 feet, the pulse rate for natives does not differ from the ordinary normal standard, and for strangers, and specially in consumptives, there is a quickening of the normal rate at the commencement of residence, and a diminution of it at a later date accompanied by more powerful cardiac impulse and a fuller vascular system. With regard to the effect of higher altitudes than 6,000 feet, the evidence on the whole points to a decided increase in the pulse rate.

Respiration.—The influence of high altitudes on the respiration is very marked. Observations of the natives of all the great mountain ranges indicate a quickening of the rate, though, owing to a peculiar conformation of the chests of mountaineers, to be presently alluded to, the rate is lower than that of lowlanders visiting these regions for the first time.

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* "Voyage dans les Alpes, 1786–1796."

† "Army Medical Reports." ‡ "Rocky Mountains Health Resorts."

§ Ornella's "De l'Influence du Climat des Andes sur la Phthisie."

‖ "Army Medical Reports."

¶ "Effets Physiologiques du Climat et des Eaux de Barèges." 1873.

** "Klimat Therapie," p. 132.

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Lortet* noted that the respirations were more frequent at the summit of Mont Blanc than in the plains, but he showed by careful tracings that their depth was not so great. Jaccoud† affirms that the number and amplitude and respirations increase in the Engadine (5,000 to 6,000 feet above sea). Armieux's‡ observations on fourteen soldiers at Toulouse, and afterwards when they had removed to Barèges, show an increase in the number of respirations and an entire absence of harmony between the respirations and circulation.

The period of stay at Barèges was only thirty-five days. Coindet§ noted that the respiration rates of French soldiers in France and Mexico were as 19·6 to 16. Hermann Weber gives the following:—Of forty-two individuals transplanted from 1,200 to 2,400 metres above sea level, 94 per cent. showed a few days after arrival an increase of two to five respirations per minute. Among his healthy mountaineers living at elevations of 4,000 to 5,000 feet, the average rate did not differ materially from that of an equal number of healthy individuals living on the plains.

Among Dr. Ruedi's 116 cases of phthisis in various stages who passed six months at Davos, ninety-five (80 per cent.) showed at the close of their visit a decreased rate of respiration, six (5 per cent.) an increased rate, and sixteen (14 per cent.) no increase or decrease.

These latter results at first sight appear at variance with Weber's and the Barèges ones; but it must be remembered that the time of stay of the patients at the above places, Barèges, Engadine, and of Weber's cases was short, not exceeding thirty-five days, whereas at Davos it was long, at least six months, and this affords the explanation; for at Davos it is found that at first the respirations are increased, but as the respiratory movements gain in depth, so they diminish in frequency, and that this change coincides with dilatation of the chest wall, enabling the individual to take deeper breaths, and therefore rendering a smaller number sufficient.

We may conclude, therefore, that at moderate altitudes the respiration rate of strangers is at first increased, but, later on, the lungs and thorax becoming expanded, the rate returns to the normal, but the respirations are deeper. The respiration rate of natives of moderate altitudes therefore does not differ from that of lowlanders, but at great altitudes there is a decided increase in the number of respirations.

Temperature.—The influence of the mountain climate on the temperature of healthy persons and on cases of quiescent phthisis is nil. In the case of pyrexia arising from almost any cause, and specially in the case of the pyrexia of phthisis, the tendency is to stimulate the fever, and thus hinder its subsidence. Dr. Ruedi writes to me "that the fever or high temperature has to sink within six weeks of the patient's arrival at Davos, or the patient is in great danger."

Changes in the thorax.—Of the various changes induced by a residence at high altitudes the expansion of the thorax and the accompanying modifications in the lungs and heart are the most striking. The large size of the chest of dwellers in high regions has been noted in various parts of the world; in the Andes among the Indians by Orbiquy,‖ Burnet,¶ and Zapater, in Mexico by Jourdanet,** who found the chests of the Indians to be increased, not only in the lateral and antero-posterior diameters, but also in the vertical, the sternum being elongated, and giving an awkwardness to the

* "Pression Barométrique," by Paul Bert. 1878.
† "Station Médicale de St. Moritz." 1873.
‡ *Op. cit.*
§ *Gazette Hebdom.* 1863.
‖ Paul Bert, *op. cit.*
¶ Paul Bert, *op. cit.*
** "Les Altitudes de l'Amérique Tropique."

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appearance. The observations of Forbes and Walshe confirmed those of Jourdanet, but Coindet, who accompanied the French expedition to Mexico, found that the French soldiers measured more at the level of the nipple than the Mexican ones. It is not stated, however, whether or no these latter had all lived at high altitudes. Armieux found a considerable increase of chest circumference in eighty soldiers after residence at Barèges of forty-three days, and still further increase at the close of four months' stay. The average increase was twenty-five millimètres. In the Himalayas Kellet found the European soldiers at Landour (7,300 feet) increase in chest circumference at least an inch during their stay of six months. A similar effect has been noted by myself with regard to the elevated regions of South Africa in one instance. At Davos this expansion of the chest has been abundantly proved by the observations of Drs. Hermann Weber, McCall Anderson, Ruedi, and myself, in patients residing during periods of two months and upwards. Dr. Weber noted an increase of from one to three centimètres in fourteen patients whose chests were badly developed, but who had no lung disease. At my request Dr. Ruedi has carefully measured 105 consumptives, before and after residence at Davos. These patients included all stages of phthisis, and whilst embracing many early cases who were enabled to take large exercise, it also included those who, through fever and other complications, were confined to bed, and therefore could not use any muscular exertion. Among these 105 the circumference increased in ninety-five (90 per cent.), decreased in four cases, and remained the same in six. Dr. Ruedi remarks that the increase in circumference is not produced by accumulation of fat or muscle alone, as in some of these patients it was accompanied by no increase, but even by decrease, in weight. My own observations on twenty-two cases, of which examples have been given, show an increase varying from one to three inches at different levels of the chest wall. The above facts prove beyond doubt the expansion of the thorax, both in natives and, still more, in visitors to high altitudes. Pursuing my investigations, I resolved to ascertain, if possible, what regions of the thorax are specially involved in the process of dilatation; what relation the expansion of the chest wall bears to the area of diseased lung; what direction the line of dilatation takes, and whether it affects the upper or the lower regions most; lastly, the length of residence at high altitudes necessary to ensure these results, and their permanence or non-permanence on descending to lower regions. To solve these questions a large number of measurements and cyrtometric tracings were taken before and after a residence at Davos; and again when an interval of several months had elapsed after the patient's return to low levels, and the conclusions arrived at were the following:—

1.—That, as a rule, the portion of chest wall overlying the healthy lung more frequently undergoes dilatation than that overlying the diseased lung.

2.—That the dilatation may be in either an antero-posterior or lateral direction, or sometimes in both. The first mentioned is most common.

3.—That the dilatation is more common in the upper regions of the chest than in the lower. The largest dilatation takes place at the level of the third rib in front, the smallest about the level of the ensiform cartilage. (Cases 6 and 7 show this last feature well.)

4.—That, if the disease be limited to the apex of one lung, there may be flattening of the chest wall immediately over the lesion, and extensive dilatation of the lower or lateral portions of that side, giving rise to remarkable deformities of the thorax.

5.—That the length of residence required to produce this thoracic expansion varies in different cases, but that, as a rule, some months are necessary; it being dependent on the respiration rate and on the yielding or non-yielding nature of the thoracic wall. As a rule, thoracic expansion is most rapid in patients who take much exercise.

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6.—That the duration of this expansion after a return to lower levels varies, but in the majority of patients is permanent. In one case considerable dilatation only lasted seven months, in another only six months.

State of the Lungs.—The modifications in the lung sounds which accompany this expansion of the thorax next engage us. What we note on examining the chest of a patient just returned from a high altitude station is its fulness. There is no clear marking out of intercostal spaces, the supra-clavicular hollows seem filled up, and there is often more apparent flattening in the region of the original lesion than when the patient started for the mountains. This is caused no doubt by the development of emphysema around the old lesion.

Percussion as a rule gives hyper-resonance over nearly the whole chest, most on the healthy side; next over the healthy portions of the affected side, leading off into resonance, and even into slight dulness over the seat of the old lesion. The hyper-resonance which most attracts our notice is that given by inter-scapular, supra-scapular, and scapular regions, perhaps, because we least expect to find it in these quarters; but in lungs free from disease, where owner has resided for several months at high altitudes, taking abundant exercise, hyper-resonance is to be found in all regions of the chest. Diminution of dulness was generally noted in my patients. On auscultation we find in the healthy side that the respiration has become harsh and puerile, the inspiration long and deep, like the sounds of the unaffected side in a case of pleuritic effusion. On the diseased side the principal changes are that dry sounds take the place of moist ones, and instead of coarse crepitation tubular sound and dry crackle are audible, and, especially prolonged expiration in the neighbourhood of the old cavity or consolidation. Bronchial breathing and bronchophony are bound to generally diminish after a residence at high altitudes.

Then physical signs point to what I believe to be the most important curative process induced by mountain climates. (1) Hypertrophy, a more complete development of certain portions of healthy lung tissue. (2) Emphysema of other portions, specially of those in the neighbourhood of the consolidations and cavities. These processes are the results partly of rarefaction of the air and the consequent necessity of at first more frequent respiration, and then of deeper ones, and partly of the lung gymnastics which the constant mountain ascents induce. By hypertrophy, I mean simply that change which fuller physiological activity induces in an organ, in this case, in the lungs; and the contrast between the extent to which these organs are brought into play in the sedentary life of most dwellers in cities, and the work to which they are liable during the daily walks and ascents of Davos, is most striking. The characteristic physical signs of this hypertrophy are the combination of hyper-resonance with deep, long inspiration with feeble expiration. These contrast strongly with the other result of high altitude residence—viz., emphysema where hyper-resonance is combined with prolonged expiration and, over certain areas, with deficiency of breathing.

The dilatation of the alveoli in the neighbourhood of the various lesions produces an important localizing effect, and thus tends to prevent the spread of tubercle by secondary infection from a caseous centre or from a secreting cavity with inflamed walls, and the emptying of the vessels and of the capillaries of the region through alveolar dilatation leads to the caseation and cretification of the tubercular masses. Dr. Weber holds that the diminished capacity for holding water in suspension must cause a withdrawal both of heat and moisture from the human body, and especially from the lungs at these altitudes. He considers that this influence has a cooling effect on the smaller bronchi extending into the alveoli. The drying influence may be the cause of the disappearance of moist sounds from the lungs, and may assist in the cretification of caseous masses.

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It is, moreover, to the hypertrophy and emphysema that we must look to explain the thoracic expansion, which otherwise would remain an unsolved problem, and the fact that the respiration rate diminishes immediately on its occurrence, and in proportion to its extent, is a remarkable argument in favour of the development of mere respiratory power. If emphysema alone were present, the patients would be short-winded, but this is not the case, and it is invariable to return to lower levels with greater powers of climbing and endurance than previously enjoyed. This would be inexplicable unless a larger amount of healthy lung were brought into play by more perfect development. This development in the case of delicate young ladies (*vide* Case 7), who seldom properly inflate the upper lobes of their lungs, is most striking. Hardly less so is it in the instance of hard-worked clerks, whose round shoulders tell the story of stooping over desks for many long hours, and whose miserable thorax may be transmitted to children little likely to survive to adult life. To these two classes the local effects of high altitudes are not less important than the general ones.

Before quitting the subject I may state that the result of numerous chest measurements of patients wintering at the low level resorts, of the Riviera, Egypt, and Algiers, and also of patients before and after long sea voyages, has been to show that, though occasionally a slight increase of circumference takes place, due possibly to increase of muscle or fat, there is no increase which is for an instant comparable in amount or in invariability to that produced by a residence at high altitudes.

The effects on the system of the antiseptic qualities of the air of mountain districts are doubtless great, and have the principal share in producing the general effects, in promoting the healing of the ulcerating cavities, and in reducing the amount of matter formed in the system; but these qualities are not confined to Alpine or mountain climates, but are shared by marine climates and others, such as cold climes.

DISCUSSION.

Dr. Hermann Weber, London: Dr. Theodore Williams has pointed out in a striking way one of the different effects produced by a prolonged stay in high elevations. I could corroborate this experience as occurring in a certain number of cases, though not in all. I have in addition observed a greater expansibility of the thorax as one of the results of high level treatment—viz., an increase in the difference of the circumferences in deep inspirations and complete expirations, the increase being in some cases as much as an inch and a half, in others only an inch or less, while in some cases with less favourable progress it had been altogether absent. I have, however, seen similar results, though less marked, in cases treated at lower elevations. As to the question, to what influences the favourable results obtained at high elevations are to be attributed, I think that climbing had very little to do with them; the three principal factors were, relative absence of impurity, dryness, and rarefaction of the air. I may allude to the fact that meat hung up in the air on the Surrey hills soon becomes putrid, while at Davos, especially during winter, it becomes dry, and remains perfectly sweet for many months. I may point also to Pasteur's researches that air bottled at the *Mer de Glace* near Chamounix did not cause fermentation, while that bottled at Chamounix itself did. I do not mean to say that high elevations are the only localities suitable to the treatment of consumption, that other localities where the air is tolerably aseptic are likewise adapted in many cases, while some cases, especially with a strong hereditary element, cannot be saved anywhere. Careful medical supervision with regard to the quality and frequency of meals, to the purity of air in the rooms, and to exercise and open-air life, is more indispensable in the management of consumption than in that of any other

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chronic disease. By such means favourable results can be obtained even under less favourable climatic influences.

Dr. WILSON FOX, London: The arrest or cure of phthisis by a residence in high altitudes must be accepted as a fact. There are other climates which confer an immunity from the disease, but time does not permit to enter into this large question. As regards the mode in which high elevations prove beneficial, it may possibly be illustrated by the causation of phthisis. Apart from the cases of hereditary origin, Bert's observation has shown two chief classes of causes. For the recognition of one, the profession is chiefly indebted to Dr. MacCormac, of Belfast—viz., the effect of impure air; the knowledge of the other—viz., dampness of houses and subsoil—is due to Dr. Bowditch and Dr. Buchanan; a fourth causation is beginning to be recognized—viz., a septic agency, but this is possibly included in the other two. It is evident that high altitudes operate in the reverse direction to all these three latter causes; and the effect is the same whatever the temperature in suitable cases—viz., the cold climates of Switzerland and Colorado, the hot climate of Bloemfontein, and the comparative temperate climates of the Andes, which latter appear to furnish some of the most favourable results. The point to which I wish to direct most attention is the influence of these in promoting essentially a cure, and the time necessary for this object. By a cure I mean the removal of the tendency to phthisis, and I think that this must be admitted to be among the most essential conditions; for the question which lies behind all cases of phthisis is why the particular subjects of it are attacked. I believe that a predisposition must exist in some cases, as, for instance, when a number of workers in a factory or other trade are all equally exposed to impure air, why do some only become phthisical? A cure involves for most people the possibility of return to ordinary life. To some this appears impossible, they must remain in, or return repeatedly to a health resort; but this for pecuniary or other reasons is impracticable for the majority. In some cases however no relapse occurs; and these might be taken to be simple cases in which inflammatory attacks of a phthisical character have been brought on by unhealthy influences; in others a prolonged residence is necessary, and the duration of this is a matter of some importance, particularly in the elevated climates of Europe. If high altitude is the beneficial agent, it cannot be so good for patients only to spend the six winter months in such a situation as it would be if they were to remain there uninterruptedly for a longer time, and not to descend to lower levels in the spring; and I wish particularly to elicit the opinions of physicians practising in these districts on this question. It bears also on the valuable researches which Dr. Williams has submitted to the Congress on the enlargement of the chest; I doubt whether the cure or benefit derived can be in a very great degree attributed to this alone, for experience since the time of Bayle has shown that people with well-formed chests, even with emphysematous lungs, can become the subjects of phthisis and tubercle. Enlargement of the chest may play a subsidiary part, but even for this object a prolonged residence must necessarily be more favourable than a shorter one.

Dr. ALAN HERBERT, Paris: I have listened with great interest to the discussion, and have only to regret that the different speakers have not laid down the cases in which this is particularly suitable. I wish to call attention to a particular condition, which seems to me to be a reason for rejecting the choice of Davos or high altitudes—that is,

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any alteration of the circulatory system, either of the heart, or especially the arteries and kidneys. A short time ago I was called to visit a member of our profession who had just returned from Davos, where he had been for the health of a member of his family—a man of over sixty years of age, but active and hitherto enjoying excellent health. He suffered for the first time at Davos, when making some mountain ascension, with shortness of breath. As soon, however, as he descended from Davos to the plains, this breathlessness greatly increased, and the slightest movement produced dyspnœa. When he came to Paris I found a slight pleurisy, degeneration of the arteries, some polyuria and a small quantity of albumen in the urine. I called Professor Peter in consultation, and he agreed with me as to the danger of our patient's condition. It was determined to try the use of the compressed air as given at Dr. Fontaine's establishment. When first placed on the compressed air, and as long as the compression was going on, he was pretty well; but gradually, as the compression was being diminished, the breathlessness increased; and when finally removed from the apparatus, the dyspnœa was so intense that Dr. Fontaine was in apprehension of the results, and wrote to say he could not continue the treatment on his own responsibility. As we had foreseen, the symptoms rapidly increased in this case, and in about three weeks or a month the poor man died. This case brought strongly to my mind the necessity there is to examine carefully the heart and circulatory system before sending patients to high altitudes. I have noticed in the discussion that many of the cases which do best at Davos are those of young people; and there may perhaps be some connection between this and the fact that the arterial system is generally in a healthy condition during youth.

Dr. Norman Chevers, London: The principle of expanding the chest has long been recognized as a prophylactic measure in phthisis. We must be very careful in selecting a high site in phthisis. This must not be in a tropical or hot climate. High altitudes try the heart and lungs very severely. With regard to the Himalayas, cases of developed phthisis almost always prove fatal within three months after the patients arrive in India. Persons with healthy lungs, known to have an hereditary tendency to phthisis, generally escape phthisis by taking up an Indian career, but, as Dr. Ewart argues, are peculiarly prone to dysentery.

Dr. Theodore Williams, London, stated in reply that he did not undervalue the prophylactic influence of high altitudes in cases of threatened phthisis, but thought it highly beneficial. He also agreed with Dr. Chevers that the ascent and descent from mountain stations should be managed with great caution for fear of inducing hæmoptysis. His plan, in the case of patients returning from Davos at the snow-melting season was to halt them at Thusis, a lower altitude, for two weeks, at Zurich for one week, and then to allow a gradual return to England. He could not agree with Drs. Weber and Wilson Fox, that cases of hereditary origin were unfit for high altitudes, as out of his twenty-two cases, treated at Davos and elsewhere, eight had hereditary predisposition, and in two it was strongly marked, and yet in all these cases either complete or partial arrest of the disease had taken place. He did not undervalue the general influence on the constitution of the purity of the air, the abundant exercise, and the careful dietary carried out at Davos, and he was quite aware that all these were factors in the mountain cure; but what he would insist on was, that apart from the aseptic influence which Dr. Wilson Fox held to be the common characteristic of all mountain climates,

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but was really shared by other forms of climate—to wit, the ocean and the desert—the diminution of barometric pressure induced certain changes in the thorax and its contents which were not produced to the same degree by any other form of climatic influence, and were very potent helpers in arresting the disease. Some increase in fat and muscle took place, and the elasticity of the thorax was augmented, but these would not account for the enormous increase in the circumference of the chest exhibited by these patients after a residence at high altitudes. As regards the length of residence required for a cure he would say that it differed in different cases according to the extent and duration of the disease. He had seen it effected in two months, but generally at least six months were requisite, and often a much longer period. He would refer Dr. Herbert to the *Lancet* of 1879 (Aug. to Sept.), which contained his published rules for the selection of cases for high altitudes.

The Climate of the Riviera.

Dr. Arthur Hill Hassall, London and San Remo.

The first chapter of the recent and very excellent work by the late Dr. Sparks on the Riviera* is devoted to a description of the meteorology of that famous district. On page 2 of that chapter are the following remarks:—

"According to Teysseire, the mean temperature for the winter of spots exposed to full sunshine is 128·9° Fahr., and of those in the shade on a northern wall 55·9° Fahr.; giving a difference between the two of 73°.

"In spring the numbers are 146·1° in the sun, and 67·6° in the shade; difference, 78·5°.

"In summer we have 155·1° in the sun, and 82·9° in the shade; difference, 72°.

"In the autumn the temperatures are 139·8° in the sun, and 64·4° in the shade; difference, 75·20°.

"The mean difference for the whole year is thus 75·2°."

Referring to M. Teysseire's own figures, which are given in accordance with the Centigrade scale, we find that when converted into that of Fahrenheit, they differ slightly from the above, and stand thus:—Difference in winter, 74·5°; in spring, 78·5°; in summer, 74°; and in autumn, 75·2°; the mean difference being 75·5°.

I was much surprised at these results, and seeing the importance of the subject, I resolved, on the first convenient occasion, to put the figures given above to the test; and I have therefore made some special observations, with a view to the determination of the range of difference between the sun and north shade temperatures. If the figures quoted are correct, and there be really anything approaching a mean annual difference of 75·2° Fahr. of spots exposed to the full sunshine and the shade of a northern wall, then the climate of the Riviera must be most trying, not to say, in many cases, highly dangerous.

Dr. Sparks does not state in what position the thermometer was placed, or with what kind of instrument the temperature of spots exposed to full sunshine was taken, an omission of some importance, as will presently appear. M. Teysseire himself thus

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* It may here be mentioned that my book, "San Remo and the Western Riviera," was published several weeks prior to that of the late Dr. Sparks.

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describes, in his "Vingt-ans d'Etudes Météorologiques à Nice," the circumstances under which the figures were arrived at:—"On est frappé dans ce pays plus encore en hiver qu'en été de la forte chaleur que l'on ressent lorsqu'on est exposé aux rayons directs du soleil . . . . pour donner une idée précise de l'enorme difference de température que l'on éprouve en passant du soleil à l'ombre, et réciproquement, je vais rapporter ici les observations comparatives que j'ai faites pendant un an (1868). J'ai placé, plusieurs fois dans chaque mois, de midi à deux heures sur le bord de ma fenêtre et en plein soleil, un thermomètre à mercure, dont j'avais préalablement enveloppé la boule d'un tissu de laine noire, afin d'éliminer l'effet du rayonnement et d'empêcher la dispersion du calorique; ce thermomètre se trouvait donc dans les mêmes conditions qu'une personne vêtue de drap noire; j'ai noté avec soin les indications de cet instrument et celles que me donnait, au même instant, mon thermomètre place au nord et à l'ombre." It will be observed that M. Teysseire states he placed his thermometer several times in each month in his window in full sunshine; and, in a note addressed to myself, he further remarks that the instrument was so placed only on days that were entirely free from cloud. Hence it appears that his mean temperatures are not founded on a continuous series of daily observations, as they should have been, in order to arrive at a true season and annual mean. M. Teysseire's figures, therefore, accepting them for the moment as correct, indicate the extreme differences between sun and north shade temperatures rather than the mean differences.

With a view to test the temperatures given by M. Teysseire, I prepared a thermometer in the manner described by him, enveloping the bulb with a piece of black cloth of sufficient substance for a moderately heavy great coat, and I placed it in a horizontal position over grass some inches from the ground, and away from houses or other objects which could be sources of reflected heat. I placed near to the clothed thermometer three other thermometers for comparative observations; an ordinary maximum thermometer, a black bulb solar radiation, and a vacuum solar radiation thermometer. Observations were taken on fifty-nine different days, mostly consecutive, extending over the months of February, March, and April, and I will now proceed to summarize the results.

The observations with the maximum thermometer gave a mean temperature of 72·9° on thirty days. Those with the ordinary solar radiation thermometer, a mean of 76·2° on fifty days, and those with the thermometer clothed in cloth for the same days, a mean of 82·6°, or 6·4° higher than the mean of the readings of the ordinary solar radiation thermometer. The results of the readings of the vacuum solar radiation thermometer will be stated later on.

Now, the mean maximum north shade temperature for the same dates and period was 60·5°, showing a difference of only 22·1° between the sun heat as recorded by the thermometer clothed with cloth and the maximum north shade temperature, in place of a mean annual difference of 75·5° as given by M. Teysseire. But the figures may be put in a still more striking form. In place of taking the mean of all the observations, I will give that of the six days in each of the three months, which showed the highest readings with the clothed thermometer—namely, 85·7°, and the north shade temperature for the same eighteen days, was 62·0°, giving a difference of only 23·7°.

The greatest difference, which occurred in February, was 38·2° on the 11th; in March, 23·0°, on the 27th; and in April, 35·6°, on the 29th.

If, in place of taking the mean maximum north shade temperature, 60·5°, we take

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the minimum north shade, 48·2°, which of course occurs almost invariably at night, and therefore is not a temperature to which the human body is subjected under ordinary circumstances, the mean difference between the readings of the minimum thermometer and the sun heat, as shown by the thermometer clothed with cloth, will even then amount to only 34·4°. But M. Teysseire made no such comparison as this, his north shade readings corresponding exactly as to date and time with those of his sun thermometer.

Thinking it possible that by clothing the thermometer still more I might obtain higher readings, approximating even to those of M. Teysseire, I placed around the bulb three coverings, the first of flannel, the second of linen, and the third of cloth, in imitation of the clothing of most individuals when out of doors. This thermometer was exposed to the sun for three consecutive days in May and the same number in June. The mean temperature with this clothed thermometer was 102·3°, while the mean maximum north shade temperature for the same days was 76·3°, showing a difference of 26·0° only, in place of 75·5° as given by M. Teysseire. The mean of the readings of the ordinary solar radiation thermometer for the same days was 91·3°, or 11·0° less than that of the clothed thermometer. That the thermometer prepared as described should indicate a higher temperature than even the solar radiation thermometer is only what might have been anticipated, seeing that the bulb of the former is protected to a considerable extent from the action of current of air and moisture, but still, as will be seen, the readings fall very far below those by M. Teysseire.

The indications furnished by the clothed thermometer, especially that with the triple covering in imitation of the ordinary clothing of man, are really of considerable practical interest and importance, as they demonstrate the actual temperature to which the human body when thus clothed is subjected. It is not a difficult matter to prepare thermometers in the manner described; but they may be obtained by order of Messrs. Negretti and Zambra, who will take care that the bulbs are always clothed with materials of the same texture and quality, a point of importance where it is desired to compare the readings of one thermometer with another.

Had M. Teysseire's readings been obtained with the vacuum solar radiation thermometer, the results would not have been surprising, and they would have stood somewhat thus:—The mean sun heat as recorded by me at San Remo by means of the vacuum solar radiation thermometer for five months of the winter season of 1879–80, was 133·08°; and the mean maximum north shade temperature for the same months was 57·3°, showing a difference between the two of 75·78°; again, the mean sun heat for four months for the following season, 1880–81, was 121·6°; and the mean maximum north shade temperature for the same period was 62·5°, giving a difference of 59·1°; the mean difference of both series of observations being 67·4°. It thus appears that even the readings of the vacuum solar thermometer do not reach by some degrees those of M. Teysseire; but if they did, the indications of the clothed thermometer are not comparable with those of the vacuum solar thermometer. M. Teysseire expressly states that his thermometer resembled a person dressed in black, implying thereby that a person thus clothed would be subjected under like circumstances to the temperature indicated by that instrument; but in the case of the vacuum solar thermometer the circumstances are totally different; the bulb is *in vacuo*, and of course protected from the influence of the moisture and currents of air; its readings therefore

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are necessarily very much higher, and they do not at all indicate the temperature to which the human body is exposed under natural conditions.

The sun doubtless possesses very great power in the Riviera, and this is one of the advantages of the climate; there is certainly considerable difference between sun and shade, but even in England and other colder climates there is much difference, although to a less extent, between the temperature in the shade and the sun. Apart from the increase of temperature occasioned by the power of the sun, the temperature of the climate of the Riviera is a very equal one, as is proved by the following figures. In the six months season of 1879–80, the greatest range of temperature which occurred, and this once only, was 14°, and the mean for the whole season was only 6·3°. The figures for the winter season of 1880–81 were almost identical, the mean being 5·7°.*

Date.	Solar Radiation Thermometer.	Thermometer with Cloth.	Vacuum Solar Thermometer.	Maximum North Shade Temperature.	Date.	Solar Radiation Thermometer.	Thermometers with Cloth.	Vacuum Solar Thermometer.	Maximum North Shade Temperature.
Feb. 4	61·5	66·0	87·4	56·7	Mar. 29	60·2	65·0*	118·6	61·6*
„ 5	72·4	89·5	123·7	56·4	„ 31	56·6	67·8*	119·0	63·6*
„ 6	70·4	73·3	125·8	53·7	April 2	63·7	66·8*	129·2	62·9*
„ 7	72·3	83·0	124·0	56·7	„ 3	76·4	81·0	130·2	66·0
„ 8	74·0	89·1	125·7	55·5	„ 4	65·6	79·7*	135·8	64·7*
„ 9	80·3	95·9	128·8	60·3	„ 5	73·4	78·2*	120·2	60·0*
„ 10	70·0	78·4	111·8	56·6	„ 7	78·4	84·6	126·7	64·6
„ 11	93·6	100·0	—	61·8	„ 8	69·8	79·0	127·5	64·6
„ 12	73·4	83·0	—	56·1	„ 9	82·4	90·2	130·2	67·5
„ 13	70·3	81·9	—	53·5	„ 10	69·9	70·0*	132·4	65·7*
„ 14	70·4	77·0	—	57·1	„ 11	74·6	79·0*	136·0	66·6*
„ 15	55·6	71·0	—	51·1	„ 12	80·0	73·0*	137·1	68·8*
„ 16	65·0	67·0	—	56·1	„ 14	59·7	61·2*	133·2	61·7*
„ 18	79 0	88·1	—	58·4	„ 15	68·0	83·6	134·0	62·6
„ 19	75·2	83·2	—	56·0	„ 16	62·9	68·0	131·9	62·2
„ 20	62·0	63·2	—	59·0	„ 17	68·7	87·2	126·3	65·0
„ 21	68·0	73·8	—	60·5	„ 18	76·1	92·3	132·0	73·1
„ 22	66·0	74·2	—	57·8	„ 20	78·2	80·5	126·2	62·4
„ 23	75·0	85·8	128·6	61·3	„ 21	77·3	94·8	143·7	69·2
„ 24	68·0	78·9	123·7	55·8	„ 22	62·6	75·0	125·1	61·5
„ 25	75·6	81·0	127·0	55·4	„ 23	76·5	97·0	130·8	63·8
„ 26	64·7	66·7	89·0	57·1	„ 24	75·0	94·4	129·0	66·0
Mar. 1	77·5	82·0	139·7	59·0	„ 25	76·8	91·9	129·0	65·4
„ 2	68·4	75·3	133·7	54·8	„ 26	78·2	95·6	125·4	64·4
„ 3	70·1	76·2	130·5	58·3	„ 27	68·9	79·7	140·1	68·2
„ 5	72·9	76·0	133·0	59·0	„ 28	73·4	94·0	136·4	63·5
„ 6	70·8	71·0	108·3	56·7	„ 29	82·4	100·0	124·7	64·4
„ 11	76·9	82·3	134·8	59·6	„ 30	79·3	83.9	123·5	63·8
„ 26	—	67·8	126·8	60·4					
„ 27	77·9	87·0	129·8	64·6	Mean	76·2	82·6	—	60·5
„ 28	73·4	82·0	124·0	63·5					

The observations the results of which I have now recorded, although they do not extend over the whole year, are yet abundantly sufficient to show that the figures given by M. Teysseire cannot be accepted as correct, and that the range of difference of temperature between sun heat and north shade is less than one-third of the range

* "On the Winter Climate of San Remo," by the Author, presented to the Section of Medicine of the British Medical Association, August, 1880.

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given by that gentleman. The great difference between my observations and those of M. Teysseire appear to be explained by the position in which his thermometer was placed—namely, as he says, "sur le bord de ma fenêtre," where doubtless it was much affected by radiation. It should be stated, however, that M. Teysseire is a very careful observer, and that he has taken a very extensive series of meteorological observations at Nice for a period of over thirty years. Although my observations were taken at San Remo and his at Nice, yet this really makes no practical difference in the general accuracy of the conclusion at which I have arrived.

In the foregoing Table are set forth the daily readings upon which some of the data above given are based.

It will be observed that the readings with the clothed thermometer distinguished by an asterisk are all less than those of the ordinary solar radiation thermometer; this has arisen chiefly from the cloth having become damp. These days are not included in any of the means given. On some other days the readings were also less, owing mainly to the action of the wind.

May	28	75·0	...	76·8	...	95·7	...	68·7
„	29	95·0	...	108·5	...	151·0	...	74·1
„	31	93·4	...	102·8	...	143·5	...	76·3
June	1	92·0	...	104·2	...	139·5	...	76·8
„	2	96·5	...	109·4	...	139·5	...	80·6
„	3	96·3	...	112·0	...	143·2	...	81·5
Mean 6 days		91·3	...	102·3	...	135·4	...	76·3

Considering the importance of the health resorts of the Riviera, and the annually increasing number of invalids and others who frequent them, it was very necessary that an error of such magnitude as the one indicated in this communication, and to which such prominence has been given, should be challenged and rectified.

De l'Influence des Chemins de Fer sur le Système Nerveux des Voyageurs.

Docteur E. Tassi, Rome.

Dès l'an 1869 je présentais au Congres médical international de Florence une série d'observations, en forme statistique, qui démontraient l'influence des chemins de fer sur la santé de l'homme.

Les études faites et les observations recueillies dans un espace de plusieurs années démontrent que, quoique d'une part ce système de locomotion apporte une grande diminution d'accidents et de mortalités, d'autre il suscite quelques maladies qui se vérifient surtout parmi ceux qui parcourent les chemins de fer.

Et quoique aujourd'hui les administrations fassent tous leurs efforts pour garantir la santé des voyageurs, avec toute sorte de progrès mécaniques et hygiéniques, soit en améliorant les voies et les moyens de locomotion, soit en établissant des préceptes et des cures hygiéniques pour les employés et les voyageurs, malgré tout, elles ne peuvent éviter certains inconvénients qui dépendent souvent de la manière de voyager, et de certains offices à l'exploitation des chemins de fer.

Ces mémoires, Messieurs, ne renferment pas les désastres et les autres accidents, dont j'ai décrit autrefois les tristes conséquences. Je n'énumérerai pas non plus les changements climatologiques et sanitaires dont les chemins de fer furent la cause, soit

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par la destruction des bois, soit par le déplacement des habitants, soit enfin par le peu de nivellation des cours d'eau. J'ai déjà fait connaître toutes ces causes : seulement je crois devoir résumer en peu de mots, à ce noble Congrès, les conséquences nuisibles, que j'ai pu remarquer pendant 25 ans sur les personnes qui parcourent souvent les chemins de fer en question.

Les effets de longs et rapides voyages se manifestent surtout sur le système nerveux et sur celui de locomotion. Je n'ai aucune intention de suivre les polémiques et les contradictions nées entre les hygiénistes et les publicistes sur la vérité et l'importance de ces faits.

Sans douter des justes observations de ceux qui croient, que ces conséquences peuvent aussi résulter d'autres moyens de locomotion, néanmoins il est à remarquer que ces effets se manifestent plus sensiblement sur ceux qui se servent des chemins de fer sur lesquels se vérifient des maladies spéciales. En effet les observations réitérées démontrent clairement, que les personnes qui font souvent de longs voyages sont affectées plus facilement d'incommodités et même de maladies, que ceux qui emploient une autre manière de voyager. Et, à l'appui de ce que j'avance, on doit remarquer, que même ceux qui, pour leur plaisir et avec tous conforts imaginables, parcourent des grandes distances n'en sont pas toujours épargnés.

Les vieux, les nerveux, les hystériques, les jeunes épouses et les cardiopathes nous donnent beaucoup d'exemples, soit avec les névropaties, soit avec les avortements ou avec les accouchements précoces et avec les métrorrhagies, soit enfin avec les syncopes et les apoplexies.

J'ai dit, Messieurs, de ne pas parler des altérations plus communes, qui ont lieu aussi fréquemment dans la classe des employés que dans celle des voyageurs, parceque j'ai déjà exposé une longue série de cas dans une autre publication. Ce qui doit surtout attirer notre attention, ce sont les lésions du système nerveux trés importantes pour la variété de leurs effets et parcequ'elles ne vinrent à notre connaissance qu'après de longues années d'expérience. Deux espèces d'altérations nerveuses se montrent surtout parmi les employés du train. La première comprend les névroses périphériques, qui ordinairement sont rhumatiques, la seconde les névroses centrales. Les névroses comprises dans la première espèce se montrent à l'instant, ou peu de temps après : tandis que celles de la seconde se préparent lentement et ont lieu progressivement. Dans le premier groupe on classifie les névroses spastiques et les paralytiques, presque toutes périphériques.

Les plus fréquentes sont celles de la face, et celles de la région cervicale, en suite on remarque celle du plexe brachiale, du sacro-lombaire, et du sciatique.

Les névroses du sous-orbitaire, de la branche sous-maxillaire, du quintième, celle du plexe cervical, du circonflexe huméral, et de quelques intercostals sont presque toujours spasthuiques et quelquefois intermittentes. Au contraire sont paralytiques ou presque paralytiques les névroses du facial et du moteur commun de l'œil et de l'élévateur de la paupière. Quelquefois il y avait des parésies ou anesthésies du nerf sacro-lombaire et même du sciatique. Cela arrivait ordinairement aux personnes qui restaient longtemps courbées ou assises, et auxquelles le mouvement secoussoir des trains apporta une secousse et une congestion dans l'origine de ces branches nerveuses.

Toutefois on ne pourrait pas les compter comme paralysies périphériques, quoique produites par l'action réflexe lente et progressive, comme nous en avons déjà parlé, et comme nous le démontrerons plus tard.

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Le Professeur de l'électrothérapie de Rome, le docteur Brunelli, m'a rapporté quelques cas de ce genre de névrose.

Je rappellerai encore les otalgies et les névroses du nerf recourant avec aphonie soudaine. Nous avons observé les pélioses rhumatiques par l'action névro-paralytique vasomotoria : ces névralgies sont moins fréquentes que les autres.

Le nerf optique avec son expansion a été plusieurs fois attaqué par l'élément rhumatique et quelquefois à l'imprévu : outre les effets instantanés, comme asthénopie, hyperesthésie, photophobie, etc., etc., nous avons des lésions profondes et permanentes, dont nous parlerons ensuite.

On établit par de nombreuses observations statistiques, que de ces deux formes névropathiques, la plus fréquente est la spastique, tandis que la paralytique est plus difficile à guérir.

J'ai observé dans un très-grand nombre de cas, que les paralysies périphériques rhumatiques ont été guéries dans le période de trois mois : et celles qui provenaient de la névrites restaient stationaires pendant plus longtemps. Dans les névroses incurables il s'est présenté cinq cas, ou la mort s'est fatalement produite après deux ans, et dont la nécroscopie n'eut pas lieu à cause de la contrariété des parents. C'était des jeunes gens robustes, qui furent pris soudainement de la paralysie de Bell, dans le facial et dans l'oculomoteur commun. Les soins les plus assidus et les plus appropriés ne leur furent d'aucun améliorement ; au contraire, la paralysie devenait chaque jour plus progressive avec symptômes de lésion bulbaire. La persistence des symptômes et la diffusion de la maladie ne purent être attribués à d'autres causes qu'à une véritable névrite lente et progressive, comme il arrive ordinairement aux nerfs sensitifs. En effet, outre l'inefficacité des moyens ordinaires thérapeutiques, le défaut de la réaction electro-musculaire, faisaient connaître, qu'il s'agissait d'une véritable névrite avec altération profonde et étendue dans quelque branche nerveuse : c'est de là que résulte la difficulté de la guérison. Je pourrais citer ici des exemples nombreux dans lesquels plusieurs moyens curatifs prodigués pendant quelques mois, produisirent une guérison incomplète.

Un nombre des névroses chroniques appartient au second groupe : nous parlerons seulement des plus remarquables. L'organe qui jusqu'ici a été constaté le plus prédisposé à être attaqué est l'œil. La particularité, la fréquence de ces altérations sont si importantes, qu'elles méritent toute notre attention.

Les plus exactes recherches m'ont depuis longtemps démontré, qu'un de cas employés souffrait d'une diminution du pouvoir de la vue et de son orientation.

L'ophthalmoscopie fait voir la pâleur rétinique ; l'artère centrale plus mince ; la papille quelquefois petite, quelquefois atrophiée ; le pouvoir de la réfraction altéré ; la sensation chromatique diminuée et souvent irrégulière. L'astigmatisme et le daltonisme aussi compliquaient quelques unes de ces altérations.

A propos du daltonisme il faut constater que, dans le pays du nord, on le trouve dans les employés des chemins de fer beaucoup plus fréquent qu'en Italie.

Pourtant il faut observer, que chez nous le véritable daltonisme a lieu bien rarement, et il faut le rechercher dans les altérations endoculaires avec névro-rétinitis ; ou bien comme conséquence de lésion cérébrale. Le daltonisme par condition pathologique de la rétine, dans les cas rares que nous avons observés, manquait de la perception du rouge et du vert.

Après ces observations vérifiées par des savants oculistes sur le personnel employé

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sur le service du mouvement, de la traction et du matérial dans les chemins de fer italiens, on a établi que les mécaniciens, les chauffeurs et ceux qui sont au service des freins, sont plus prédisposés à ces lésions que l'autre personnel actif. Nous observerons ici que ces employés sont acceptés après avoir subi une visite sanitaire faite par des médecins appartenant à la société des chemins de fer. Et quoiqu'ils soient des jeunes gens forts et robustes, il y en a, cependant, plusieurs qui, après quelques années, commencent à souffrir des altérations dont nous avons parlé jusqu'à présent; de sorte qu'après quinze ou vingt ans de service ils doivent être reformés, et cela arrive ordinairement aux mécaniciens et aux chauffeurs: tandis que les autres employés du mouvement actif souffrent, comme nous avons dit, des névroses périphériques et quelquefois de l'hypéresthésie de la rétine, et du défaut de la refraction statique, dû à la paralysie de l'accommodateur, d'où provient l'hypermétropie.

On doit attribuer tous ces accidents à la pression endoculaire, à laquelle sont exposés les mécaniciens, les chauffeurs, et ceux qui sont au service actif dans les trains à grande vitesse. En effet, la rapidité et la variation atmosphérique, et surtout la haute température et les évaporisations du gaz de la machine sont nuisibles à tout l'appareil oculaire, autant de la sensation que de la réfraction. Et si l'on ajoute l'abus du tabac et de boissons alcooliques, il est facile d'expliquer comme tous ces inconvénients sont dévenus plus fréquents.

Il n'est pas facile de décrire la grande importance pour ces employés d'avoir l'organe de la vue en condition parfaite, et cela a obligé les administrations des chemins de fer à prendre des mesures préventives très importantes pour garantir les employés et les voyageurs par des moyens hygiéniques.

Je ne parlerai pas des autres névroses qui attaquent encore les personnes qui voyagent fréquemment sur les chemins de fer; vous les connaissez déjà, Messieurs; permettez moi toutefois, que, pour raison de connection et de valeur historique, j'en cite quelques-unes des plus évidentes.

Ma longue expérience m'a persuadé qu'outre les névroses périphériques, dont j'ai déjà parlé, il y en a des centrales, qui se manifestent avec des dérangements sur les centres nerveux, tantôt du cerveau, tantôt de la moëlle, et avec une spéciale fatigue névro-musculaire et abattement vraiement général et caractéristique. L'insomnie; l'étourdissement; la diminution de la mémoire et de la faculté d'association; l'hyperesthésie et l'agitation nerveuse qui arrivent souvent aux personnes délicates et sensibles, qui ne sont pas habituées à ces sortes de voyages; résultent des causes dont nous avons parlé plus d'une fois.

On a voulu attribuer à une particulière irritation nerveuse périphérique l'avortement, la métrorrhagie, l'accouchement précoce qu'on retrouve ordinairement dans les personnes délicates et dans les jeunes épouses qui entreprennent imprudemment des voyages circulaires et parcourent, en peu de temps, plusieurs centaines de kilomètres de chemin.

Les ataxiques, les épileptiques et les enfants faibles, lorsqu'ils parcourent un trop long chemin, le mouvement secoussoir des trains leur apporte des tristes conséquences au système nerveux central, en faisant souffrir les uns de paraplègie, les autres d'épilepsie, les derniers d'accès d'éclampsie.

La dysurie, l'ischurie et la stypsis opiniâtre attaquent souvent les vieux et les nerveux; et on les fait dériver d'une condition névro-paralytique par l'irritation nerveuse centrale.

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Ces névropaties, quoique provenant des différentes causes, se verifient plus facilement quand le mouvement secoussoir est plus sensible et plus prolongé, surtout en ceux qui, par de longues courses, souffrent d'incommodités, de la trépidation, et de veillées.

En outre ce n'est pas inutile pour les hygiénistes d'observer les dérangements gastro-entériques, les gastralgies, les palpitations, les hémicraines, les cephalées prolongées, et enfin l'artralgie, auxquels sont souvent exposés ces voyageurs par de longues courses à temps déterminé avec des billets circulaires : et si l'on y ajoute la manière de se nourrir, à laquelle ils ne sont pas habitués, la fatigue, les incommodités auxquelles on ne peut rémedier à temps, les changements de climat et d'habitudes, on se persuadera combien il est facile de tomber malade. Les médecins qui pratiquent les hôtels connaissent bien la fréquence de ces accidents.

Voilà, Messieurs, en peu de mots ce que j'ai pu recueillir, après une periode de 25 années, sur les affections du système nerveux dans les personnes qui exécutent des longs et nombreux voyages sur les chemins de fer.

Dans cette courte rélation il m'a été très difficile de compiler une exacte statistique, parcequ'on n'a pas pu recueillir des faits dans tous les pays ; j'ai pourtant l'espoir, très honorés Confréres, qu'en suivant le chemin que j'indique, vous rendrez de très grands services à la santé publique, surtout, si vous indiquerez les moyens pour garantir ces inconvénients.

Ce qui est indispensable pour un hygiéniste c'est de conseiller les personnes nerveuses, délicates, indisposées, les femmes enceintes, et particulièrement les malades, de voyager à petites journées avec un repos périodique et régulier. A propos des malades, il faut bien considérer ceux qui souffrent du cœur et du cerveau et les empêcher d'exécuter de longs voyages totalement contraires à leur santé. Nous devons appeler l'attention des gouvernements sur les transports des personnes affectées de maladies contagieuses, afin de leur défendre tout voyage. Car on a vu plus d'une fois transporter les maladies de pays en pays et même les communiquer aux voyageurs. Nous avons eu l'occasion d'observer la petite vérole, le typhus, le choléra, apportés, en peu de temps, d'un pays à l'autre.

Notre devoir, Messieurs, est celui de garantir la santé publique, et c'est à ce but que nous devons recommander aux gouvernements d'accepter nos vœux ardents, que nous ne cesserons jamais de faire.

Néphrite et Arthrite Saturnines; coïncidences de ces affections; parallèle avec la Néphrite et l'Arthrite goutteuses.

Dr. Lancéreaux, Paris.

L'existence d'une néphrite liée à l'intoxication saturnine chronique n'est plus à démontrer, car depuis l'année 1863 où j'ai cherché à en déterminer les caractères elle est généralement admise. Plusieurs auteurs conservent néanmoins des doutes sur cette affection ; quelques uns même refusent absolument d'en reconnaître la nature. A ces derniers je présenterai les tableaux suivants ou se trouvent résumés une partie des faits observés par moi depuis vingt ans.

Ces tableaux contiennent vingt quatre faits dont vingt deux ont été suivis d'autopsie. En dépit de l'insuffisance forcée du résumé, il est facile de reconnaître que ces faits ont une physionomie propre tant au point de vue des symptômes cliniques et de leur marche que des lésions anatomiques. Les symptômes sont de deux ordres et

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se rapportent les uns à l'intoxication saturnine, les autres à l'altération rénale concomitante. Les premiers, qui comptent parmi les manifestations les plus avancées du saturnisme, sont la preuve que l'altération rénale se lie non à l'intoxication aiguë, mais à l'intoxication chronique par le plomb. Ce sont des paralysies des muscles extenseurs des avant-bras, des douleurs articulaires, des accès de goutte, quelquefois, mais rarement, des crises de colique saturnine.

Les seconds, c'est-à-dire, les phénomènes que se rattachent à l'altération rénale sont tout d'abord une polyurie plus ou moins prononcée et surtout nocturne, donnant lieu à des urines plus claires et moins denses qu'à l'etat normal ; une anémie avec amaigrissement progressif et diminution des forces musculaires, un certain degré d'anhélation pendant la marche, la diminution de la vue, etc. ; plus tard enfin l'ensemble de désordres connus sous le nom d'accidents urémiques. Ces accidents ultimes ont pour siège tantôt les voies digestives, et se manifestent par de la diarrhée et des vomissements, tantôt l'encéphale et se traduisent par des convulsions, des accès d'éclampsie, du coma ou encore par une dyspnée excessive. L'hydropisie n'existe que dans un petit nombre de cas et seulement à une période avancée de la maladie. Elle est toujours très prononcée, et consiste le plus souvent en un œdème limité aux paupières et aux malléoles, plus rarement aux deux membres inférieurs.

L'albuminurie est un symptôme très-fréquent mais non constant, du moins à une certaine phase de l'altération rénale. Quelques uns de nos malades n'ont présenté la réaction albumineuse des urines que pendant les derniers jours de leur existence. Quelques autres, morts de tuberculose pulmonaire, n'avaient offert aucune trace d'albumine dans les urines, quoique leurs reins fussent trouvés sérieusement altérés à l'autopsie.

La mort, provenant dans ces conditions, si elle n'est le fait d'une circonstance accidentelle, doit être attribuée à l'urémie. C'est à des phénomènes de dyspnée, d'éclampsie, ou de coma urémique que succombent la plupart des individus atteints de saturnisme chronique, et ce qu'on a décrit autrefois sous le nom d'encéphalopathie saturnine n'est en réalité que de l'éclampsie urémique.

Les lésions anatomiques sont de deux ordres et subordonnées, les unes à l'intoxication plombique, les autres à l'empoisonnement dit urémique.

Les premières sont celles qui ont pour siège les muscles extenseurs des avant-bras et les cordons nerveux qui s'y rendent ; les reins, le systéme artériel, et les articulations.

Les secondes qui occupent l'estomac et les intestins ne diffèrent pas de celles qui appartiennent à toute dégénérescence brightique ; aussi nous contentons nous de les mentionner ; ainsi que les lésions musculaires et nerveuses, bien connues depuis le Mémoire que nous avons publié sur ce sujet en 1861 (voyez *Gazette Médicale de Paris*).

Par contre, les altérations des reins, celles du système artériel et des articulations meritent de nous arrêter.

Le volume du rein est variable suivant que la mort a été l'effet de la lésion rénale ou d'une autre maladie. Dans le premier cas, il offre assez ordinairement un volume moitié moindre qu'à l'état normal et pèse de 100 à 150 grammes. Dans le second cas, il est plus volumineux. Les deux reins tout en n'étant pas egalement altérés offrent une difference peu sensible. La surface externe de ces organes, excepté tout a fait au début de l'altération, n'est jamais lisse, mais parsemée de granulations miliaires ou lenticulaires plus ou moins régulièrement disposées. La substance corticale resiste sous le doigt qui le presse. La surface de section est ferme, légèrement irrégulière et parsemée de fines saillies, formées par les parties qui sont le moins altérées. Elle est notable-

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ment diminuée d'épaisseur et parfois un peu décolorée tandis que la substance tubuleuse, lisse et violacée, conserve à peu prés son volume naturel. Histologiquement, cette lésion rénale se révèle à son début par la présence au pourtour ou dans la tunique des vaisseaux, principalement a la périphérie du lobule de jeunes cellules rondes (cellules dites embryonnaires) qui s'allongent peu à peu pour se transformer en un tissu fibrillaire ; d'où l'état granulé du lobule et la diminution du volume du rein. Les artérioles sont épaissies ; le tronc et les principales branches de l'artère rénale présentent peu de modifications. Les tubes contournés sont, les uns diminués de volume et rétrécis : ce sont ceux situés au niveau des points altérés ; les autres sont normaux et legèrement dilatés. Les épithéliums altérés, granuleux ou en voie de dégénérescence colloïde dans les premiers, sont peu ou pas altérés dans les derniers.

En résumé, l'altération rénale est dans tous nos faits, pour ainsi dire, identique, car si elle présente quelques différences, ce n'est pas dans la nature mais seulement dans le degré du mal. Elle consiste essentiellement dans une végétation des éléments conjonctive-lymphatiques qui, en s'organissant d'une façon définitive, produisent la rétraction du substratum fibreux du rein et la diminution progressive du volume de ces organes.

Tels sont les caractères de la néphrite observée dans l'intoxication chronique par le plomb. La constance de ces caractères, l'évolution spéciale de la lésion, la fréquence relativement grande de l'altération des reins en pareil cas, ce sont là autant de circonstances qui démontrent l'existence d'une relation entre cette altération et l'action prolongée du plomb sur l'organisme.

Cette altération du rein coéxiste généralement avec une modification du système artériel, caracterisée par un épaississement qui porte sur la tunique interne oú il détermine des nodosités plus où moins saillantes. Le cœur est ordinairement hypertrophié.

L'aorte et les principales branches, les artères rénales en particulier sont presque toujours un peu dilatées. Dans ces conditions, il y a lieu de se demander si la néphrite saturnine n'est pas un effet de l'altération du système artériel, mais à notre avis, cette hypothèse est peu admissible à cause des différences manifestes existant entre le rein saturnin et le rein artériel. Dans ce dernier, en effet, les granulations sont toujours plus irrégulières que dans le premier, les, artérioles sont beaucoup plus atteintes et l'altération des deux organes est rarement égale.

Une circonstance importante et qui d'ailleurs suffirait à séparer la néphrite saturnine de la néphrite consécutive à l'altération du système artériel, c'est la coexistence habituelle avec chacune de ces lésions d'une arthrite spéciale. L'arthrite qui fait cortège à la néphrite saturnine est sans doute assez commune, car, sur un chiffre de 24 observations, elle existait 1 fois et fut constatée 5 fois à l'autopsie. Une seule fois elle n'a pas été rencontrée et dans le reste des cas les articulations ne furent pas examinées.

Cette arthrite consiste dans l'infiltration des cartilages par des urates de soude et d'ammoniaque.

Les articulations les plus souvent affectés sont celles des métatarsiens et des métacarpiens avec les phalanges, puis celles des poignets, des genoux, et des coudes.

Les surfaces articulaires et principalement celles de l'articulation metatarso-phalangienne du gros orteil qui est le siège de prédilection de cette lésion, se font remarquer par une coloration blanche comme si elles avaient été saupoudrées de farine ou d'une poussière de plâtre qui les aurait pénétrées, incrustées. Les franges synoviales

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et même les tendons sont parfois aussi infiltrés par cette même substance. Une coupe du cartilage perpendiculaire à sa surface, permet de reconnaître que le dépôt uratique a pour siège les couches les plus superficielles et qu'il est formé de cristaux en aiguilles, disposés sous forme de rayons partant d'un point central, le plus souvent une cellule de cartilage, agglomérés en grand nombre et sans ordre apparent. Rarement toutefois le dépôt est assez abondant, pour donner lieu aux nodosités connues sous le nom de tophus.

Bien différente est l'arthrite qui accompagne la néphrite d'origine artérielle. Ici, absence totale d'infiltration uratique, simple usure des cartilages d'encroûtement, mais formation au pourtour des surfaces articulaires de productions osseuses, désignées sous le nom d'ecchondroses. Cette arthrite s'observe de préférence dans les articulations métatarso-phalangiennes du pouce et des genoux; elle présente les caractères de l'arthrite noueuse ou arthrite sêche, tandis que l'arthrite observée chez les saturnins ressemble à celle qui se rencontre chez les goutteux. Ce sont donc là des lésions articulaires distinctes, et, partant, il faut bien admettre que les lésions rénales qu'elles accompagnent le sont également.

Il n'est donc plus possible de nier l'existence d'une néphrite liée à l'intoxication par le plomb et cette néphrite que nous appelons néphrite saturnine est fréquemment accompagnée d'une arthrite qui a les caractères de l'arthrite de la goutte. Il nous reste à comparer ces lésions du saturnisme et de la goutte et à rechercher si elles sont oui ou non identiques.

Le rein goutteux, tel qu'il a été décrit par Todd, Dickinson, et autres auteurs, et tel que je l'ai vu dans plusieurs cas, est ferme, résistant, diminué de volume, semé à sa surface de granulations miliaires ou lenticulaires à peu près égales et parfois de kystes du volume d'un pois. Or, ces caractères sont précisément ceux du rein saturnin, et si nous ajoutons que la néphrite goutteuse coexiste avec des lésions artérielles semblables à celles qu' on observe dans la néphrite saturnine, le rapprochement de ces deux affections est encore plus sensible. Quant à l'arthrite goutteuse, elle est comme l'arthrite saturnine, caracterisée par l'infiltration uratique des cartilages d'encroûtement, des franges synoviales, des tendons et parfois du périoste.

Elle ne diffère donc pas de l'arthrite saturnine, si ce n'est peut-être dans quelques cas par un dépôt uratique plus abondant, mais ce n'est là qu'une question de degré qui ne permet pas de séparer ces deux affections. Conséquemment la néphrite et l'arthrite saturnines nous paraissent être des lésions identiques à la néphrite et à l'arthrite goutteuses. Or, la cause première de ces affections n'étant pas la même, il faut, nécéssairement, que leur mode pathogénique soit semblable; mais comme l'arthrite goutteuse n'est pas le fait de l'élimination du plomb par les reins, il y a lieu de croire que la néphrite saturnine n'est pas d'avantage le résultat de cette élimination et que chacune de ces lésions a son origine dans un trouble primordial de l'innervation nutritive. Il n'est douteux pour personne que la goutte soit le résultat d'un trouble de ce genre, et tout le monde sait que le plomb a une action manifeste sur le système nerveux.

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NÉPHRITE SATURNINE.

No.	Sexe.	Age.	Profession.	Symptômes.	Reins.	Cœur et système artériel.	Articulations.	Autres organes.
1	H.	66	Peintre, depuis 40 ans.	Coliques de plomb en 1850, en 1868. En 1877, étourdissements, douleurs articulaires, dyspnée. 3 litres d'urine albumineuse en 24 h. Souffle au cœur (1er temps). Le 16 Mai, diarrhée, voix rauque. Diminution de l'urine: 2½ lit. En Sept. dysphagie, crachats sanguinolents. Mort le 28 Octobre.	La surface du rein droit est semée de granulations miliaires et de kystes gélatiniformes. L'un d'eux a le volume d'un œuf de pigeon. Sa substance corticale n'a pas plus d'un millim. d'épaisseur. Le rein gauche est petit, exempt de kystes, mais fortement granulé.	Aorte large et dilatée présente des saillies à bords sinueux; les iliaques primitives sont altérées dans toute leur étendue par des plaques mamelonnées. Les valvules sigmoïdes épaissies au niveau des nodules d'Aranzi. Cœur volumineux. La paroi ventriculaire gauche a 3 centim. d'épaisseur; le muscle ferme, rouge, n'est pas altéré.	Non examinées.	Foie hyperémié. Bile brunâtre. Rate atrophiée. Artère splénique variqueuse. Méninges molles, opalines, à la convexité des hémisphères. Artères cérébrales intactes. Liquide ventriculaire abondant. Substance nerveuse à peu près normale. Adhérences du poumon gauche. Emphysème du poumon droit, quelques tubercules au sommet. Œdème des replis aryténo-épiglottiques. Petit foyer purulent au niveau du cartilage cricoïde.
2	H.	41	Ferblantier.	Depuis l'âge de 17 ans, sauf pendant 7 ans, où il fut militaire. Pas de coliques de plomb. Depuis 18 mois, urines abondantes. Depuis 3 semaines, œdème des bourses puis des jambes. En dernier lieu, ascite.	Durs, petits, diminués d'un tiers de leur volume. Surface granulée, substance corticale atrophiee. Poids, l'un 95 gr., l'autre 98.	Insuffisances pulmonaire et tricuspide. Dilatation du ventricule droit. Insuffisance aortique. Hypertrophie du ventricule gauche. L'aorte ne présente pas de lésions manifestes.	Deux accès de goutte au gros orteil gauche.	Emphysème et bronchite. Foie diminué de volume, dur, surface grenue. La rate présente des adhérences avec le péritoine.
3	H.	46	Ferblantier.	Hemiplégie flasque droite survenue brusquement. Albumine dans les urines; pas d'œdème.	Petits, granulés. Artères rigides. La substance corticale a presque complètement disparu.	Ventricule gauche hypertrophié. Les parois de l'aorte sont épaissies surtout la tunique interne.	Absence de lésions articulaires.	Opacité des méninges. Foyer hémorrhagique entre le corps strié et la couche optique de l'hémisphère gauche. Excavation de la papille de l'œil droit, vaisseaux atrophiés.
4	H.	50	Peintre en bâtiments.	Quatre accès de coliques depuis 2 ans. Ascite légère. Urines abondantes. Albumine et œdème à la fin de la maladie.	Reins de volume normal, surface très-décolorée et déjà granuleuse. C'est la néphrite interstitielle au 1er degré.	Cœur décoloré. Insuffisance aortique. Aorte élargie, légèrement stéatosée, artères iliaques larges, épaissies.	Non examinées.	Foie muscade. Rate lobulée ferme, granulations tuberculeuses aux 2 sommets des poumons et dans l'intestin grêle.
5	H.	57	Peintre en bâtiments.	Un peu d'albumine, œdème des membres inférieurs, des bourses et du ventre.	Reins petits. L'un pèse 65 gr., l'autre 55. Surface granulée. Substance corticale amincie.	Hypertrophie du ventricule gauche. Aorte dilatée au niveau de la crosse, plaques blanches. Valvules sigmoïdes un peu épaissies. Artères sylviennes épaissies.	Dépôts uratiques blancs dans les articulations métatarso-phalangiennes.	Hydrocèle et atrophie du testicule du côté droit. Foie petit, Papille de l'œil droit un peu pâle. Cristallin opaque et dur.

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NÉPHRITE SATURNINE—(*Suite*).

No.	Sexe.	Age.	Profession.	Symptômes.	Reins.	Cœur et système artériel.	Articulations.	Autres organes.
6	H.	28	Tailleur de cristaux.	Urines albumineuses, douleurs lombaires à droite, depuis 2 ans. Pas d'œdème. Tousse depuis 2 ans.	Reins aplatis. La capsule se détache facilement. Substance corticale congestionnée, légèrement blanchâtre. A la base des pyramides, on constate dans les tubuli des cristaux d'acide urique.	Cœur. Ventricule gauche normal. Le ventricule droit est un peu dilaté. Un peu de sang liquide dans les cavités du cœur. Les branches de l'artère pulmonaire sont remplies de caillots cruoriques.	Les cartilages de l'articulation tarso-métatarsienne des 2 gros orteils présentent des points blanchâtres constitués par de l'urate de soude.	Foie volumineux, congestionné. Rate de même. Excavations aux 2 sommets des poumons, granulations tuberculeuses noires disséminées.
7	H.	39	Peintre en bâtiments.	Depuis l'âge de 14 ans. Faiblesse de la vue depuis 2 ans. Il y a 3 ou 4 ans il éprouva une douleur dans les genoux qui l'obligea à garder le lit plusieurs jours. Il y a un an vive douleur dans le talon. Durée quelques jours. Albuminurie. Pas d'anasarque	Reins petits. La gauche pèse 85 gr., le droit, 75 gr. Aplatis. La capsule enlevée, ils paraissent comme chagrinés. A la coupe, on voit la substance corticale réduite a 2 mm. d'épaisseur.	Cœur normal. Artères non examinées.	Non examinées. A son entrée à l'hôpital accès de goutte dans le gros orteil droit.	Foie gras. Rate petite. Méninges normales. Cerveau pâle, très ferme. Vaisseaux de la rétine petits, peu nombreux. Ambliopie.
8	H.	43	Peintre en bâtiments.	Paralysie des extenseurs de l'avant-bras. Gonflement douloureux des orteils, des pieds, des doigts. Vomissements, céphalalgie, bouffissure de la face. Constipation. Accès convulsifs des muscles de la face et des membres. Embarras de la parole. Intelligence obtuse, mémoire affaiblie. Hyperesthésie de la peau de la face. Agitation, somnolence. Mort dans le coma. Albuminurie.	Reins atrophiés; diminués de moitié; leur surface est irregulière, la capsule adhérente; épaississement du stroma conjonctif, diminution des glomérules. Epithéliums des tubuli granuleux, quelques uns renferment une substance colloïde.	Cœur chargé de pelotons graisseux et de plaques laiteuses. Ventricule gauche hypertrophié (cœur de bœuf). Léger degré d'insuffisance aortique. Plaques saillantes, jaunâtres sur l'aorte. Carotides modifiées, artères cérébrales athéromateuses; artères rénales dilatées.	Surfaces articulaires des pieds et du poignet infiltrées d'urates de soude.	Extenseurs des bras pâles, jaunâtres, atrophiés. Myéline de nerfs granuleuse. Adhérences pulmonaires, et petit foyer de pneumonie caséeuse devenue calcaire. Foie hyperémié. Hydrocèle et atrophie du testicule gauche.
9	F.	48	Polisseuse de caractères d'imprimerie.	Paralysie incomplète des deux bras. Œdème léger des jambes; Diarrhée; vomissements. Symptômes de phthisie pulmonaire. Albuminurie.	Reins petits, granuleux, légère adhérence de la capsule; substance corticale atrophiée.	Cœur chargé de graisse à la base. Valvules normales. Aorte large.	Non examinées.	Adhérences pulmonaires aux 2 sommets qui sont détruits. Plaques tuberculeuses, noyaux caséeux, hépatisation. Foie gras. Ganglions mésentériques hypertrophiés, caséeux. Ulcérations à l'extrême de l'iléon.

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NÉPHRITE SATURNINE—(*Suite*).

No.	Sexe.	Age.	Profession.	Symptômes.	Reins.	Cœur et système artériel.	Articulations.	Autres organes.
10	H.	55	Peintre en bâtiments.	Colique et arthralgie saturnines. Gonflement douloureux des orteils. Vomissements de sang, amenant une syncope et la mort. Albuminurie ancienne.	Reins diminués de moitié de leur volume, irreguliers, granuleux. Substance corticale atrophiée. Epaississement de la substance conjonctive. Corpuscules de Malpighi diminués. Epitheliums des tubuli altérés ou disparus.	Cœur gauche, hypertrophié; aorte dilatée, athéromateuse.	Infiltration uratique des articulations des orteils.	Léger épanchement dans les ventricules cérébraux. Foie normal; rate volumineuse.
11	H.	31	Peintre en bâtiments.	Coliques saturnines; délire, amaurose. Mort, dans un accès d'éclampsie. Albuminurie.	Reins diminués; substance corticale atrophiée; surface parsemée des grains saillants, blanchâtres. Hyperplasie de la substance conjonctive; quelques corpuscles de Malpighi atrophiés. Cellules épitheliales des tubuli granuleuses.	Cœur hypertrophié. Aorte dilatée, parois épaisses et athéromateuses.	Non examinées.	Adhérences pulmonaires. Foie normal. Rate volumineuse. Quelques tubercules crétacés.
12	H.	31	Estampeur.	Mouvements convulsifs. Résolution des 4 membres et mort. Albuminurie.	Reins petits, atrophie de la substance corticale; petits grains blanchâtres, peu saillants, à leur surface.	Cœur presque normal, renferme du sang liquide. Les artères de la base du crâne sont intactes.	Non examinées.	Petit foyer sanguin dans la protubérance. Poumons très congestionnés, apoplexie infiltré. Tubercules crétacés du foie, surtout à la périphérie.
13	F.	37	Coloriste.	Colique saturnine, arthralgie, accidents épileptiformes, Paralysie des extenseurs de la main et du pied. Bouffissure et atrophie de la face. Œdème des malléoles. Amaurose. Analgésie des extrémités, céphalalgie, étourdissements, vertiges. Paralysie des muscles abdominaux et thoraciques. Albuminurie.	Reins petits, granuleux; substance corticale atrophiée, surtout à droite. Tissu conjonctif épaissi par formation de jeunes éléments. Epitheliums des tubuli en partie detruits.	Myocarde jaunâtre et fibres vasculaires granuleuses. Ventricule gauche hypertrophié. Valvules saines.	Non examinées.	Rate hypertrophiée, granuleuse. Œdème des poumons. Légers ramollissements de la moëlle. Extenseurs de la main et du pied atrophiés, décolorés, reduits à de minces bandelettes. Les nerfs qui s'y rendent sont altérés, leur substance médullaire est granuleuse ou disparue. Granulations noires sur la pie-mère finale (sulfure de plomb?).

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NÉPHRITE SATURNINE—(*Suite*).

No.	Sexe.	Age.	Profession.	Symptômes.	Reins.	Cœur et système artériel.	Articulations.	Autres organes.
14	H.	49	Peintre en bâtiments.	Absence d'hydropisie. Dyspnée. Pendant les derniers jours, coma léger, avec diminution de la motilité et de la sensibilité. Albuminurie.	Les deux organes petits, granuleux : atrophie portant sur la substance corticale, qui est jaunâtre. L'examen microscopique révèle l'existence de tubes atrophiés et profondement altérés, à côté de tubes à peu près sains : la trame conjonctive est notablement épaissie.	Hypertrophie considérable du ventricule gauche et des colonnes charnues. Valvules intactes et suffisantes, à part les aortiques, qui sont un peu épaissies. Aorte dilatée, épaissie et parsemée de nodosités. Même altération des artères rénales et de la plupart des artères cérébrales.	Non examinées.	Liquide céphalorachidien abondant. Petits foyers disséminés de ramollissement cérébral, sans oblitération artérielle. Adhérence des poumons œdématiés a leur base. Foie et rate peu modifiés.
15	H.	64	Peintre en bâtiments.	Œdème passager de la face et des membres inférieurs. Léger degré d'amaurose; faible quantité d'albumine dans les urines. Gonflement douloureux de l'orteil dix mois avant la mort. Diarrhée et vomissements. En dernier lieu, coma profond.	Les deux organes diminués de près des deux tiers, granuleux à leur surface, où existent quelques kystes ; substance corticale atrophiée, jaunâtre, indurée. Tubes urinifères diversement altérés ; epaississement de la trame conjonctive.	Cœur hypertrophié, paroi ventriculaire gauche doublée de volume, cavité dilatée ; deux valvules aortiques adhérent entre elles et détermisent un léger degré d'insuffisance ; les autres valvules saines. Aorte dilatée, épaissie en plusieurs points. Endartérite des vaisseaux qui en émanent et notamment des artères rénales et de leurs branches. Artères cérébrales altérées.	Non examinées.	Amincissement des os du crâne, surtout à la base ; anémie de la substance cérébrale, atrophie avec sclérose des nerfs optiques, adhérence des poumons au niveau de leurs sommets scléroses et infiltrés de quelques points caséeux. Foie petit. Rate normale, gastro-entérite, quelques muscles décolorés.
16	H.	39	Peintre en bâtiments.	Œdème léger et ultime au niveau des malléoles. Vomissements, diarrhée, signes de tuberculose. Bouffissure de la face. Mort dans un état comateux. Pas d'albumine.	Les deux reins altérés et diminués d'un tiers. Capsule adhérente. Surface inégale. Substance corticale jaunâtre, ferme, atrophiée, infiltrée de quelques kystes ; épaississement de la substance conjonctive.	Cœur hypertrophié, jaunâtre ; insuffisance légère des valvules aortiques. L'aorte, large, offre sur plusieurs points les nodosités de l'artérite. Même altération des artères rénales et cérébrales.	Articulation du gros orteil normale.	Cerveau intact ou seulement pâle, adhérences pulmonaires. Infiltration tuberculeuse du poumon droit distribuée principalement sur le trajet des vaisseaux, quelques granulations à gauche. Entérite avec un petit nombre d'ulcérations dans le gros intestin ; dans le rectum, notamment. Foie et rate normaux Décoloration des 2 supinateurs.

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NÉPHRITE SATURNINE (*Suite*).

No.	Sexe.	Age.	Profession.	Symptômes.	Reins.	Cœur et système artériel.	Articulations.	Autres organes.
17	H.	36	Imprimeur cerusier.	Pas d'œdème. Ictère. Paralysie des muscles des avant-bras et du diaphragme. Constipation, étourdissements, delire, convulsions finales. Albuminurie.	Rein droit atrophié, substance corticale indurée, surface granuleuse, capsule adhérente. Rein gauche légèrement augmenté de volume. Dans les deux organes, la substance corticale est jaunâtre; cellules épithéliales, tantôt volumineuses, tantôt disparues, tantôt granuleuses.	Cœur d'un volume normal, chargé de graisse [à droite. Caillot fibrineux dans le cœur droit, se prolongeant dans l'artère pulmonaire. Artères cérébrales saines.	Non examinées.	Foi diminué; altérations granulo-graisseuse d'une partie de ses cellules. Rate normale. Poumons pâles, œdématiés; le sommet du poumon droit contient un tubercule crétace. Muscles extenseurs, jaunâtres, altérés; état granuleux des extenseurs des doigts; les nerfs qui s'y rendent ont leur substance médullaire granuleuse.
18	H.	53	Peintre.	Urines albumineuses. Œdème des jambes. Diarrhée. Goutte.	Reins diminués de moitié à surface granuleuse. La substance corticale a 1 mm. d'épaisseur. Quelques petits kystes dans les pyramides.	Hypertrophie du ventricule gauche. Valvules intactes. Sur le bord supérieur et adhérent d'une des valvules aortiques, il existe un petit bouquet de végétations.	Dans l'art. métatarso-phalangienne du gros orteil gauche, on aperçoit sur la cupule cartilagineuse l'usure du cartilage et un point blanc, indice d'un dépôt uratique. L'art. de l'orteil opposé renferme une masse blanche, infiltrée de points formés d'urates. Id. pour les têtes des 2 premiers métacarpiens.	La membrane muqueuse de l'estomac pigmentée par points, est blafarde sur d'autres. L'intestin grêle, l'iliaque et surtout le rectum sont le siège de cicatrices pigmentées à diamètre longitudinal.
19	H.	54	Peintre en bâtiments.	Depuis l'âge de 12 ans. Coliques de plomb à cet âge. Ascite. Œdème du scrotum et des membres inférieurs. Deux ponctions.	Reins petits, remplis de petites granulations, quelques kystes dans la substance corticale, qui est dure.	Cœur volumineux. Ventricule droit dilaté, orifice pulmonaire légèrement insuffisante. Ventricule gauche hypertrophié. Orifice aortique insuffisant. Valvules épaissies. Orifice mitral insuffisant. Aorte dilatée; altérations graisseuses.	...	Foie petit, capsule épaissie. Estomac dilaté, présente des arborisations d'une coloration ardoisée. Méninges adhérentes au cerveau, épaissies, congestionnées. Les vaisseaux sont le siège d'endartérite sur le sylvienne gauche et la basilaire surtout.
20	H.	42	Peintre en bâtimens	Depuis 30 Aug. Pas d'accès de goutte antérieurs. Coliques saturnines, l'an dernier. Urines albumineuses. Œdème des jambes. Diarrhée, 2½ lit. d'urine. D.=1008. Troubles de la vue. Il voit à peine pour se conduire.	Pas d'autopsie.			Sur la rétine, plaques blanches le long des vaisseaux.

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NÉPHRITE SATURNINE—(*Suite*).

No.	Sexe.	Age.	Profession.	Symptômes.	Reins.	Cœur et système artériel.	Articulations.	Autres organes.
21	H.	47	Broyeur de couleurs.	Coliques depuis 1875, pendant 4 ans. Paralysie des extenseurs des avant-bras en 1879. Liséré peu marqué, anémie. En 1879, vomissements, coma et mort en quelques jours. Albuminurie.	Reins granuleux, petits, irréguliers et parsemés de kystes lenticulaires, pesant de 100 à 120 gr. Les artères épaissies sont bésntes.	Hypertrophie cardiaque, valvules sigmoïdes normales, aorte dilatée, parsemée à la face interne de plaques saillantes blanchâtres.	Non examinées.	Foie et poumons congestionnés. Rate indurée. L'estomac hyperémique. Méninges opalines à la base. Pulpe cérébrale un peu molle. Artères à peine modifiées.
22	H.	49	Peintre en voiture.	Depuis l'âge de 10 ans. Plusieurs coliques de plomb Œdème des jambes. Pas d'albumine, excepté à la fin. Mort dans un état de dyspnée urémique.	Reins diminués de volume. Granuleux à leur surface. Artères rénales épaissies.	Cœur gros, hypertrophié. Valvules saines. Artérite déformante de l'aorte et des artères iliaques et fémorales. Quelques tâches blanchâtres sur les artères de la base encéphalique.	...	Poumons adhérents. Emphysème à un léger degré. Liquide encéphalo-rachidien abondant. Rate petite, indurée.
23	H.	63	Potier émailleur.	Albuminurie et polyurie ancienne. Anémie. Maigreur; paralysie des extenseurs des avant-bras. Accidents maniaques considérés comme liés à l'urémie. Dyspnée et coma.	Reins non examinés.	Cœur hypertrophié. Système artériel affecté.	Goutte saturnine, plusieurs attaques depuis 10 Aug.	Emphysème léger des poumons.
24	H.	69	Peintre en bâtiments.	Coliques de plomb. Polyurie ancienne. Hémiplégie. Mort subite.	Reins granuleux, indurés, diminués de volume et parsemés de kystes.	Dilatation du système artériel et de l'aorte en particulier, qui est semée de petites plaques saillantes.	Non examinées.	Altération du corps calleux. Thrombose veineuse et embolie pulmonaire.

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INAUGURAL ADDRESS,

BY THE PRESIDENT,

JOHN ERIC ERICHSEN, Esq., F.R.S.

GENTLEMEN,—Surgery is never stationary. To be stationary while all around is in movement would be practically to retrograde. But movement does not necessarily mean advance. The general direction of the movement may undoubtedly be forwards, but the factors of that movement do not all equally tend to progress. When the history of Surgery comes to be written—and this has never yet been done—it will be found that the surgery of the nineteenth century has not been uniform in its progress in all departments; that its advance has not been continuously in one line, but that its progress has been materially affected by the prevailing bias of the professional mind of the day. Anatomical at one time, physiological at another, the tendency of the surgery of the present day is influenced in one direction by the mechanical spirit of the age, and in another by the advanced Pathology, which is one of its chief medical characteristics. Yet the continuous advance of our Art is undoubted. The gain that thus results has been definitively secured to Surgery and to mankind. It can never be lost. Every conquest that has been made has been permanent. Year after year some new position has been won, often, it is true, after a hot conflict of opinion; but once occupied it has never been abandoned. Thus our standpoint has ever been pushed on in advance. For knowledge in science is cumulative, and skill in art is a tradition that is hereditarily transmitted from master to pupil, if not by the individual, yet by the profession to which he belongs, from which he has acquired, and to which he bequeathes it, augmented and perfected by his own labours. With the knowledge of our predecessors we are familiar; to its stores each generation has added. What they have done has been transmitted to us, and we can readily accomplish. In what we can do, we may be sure our successors will not fail. It is well that from time to time this advance should be measured, this gain weighed. The business of this Section is not only to measure the extent of the advance, but to determine the value of the gain, and to do this, not so much by the novelty of the practice, or by the brilliancy of its exposition, as by an estimate of its intrinsic merit, as shown by its proved utility. Our business here has to do with practical considerations, having reference to the recent advances in, or the future lines to be followed by, modern Surgery.

The Executive of this Section has proposed eight subjects for the consideration of its members. It is hoped that these will be found to include the more important surgical questions that are at present most prominently before the Profession. The short time at our disposal, which will scarcely enable us to do full justice even to these subjects, has prevented the possibility of our bringing forward other and perhaps equally interesting questions; but some of these will be found to have received consideration in the papers which will be read, either *in extenso* or in abstract, as time may allow.

I will now briefly refer to the more important subjects that have been set down for our consideration.

(1) In no department of Surgery has a more marked or brilliant advance been made of late years than in that of the operative treatment of Intra-Peritoneal Tumours.

The establishment of Ovariotomy as a recognized surgical operation has now long been matter of history; but the perfection of safety to which it has of late years been carried, by the improvement of its details, has led the way to a vast and rapid extension of operative surgery, for the cure or relief of various diseased abdominal organs. The uterus and the spleen, the stomach, the pylorus and the colon, have each and all been subjected to the scalpel of the surgeon; with what success has yet to be determined; and it is for you to decide whether some, at least, of these operations constitute real

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and solid advances in our Art, or whether they are rather to be regarded as bold and skilful experiments on the endurance and reparative power of the human frame—whether in fact they are surgical triumphs or operative audacities. There must, indeed, be a limit to the progress of operative surgery in this direction. Are we at present in a position to define it? There cannot always be new fields for conquest by the knife; there must be portions of the human frame that will ever remain sacred from its intrusion, at least, in the hands of the surgeon. May there not be some reason to fear lest the very perfection to which ovariotomy has been carried may lead to an over-sanguine expectation of the value and the safety of the abdominal section and exploration, when applied to the diagnosis or cure of diseases of other and very dissimilar organs, in which but little of ultimate advantage, and certainly much of immediate peril, may be expected from operative interference?

(2) In the discussion of the next great question, I would submit that we may, with advantage, direct our attention less to the mere mechanical—the simple operative part of the business, the details of which are now well understood, than to the consideration of those higher questions as to the diagnosis and nature of the various forms of renal disease, in which Nephrotomy and Nephrectomy may be respectively used, with a reasonable hope of relief or cure. And in considering the prospects afforded by these operations in the improvement of the health and the mitigation of the sufferings of the patient, it is surely not the least interesting point for us to study the after-physiological effects produced on the system by the extirpation of so important an eliminatory organ as the kidney.

(3) We naturally pass from the consideration of operations on the kidney to those which implicate the bladder; and in doing so we have specially to direct our attention to the question as to what advances have of late been made in Lithotomy and Lithotrity.

In Lithotomy we see much of change, possibly something of novelty, but not so certainly anything of real progress. Have we indeed advanced one single step either in the perfection or in the results of that operation since the days of Cheselden, of Martineau, or of Crosse, not to mention the names of more recent but equally illustrious surgeons and successful operators? The revived median, the combination of it with Lithotrity, the supra-pubic, whether done antiseptically or not, have certainly not been very encouraging in their results, and can scarcely claim to be considered in the light of an advance on the old lateral operation in skilful hands. But yet we must admit that these methods of Lithotomy may deserve this consideration—that possibly in some forms of calculus, and in certain conditions of the urinary organs, a wise eclecticism may be exercised in the choice of one or other of them.

In Lithotrity, however, it is probable that a great and real advance has been made, and certainly it is undoubted that a complete revolution has been effected by the enterprize and skill of one of our American brethren. For it cannot be questioned that "Bigelow's operation" has completely changed the aspect of Lithotrity, and there is every reason to believe that it constitutes one of those real advances in a method which marks an epoch not only in the history of the operation itself, but in the treatment of the disease to which it is applicable.

But here a fertile field opens up for our deliberation. We have to consider not only in what cases, as regards the mere size of calculus, "Bigelow's operation" may safely be used; but also, and far more important than this, the ultimate result both upon the bladder and the kidney of prolonged intra-vesical instrumentation. The mere question as to the comparative advantages of removal of stone by one or by several sittings, is dwarfed by the more important one of determining the state of the bladder that results, not perhaps so much as concerns the life, as the future comfort of the patient. It is here

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that information is much needed, and it is here that unfortunately, but for very obvious reasons, the lithotritist himself may in many cases be unable to furnish it.

(4) Pre-historic man was doubtless a victim of injury before he became a sufferer from disease, and the treatment of wounds constituted probably the first effort of the healing art. From the earliest dawn of human intelligence the attempt to cure a wound must have suggested itself to man; and yet, at the close of the nineteenth century, we are still discussing the best methods of doing this, and the causes of their failure. There is still difference of opinion and of practice amongst surgeons, not only as to the comparative advantages of the "open air" method, and that in which all atmospheric contact is carefully guarded against; of the "dry" and of the "moist" system of dressing; as to whether the "antiseptic method" in a modified form suffices, or whether the more elaborate system of local treatment before, during, and after an operation, which has been devised by the skill and worked out by the unwearied labour of Lister, be essential in all cases of operation wound. Not, of course, for its primary union —for this may be obtained by any and every of the methods mentioned. If it be contended that this system is necessary for the safety of the patient, and the due healing of the wound in some cases, has it been proved to be equally essential in traumatic lesions of all tissues, of all organs, and of all regions? These are questions that may well deserve the consideration of this Section. But there are others of a yet wider character that must also engage our attention in any discussion on the best methods of securing primary union in wounds, for it is impossible to fail to recognize, in the general constitutional state of the patient, a most important factor in this direction; and we should be taking a narrow view of this many-sided question if we did not give due weight to the influence of those hygienic conditions which, if faulty, are inimical, or even destructive to, the due performance of those actions which are necessary for the maintenance of the organism in a healthy state, and for the proper nutrition and consequent repair of the tissues of the body. Is there no fear that in some of the modern systems of treating wounds we are in danger of expending all our precautions in the prevention of the local, and of ignoring the risk of a constitutional, infection?

(5) The treatment of Aneurism is one of those great questions which, from an early period in the history of modern Surgery, has occupied the attention of practitioners, and has undergone no little fluctuation. A few years ago the battle between the ligature and compression appeared to have been decided in favour of the latter; but the invention of improved ligatures, made of various kinds of animal tissue, and applied with antiseptic precautions, has once more inclined the balance of professional opinion towards the Hunterian operation. But now again the practice of compression has received renewed strength from the employment of Esmarch's elastic bandage in the cure of certain forms of external aneurism, and it is for you to determine in what cases it can be used with advantage, and in what way a cure is effected by its means. For in the treatment of aneurisms, as in that of so many other surgical diseases, the wiser and more scientific course is to follow a judicious system of selection in the method to be employed in each particular case, rather than to subject all to one unbending line of practice.

(6) The treatment by Resection of some forms of chronic and otherwise incurable joint diseases, has in certain articulations, and at suitable ages, met with the universal approbation of surgeons, and the wide extension of the principles of "conservative surgery," is one of the most striking evidences of advance in our Art in modern times. Resection has, however, of late years come to be extensively applied to the treatment of cases of articular disease which formerly were subjected to procedures of a less heroic character; and it will be for the members of this Section to weigh carefully the wisdom of such a measure, and to contrast its results, both as regards life of patient and after utility of limb, with those which may be obtained from the employment of milder means,

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such as absolute immobility with extension, and possibly, in some cases, simple incision of the articulation.

(7) In considering the relations between Adenoma, Sarcoma, and Carcinoma in the mammary gland of the female, I would venture to submit that this subject has to be discussed here from its clinical rather than from its pathological side. We have here less to do with the ultimate structure of the tumours, with their histological affinities, with the parts that are played by epiblasts and mesoblasts, with what epithelium or connective tissue cells can or cannot do, than with their clinical history, their differential diagnosis in their earlier stages, the best time for their removal by operation, the liability to recurrence after operation, and the possibility in recurrence of the substitution of one form of disease for another. With these, and such questions as these, we, as Clinical Surgeons, may advantageously occupy ourselves.

(8) The last subject set down for discussion is one that has practical bearings of an importance that cannot be over-estimated. There are few questions of the present day of deeper surgical or social interest than the far-reaching, the apparently illimitable, and most pernicious extension of a syphilitic contamination of organs and of tissues; of the modifications impressed by it on other diseases that are the local developments of diatheses, whether strumous, tubercular, rheumatic, or gouty. Does the diathesis exercise any influence upon the form assumed by the syphilitic disease, and to what extent does it modify the characters presented by it in its primary and its secondary affections, more especially when the latter manifest themselves upon the skin or in the bones how far are gummata and caries, psoriasis, and rupia the consequences of a constitutional impress, influencing the direction of the syphilitic poison? To what extent may rickets and grey granulations be the ultimate products of the syphilitic taint? These and various other questions will probably occupy the attention of those who enter on the discussion of this wide-spreading subject.

We hope to be able to take the discussion of two questions on each day, so as to work through the eight in the time allotted to us. In addition to these there are various detached papers on subjects which are of much interest, but which scarcely admit of being classified under one or other of the above heads of discussion. These we shall take up as time and opportunity admit, but their number is so great that it is to be feared that full justice can scarcely be done to all, and that it will be unavoidable, on account of the limited time at our disposal, that a large number be read in abstract.

The Forms of Aneurism, in which Treatment by Esmarch's Elastic Bandage is applicable, and the Method by which a Cure is effected.

Staff-Surgeon Walter Reid, R.N.

Mr. President and Gentlemen,—The subject which I was requested to introduce is one on which I chance to be considered the original authority.

My original case was published in the *Lancet*, in September 1875, and I now give a brief account of it, in order that I may best explain the method of procedure.

The patient was a seaman, who had been a month under my care in the Royal Naval Hospital at Plymouth. He had a sacculated aneurism of the left popliteal artery. After failing to cure the disease by digital and instrumental pressure, I adopted the following procedure:—An Esmarch's elastic bandage was applied evenly from the toes up to the level of the aneurism, it was then wound lightly over the sac, after which it was continued as before, half-way up the thigh, where

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an elastic constrictor was applied. The bandage was now removed, and the constrictor kept in position for fifty minutes. At the end of this time, a Carte's compressor was adjusted, so as to control the circulation through the femoral at the pelvic brim. This being done, the elastic constrictor was removed. A few minutes afterwards I raised the pad of the compressor, and felt the aneurism. It was solid, hard, and pulseless. The pressure on the femoral was kept up in an intermittent manner by the patient himself for the next twenty-four hours, when the disease was practically cured.

This patient died eight months afterwards from other causes, and I was then offered the opportunity of examining the remains of the aneurism. An account of appearances which were found was published in the *Lancet* of August 5, 1876. It is enough to say here that they showed that consolidation must have taken place, not by the deposition of laminated fibrine, but by the coagulation of the blood *en masse;* further, that the vessel was occluded for about 2½ inches of its course by fibrous tissue, and that the collateral circulation had been carried on by vessels springing immediately above and below the occluded portion of the popliteal artery.

Now, the first point raised for discussion is, What are the forms of aneurism to which this treatment is applicable? It has always appeared to me that the only form of aneurism, in which success is likely to follow the use of this method, is that known as the sacculated. It is only in this form that I consider it possible to carry out one of the essential conditions of success—viz., the locking up of the blood in the aneurismal cavity; for the sac is like a flask, with walls, which have become more or less rigid by the deposition of laminated fibrine. It can only be emptied of its blood by collapse of its walls, or else by air or some other substance passing in to take the place of the blood. But this is not so with a fusiform aneurism, which must be looked upon merely as a dilatation of the arterial tube, open at both ends, and with collateral branches springing from it. We cannot, by this method of treating aneurism, lock up the blood in such a cavity, and maintain it in a state of stagnation, sufficiently long and complete for coagulation to take place.

In looking through the numbers of the *Lancet* and *British Medical Journal*, to the month of April last, I find reports of thirty cases of aneurism which have been treated successfully by this method. They were all popliteal, with two exceptions, one being an aneurism of the femoral and another of the anterior tibial. I think there can be little doubt that this method will be had recourse to chiefly in cases of sacculated aneurism occurring in the popliteal artery. It is only applicable to aneurisms in the extremities, for obvious reasons, and aneurisms in the extremities are, to a large extent, popliteal.

The second point which we have to consider, is the method by which cure is effected. I do not need to prove now, otherwise than by a reference to recorded experience, that it is possible to lock up the blood in the sac of an aneurism and keep it stagnant long enough for coagulation to take place *en masse*.

In my original case, I failed to effect a cure by pressure on the main artery of the limb, owing, as I believe, to the disturbing influence of the collateral currents upon the contents of the sac. To overcome this, it was necessary to arrest the currents of blood in all the vessels of the limb at some point above the aneurism. This could evidently be done by means of an elastic constrictor applied on the proximal side of the aneurism. This would arrest the currents and yet not empty the sac which, owing to its flask-shaped cavity, its small mouth and rather rigid walls, would retain its fluid contents free from all sources of agitation, and thus be in a condition favourable to rapid coagulation. The difficulty, in my mind, was to keep the constrictor applied

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long enough for the blood to coagulate, without endangering the safety of the limb subsequent to consolidation. The arteries, no doubt, would have become emptied by the blood passing on to the tissues. But then the veins would have become engorged, while therefore the blood in the aneurismal sac was undergoing consolidation, the same process must have taken place with regard to the blood in the distended veins. In the event, therefore, of a cure of the aneurism from consolidation of its contents I should have had to face the danger of a limb in which the circulation was doubly embarrassed; first, by obstruction to the main arterial current, arising from the presence of a suddenly consolidated aneurism in the main artery, and secondly, by obstruction to the venous currents arising from the presence of extensive coagulation in the veins. A limb with its circulation thus embarrassed must be in great danger of gangrene.

These were the considerations which led me to the use of Esmarch's elastic bandage; it seemed to me possible that by applying it after the manner described I might empty all the vessels of the limb, veins as well as arteries, of their blood, and yet keep the aneurismal sac full. In this I succeeded. Thus, the use of Esmarch's bandage is not curative so far only as the aneurism is concerned, but I regard it as essential to the safety of the limb.

That the blood does coagulate *en masse* in cases where this method of treatment is successful I think there can be no doubt. The post-mortem appearances in my own case confirmed this view. I shall not attempt to enter into the general question of the causes of the coagulation of the blood. This much, at any rate, is certain, that if we isolate a portion of the blood from the general circulation, and kept it in a state of stagnation, the tendency is for coagulation to take place. In about an hour, coagulation generally takes place, and the cure is effected.

This method consists essentially of three steps. I. The emptying of all the vessels of the limb of their blood, so as to avoid subsequent gangrene from an embarrassed circulation. II. Locking up the blood in the aneurismal cavity, and keeping it undisturbed till coagulation has taken place. III. Protecting the newly-formed clot from the influence of the arterial current until it has become sufficiently tough and solid to resist it.

I am unable to give as yet any reliable statistical results showing the proportion of successes to failures which have followed the use of this method. I find records of thirty successful cases in two British periodicals, up to April last, but of failures very few. Yet I know that there have been many failures, which have not been published. In justice, I must also say that I know of successes which have occurred, especially in the Australian colonies, and which have not appeared in the journals of this country. I invite surgeons to let us know of their failures, and, if possible, the causes of failure; it is thus that we shall come to know the true value of this practice, and perhaps arrive at improvements which shall render it more certain.

On the Modus Operandi of Esmarch's Elastic Bandage employed in the Treatment of External Aneurism.

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In considering the mode of action of Dr. Reid's treatment of external aneurisms the first fact to be noted is, that while the Hunterian ligature and digital or

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instrumental compression aim only at impeding or entirely stopping the direct flow of blood through the aneurismal artery, and depend for their success upon the establishment of a sufficiently free anastomotic circulation and unimpeded return of venous blood, the effect of the efficient application of Esmarch's elastic bandage is to empty every vessel, great and small, arterial, capillary, and venous, of the part to which it is applied, and stop entirely all circulation for the time. When applying the bandage for aneurism, however, care is always taken either not to cover in the aneurism at all, or to apply the bandage so loosely over it that the tumour and the part of the artery from which it springs are left full of blood, while the vessels above and below it are emptied by the firm pressure. The primary effect of the bandage, then, is to leave the aneurism and adjacent artery full of stagnant blood.

Unless submitted to incision or excision an aneurism is only cured by a filling up of its sac with some solid material derived from the blood, and its subsequent absorption. This solid material may be either pure fibrin, usually deposited in concentric laminæ, or blood-clot, or fibrin, in the meshes of which the serum and corpuscles are enclosed.

The only way of obtaining pure fibrin outside the body is by the process well known by the name of "whipping;" the fine filaments, as they form, adhere to the "whip," and are so separated from the corpuscles and serum. Whenever unaltered human blood is left motionless the forming fibrin encloses the corpuscles, and a true blood-clot is formed. The same rule holds good in the body during life; pure fibrin is only found when it is formed from blood rapidly moving, as, *e.g.*, the blood flowing over vegetations on the valves of the heart, or circulating tumultuously in an aneurismal cavity; when, on the other hand, fibrin separates in blood nearly or quite at rest, a blood-clot is the necessary result; we see this in the clots forming in ligatured arteries. The intermediate condition will lead to the production of an over-fibrinous coagulum. If, therefore, coagulation takes place in the aneurism and artery under treatment by the elastic bandage, the complete stasis of the blood ensures the formation of a blood-clot, and entirely prevents a separation of fibrinous laminæ. The examination of cases after death demonstrates that this is the actual result of the treatment (see Appendix), and we may assert that whenever, under this treatment, an aneurism becomes solid, it is from the coagulation *en masse* of the contained blood. The Table of Cases appended shows that this effect has been far more frequent than the ultimate cure of the aneurism, and that the two are not identical. The clotting of the blood is only one—the first—step towards the cure, but it is the only step taken while the bandage is applied, and so soon as the blood has firmly coagulated the bandage has ceased to be useful and ought to be removed. Clinical evidence shows that the blood-clot may disappear entirely when the obstruction to the circulation is removed (*cf.* Cases 16, 25, 36, 38), and this is the history of nearly all the cases of failure recorded in the table. There is distinct pathological evidence to show that the blood-clot may, under other circumstances, either become organized or remain for a time as a granular desiccated mass. The organization of blood-clot in ligatured, lacerated, or twisted arteries, in cavities, and in wounds is a familiar fact, and its occurrence in these cases of aneurism is shown by the examination of Cases 1 and 2 (Appendix). A blood-clot, however, possesses no inherent power of organization; it cannot by itself and of itself organize, and whether it is regarded as a favourable nidus for organizing lymph, or as being itself transformed into living tissue, it is equally dependent upon the surrounding

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tissues for the necessary vital stimulus and supply of nutritive material, which is again directly dependent upon the healthy condition of those parts. Thus, in a blood-vessel a thrombus is organized by lymph exuded from the vasa vasorum, and possibly in part by proliferation of the endothelium, and advanced disease of the vessel wall, as is well known, hinders or prevents altogether this occurrence. In the case of a clot in an aneurism not only is an enveloping healthy vessel wall wanting, but between the vascular sac and the clot is, in at least the great majority of cases, a more or less thick layer of laminated fibrin previously "whipped" from the blood. And, unlike blood-clot, pure fibrin is a particularly stable resistant unorganizable substance. In old aneurisms—*e.g.*, of the aorta—the outer laminæ of fibrin show no trace of organic adhesion to the sac, and are in nearly all their characters like the inner layers of more recent formation. Fibrin can be readily absorbed, but it appears not to form a suitable nidus for organizing lymph, and we therefore find that when it surrounds a blood-clot it acts as a barrier to the passage of plastic material and prevents the organization of such clot. This is well shown in Cases 1 and 2 (Appendix). These same cases show also that when a blood-clot is protected from circulating blood, and also does not organize, the serum is gradually expressed from it, and it becomes converted into a friable material. But both clinical and pathological evidence show that in successful cases of this treatment the clot also fills the part of the vessel from which the aneurism springs. Here the thrombus is not surrounded by fibrin, and if duly protected, and if the vessel itself be not in an advanced state of disease, it readily organizes. The aneurism is then completely protected from the impulse of the blood, and both it and the occluded artery are absorbed, and leave only a small fibrous cord behind. The clinical evidence of the truth of this statement is first the fact that after a lapse of weeks nothing has been felt of either tumour or artery (as in Case 13), and further the fact that wherever any reference is made to the subject in the reports of successful cases, it is noted that the smaller arteries of the part became enlarged, and evidently carried on the circulation—*e.g.*, the articular arteries about the knee-joint were seen and felt enlarged and pulsating in the cases of cured popliteal aneurism.

I would, therefore, submit to this Section this proposition, that in cases of cure under this treatment by the elastic bandage, the blood left stagnant in the aneurism and artery coagulates *en masse;* the part of the clot in the artery being surrounded by a more or less healthy wall, is organized into fibrous tissue; the part of the clot in the aneurism is either organized or more commonly shrivels and dries, and ultimately both aneurism and artery are nearly completely absorbed, an anastomotic circulation being established, as after the ligature of an artery in its continuity.

The Table of Cases contains all those I have met with in the literature accessible to me, but this plan of treatment has been adopted with varying success in many cases that have not been recorded. I would not, therefore, draw any statistical conclusions from the Table, but my object in drawing it up has been to show if possible what are the elements of success or failure; what conditions of aneurism, or of the treatment, are common to the cases cured, and what may explain the want of success. Passing over, for the present, many minor details, there are two or three striking points to which I will refer.

(1) The first is that in many cases (*e.g.* 19, 26, 30, 31, 32, 34, 39, 44) the blood left stagnant for a time varying from half an hour to two and a quarter hours, did not coagulate. In view of the fact that blood has been kept fluid in vessels shut off from

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the circulation for many hours (Sharpey), and that it can be kept in veins out of the body without coagulating for twenty-four to forty-eight hours (Lister), there is nothing surprising in this fact. That it ever coagulates is rather to be wondered at, and the causes of this clotting required to be determined: those that have occurred to me are, the irregularities of the sac, the contact with the laminæ of fibrin often present, or with structures other than endothelium, causes like those leading to clotting in an artery closed by a ligature, torsion or the actual cautery. But this difference in behaviour of the stagnant blood may be in part due to the varying degree of plasticity of the blood itself. And this consideration points to the necessity or, at any rate, desirability of taking means to increase this plasticity by preparatory treatment by diet and drugs, such as iodide of potassium; and everything should be done at the time to favour the clotting process, thus the part should be kept thoroughly warm—at a temperature of 100° Fahr.—and perhaps gentle manipulation of the sac might be of service.

(2) The second fact noticeable in the cases of failure is the frequency with which the clot first formed disappeared, and the aneurism which had consolidated to a greater or less extent resumed its original characters (*cf.* 16, 32, 33, 36, 38, 54). This process is not spontaneous, but the result of some force external to the clot itself, and probably it is the flow of arterial blood. Outside the body, blood when once coagulated never again assumes its former condition. In venous thrombosis the clot is never exposed to the impulsive flow of blood like that in an artery; and in other varieties of arterial thrombosis, the vessel is either artificially occluded, as by a ligature, and the flow through the vessel quite checked; or the clot forms when the force of the heart is weakened. In all the cases noted as cured where details are given, with one apparent exception (No. 62), some form of compression was applied to the artery for one or several hours after the removal of the elastic bandage, and others, although the same course was adopted in many of the cases of failure, in eight (Nos. 32, 33, 35, 36, 39, 45, 57, 60) the subsequent compression was not employed. In the absence of any other cause of the disappearance of the clot, it is, therefore, safe to assume that such neglect may be one of the causes of failure, and that compression of the vessel, subsequent to the removal of the bandage, is a necessary part of Dr. Reid's treatment.

(3) On removal of the elastic bandage there is still some pulsation perceived, even where the tumour has solidified, though it is as a rule slight, and in some cases has been described as distant (*cf.* 5, 6, 10, 17, 21, 22, &c.). This shows that the artery, and in some cases also the aneurism, too, is not completely obstructed by the clot, and hence a certain amount of blood flows through one or both. Indeed, this must be the case. For as soon as the blood-clot is formed it begins to contract and express serum, and supposing the whole of the blood in the aneurism and artery to have coagulated, the clot will soon cease to fill its original space, and will be surrounded by serum; when the bandage is removed the serum mingles with the blood. The practical bearing of this fact is, I believe, important, for, first, it teaches that this slight pulsation is not to be regarded as an unfavourable symptom; and, second, it teaches that the clot just formed requires to be enlarged by fresh deposits on its surface before it can fill the artery and adhere to the walls of it. The most favourable circumstance for this growth is a gentle trickle of blood into the artery for a few hours, such as occurs under digital or instrumental compression, and it further illustrates the necessity for this subsequent compression. Probably we find here an explanation of the peculiarity

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observed by Mr. Clutton in Case 52; the aneurism was very large, and a few days after its consolidation, and when it had been shrinking in size, without any return of pulsation, fluctuation was noted in the sac. In contracting, this bulky clot must have expressed a large quantity of serum which, collected in the sac, would cause fluctuation.

This treatment is not without its dangers. The application of the elastic bandage has been shown to cause an increase of blood pressure in the arteries of the rest of the body under which aneurisms elsewhere may burst, as in Case 53; while in Cases 2 and 52, death occurred after an interval from rupture of an aortic aneurism, and it is difficult to escape the conviction that these aneurisms might have been produced or exaggerated by the treatment—at any rate the diseased aorta, or the aneurism, as the case may be, was exposed to the worst possible influence. Case 29, in which there was fatal collapse, demonstrates the effect of this increased arterial pressure upon a fatty heart. As disease of some other part of the arterial system or degenerative change in the heart is often associated with an aneurism requiring treatment, it is certainly injudicious to run such a risk as this unnecessarily. It is unnecessary, for without letting blood, we can obtain a similar effect by either applying a ligature around the upper part of the opposite limb just tight enough to obstruct the venous return, or, far better, put the limb in Junod's boot and exhaust the cavity around it, and in either case retain an excess of blood in the fellow limb, and so relieve the heart and general circulation.

(4) Failure to cure an aneurism by this treatment appears not to lessen the prospect of the successful issue of ligature of the artery above. Out of 20 cases, in which it is noted that the ligature was subsequently employed 17 were cured; in one gangrene of the foot occurred (No. 36); in one death occurred from blood-poisoning (No. 38); and in one case of diffuse aneurism amputation was subsequently performed (No. 57).

In conclusion, I would submit the following propositions:—

1. That in all cases, preparatory treatment, with the view of increasing the plasticity of the blood, should be employed, unless the case is too urgent to admit of the necessary delay.

2. That the bandage should be applied so as to leave the aneurism full of stagnant blood, and should be retained until the aneurism has consolidated, if that occurs within five hours. During this time the limb should be kept at a temperature of 100° Fahr.

3. That it is desirable to avoid increasing unduly the blood pressure in all cases, but particularly where the heart or arteries are evidently diseased, and that with this view efforts should be made to keep an excess of blood in the veins of the opposite limb during the application of the bandage.

4. That after the removal of the bandage, the arterial circulation should be controlled by instrumental or digital compression, for at least a few hours after all pulsation has ceased in the sac.

5. Failure to cure by this treatment does not diminish the prospect of cure by other means.

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A Table of Cases of Aneurism treated by Esmarch's Elastic Bandage.

ANEURISMS OF POPLITEAL ARTERY.

No.	*Name of Surgeon and Reference.*	*Clinical History.*	*Characters of Aneurism.*	*Treatment.*	*Result.*
1	Dr. W. Reid. *Lancet*, 1875, ii. 448, & 1876, ii. 184.	Marine, æt. 37. Strain while lifting sails 3 months before, hypertrophy of heart, cirrhosis of liver.	Sacculated: of "considerable size, with strong pulsation."	Rest and farinaceous diet; then genuflexion, followed by Carte's compressors—no improvement. Esmarch's bandage from instep to middle of thigh—lightly over tumour; elastic tubing applied and bandage removed; aneurism unaltered in size: tubing removed in 50 minutes—Carte's compressors applied intermittently at the groin for 30 hours.	Cured. Patient died subsequently from bronchitis, &c.
2	Mr. Wagstaffe. *Lancet*, 1876, ii. 461.	Barman, æt. 32. Strain 4-5 months previously. Throbbing noticed 2 months.	2 in. long, filling upper half of popliteal space.	Esmarch's bandage up to, and lightly over, tumour, and firmly up the thigh; tubing not used; removed in 50 minutes: 2 tourniquets applied to control artery for 6½ hours: more loosely for 11½ hours, and very loosely for 11 hours more.	Cured. Patient died subsequently from rupture of small aneurism of aorta, atheroma of aorta; other arteries healthy.
3	Mr. F. A. Heath *Lancet*, 1876, ii. 638.	Labourer, æt. 29. No history of syphilis; alcoholism. Symptoms noticed 3 weeks.	Aneurism size of small orange	Esmarch's bandage applied firmly as high as ham, man then stood up, bandage lightly applied over tumour, and tightly up the thigh; tubing not used; bandage left on 1 hour. Signoroni's tourniquet applied for 5 hours, and loosely for 2 days more.	Cured. No pulsation when bandage removed. Anastomotic arteries felt pulsating over knee.
4	Mr. T. Smith. *Lancet*, 1877, i. 750.	Bonnet blocker, æt. 45. No history of syphilis. Symptoms noticed 3 weeks.	Aneurism size of a hen's egg, pulsation very forcible.	Rest in bed 10 days. Esmarch's bandage applied up to the aneurism, not over it, and then up the thigh; left on for 1 hour. Signoroni's tourniquet applied for 2 hours.	Cured.
5	Mr. Tyrrell. *Lancet*, 1877, i. 940.	Grocer, æt. 36. No history of syphilis; history of alcoholism. Pain in ham 6 weeks; pulsation noticed 12 days.	Aneurism measured 5 by 5½ in.	Restricted dry meat diet. Esmarch's bandage up to, lightly over, and firmly above, tumour; tubing applied; removed in 50 minutes. Digital compression to artery 2 hours; followed by compressor at groin "moderately tight."	Cured. Aneurism was quite hard, with a very slight pulsation when bandage was removed. Subsequently the articular arteries around the knee were seen and felt to pulsate.
6	Mr. Croft. *Lancet*, 1878, i. 85.	Woman, æt. 43. No history of syphilis. Pain felt 8½ months.	Aneurism was as large as an orange, pulsated freely, no appearance of clot.	Milk diet and rest 3 days. Esmarch's bandage up to and above aneurism, retained 1 hour; digital compression, 6 hours.	Cured. On removal of bandage pulsation at once recurred, though not so strongly as before.
7	Dr. G. B. Ferguson. *Lancet*, 1878, ii. 439.	Cabinetmaker, æt. 69. Temperate habits, arteries not apparently atheromatous. Symptoms noticed 3 months.	Aneurism size of a large hen's egg, expanding forcibly.	Rest in bed and iodide of potassium 1 week. Esmarch's elastic tube applied tightly round the thigh for 1 hour; then digital compression of artery 2 hours 20 minutes; tube for ½ hour; digital compression 1 hour; tube ½ hour: digital compression 1 hour 20 minutes; tube 1 hour; digital compression 1 hour.	Cured. Anastomotic arteries felt pulsating over the inner condyle and patella.
8	Mr. T. Smith. *Lancet*, 1878, ii. 880.	A medical man.	Not stated.	Elastic bandage applied below and above aneurism "as long as patient could bear it," then a tourniquet was firmly applied; this alternation was continued throughout the day: after nine hours there was severe pain at the seat of the aneurism.	Cured. The art. anastomotica was seen to pulsate after the consolidation of the aneurism.
9	Dr. McDougall *Lancet*, 1878, ii. 880.	Not stated.	Aneurism was large with thin walls.	Exact treatment not stated.	Cured.

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No.	Name of Surgeon and Reference.	Clinical History.	Characters of Aneurism.	Treatment.	Result.
10	Mr. Hewetson. *Lancet*, 1879, ii. 81.	Marine, æt. 26. Syphilis. Symptoms noticed 2 months.	Not stated.	Esmarch's bandage applied below and above aneurism for 80 minutes; digital compression kept up for 11½ hours after.	Cured. Faint pulsation when bandage was removed; 6 hours later, no pulsation. Articular arteries felt to pulsate after the consolidation.
11	Mr. S. Jones. *Lancet*, 1880, i. 289.	Not stated.	Not stated.	Esmarch's bandage applied for 1 hour, and followed by digital compression.	Cured.
12	Mr. Croft. *Lancet*, 1880, i. 289.	Male, æt. 36. Syphilis. Symptoms noticed 10 weeks.	Not stated.	Esmarch's bandage up to and above aneurism 1 hour; followed by digital compression 2½ hours.	Cured.
13	Mr. Goodsall, *Lancet*, 1880, i. 770.	Porter, æt. 30. Produced by a strain.	Aneurism 2 in. in diameter.	Rest in bed and low diet 6 days. Esmarch's bandage up to and above aneurism, with the tubing 3 hours; then upper part removed and a tourniquet applied to femoral artery for 1 hour.	Cured. Articular arteries were subsequently felt to pulsate.
14	Mr. Rivington. *Lancet*, 1880, ii. 608.	Soldier, æt. 44. Syphilis. Pain felt 2 months.	Aneurism size of a large orange or shaddock; it felt solid.	Esmarch's bandage and strap 1 hour. Digital compression 3 hours.	Cured.
15	Mr. C. Macnamara. *Lancet*, 1881, i. 539.	Fishmonger, æt. 32. Symptoms noticed 3 weeks.	Aneurism size of a hen's egg.	Esmarch's bandage applied up to and above aneurism 55 minutes: digital compression 1 hour; genuflexion. Esmarch's bandage re-applied, 5 days later, for 1 hour; digital compression 2½ hours, followed by genuflexion.	Cured. Slight pulsation felt after the digital compression at first sitting, none after the second sitting.
16	Mr. S. Jones. *Br. Med. Jour.* 1877, ii. 562.	Dairyman, æt. 31. Pain felt for 2 months.	Aneurism size of a cricket ball; walls seemed to be very thin and pouched in places.	Esmarch's bandage below and above tumour 65 mins; Carte's compressors on artery 4½ hours, followed by digital compression 4½ hours, no pulsation felt; digital compression 3½ hours longer. Next day slight pulsation, digital compression 5 hours, subsequently aneurism pulsated freely, but was much firmer than at first. Esmarch's bandage re-applied for 50 minutes, and digital compression 5 hours 10 minutes.	Cured. After first sitting "the collateral circulation" was felt on inner and outer sides of knee.
17	Mr. Croft. *Br. Med. Jour.* 1880, ii. 15.	Retired soldier, æt. 34. Syphilis. Symptoms 5 months	Size of a full-sized orange, fluctuation, pulsation free.	Rest in bed, milk diet with meat, iodide of potassium. Esmarch's bandage up to and above tumour 1 hour, digital compression 4 hours.	Cured. No pulsation felt after two hours' digital compression. Death subsequently from innominate aneurism.
18	Mr. J. Hutchinson. *Clinical Soc. Trans.* xii. 55.	Gentleman, æt. 26. No history of syphilis. Pain 3 months. Pulsating tumour 3 weeks.	Aneurism filled the ham, pulsation strong and visible, loud bruit.	Rest in bed 6 days; comparative abstinence from fluids. Esmarch's bandage applied firmly below and above tumour, and lightly over it, and tubing around thigh, then bandage removed, tube removed after 70 minutes, horseshoe tourniquet applied to artery for 9 hours.	Cured.
19	Do. do.	Artilleryman, æt. 41. Syphilis. An aneurism in calf 2 years before, cured by compression. Symptoms 2 weeks.	Aneurism as large as an orange, rapidly increasing.	Esmarch's bandage applied as above 1 hour; interrupted instrumental compression 12 hours; no benefit. Four days later Esmarch's bandage re-applied nearly 2 hours, followed by digital compression 11 hours.	Cured.
20	Do. do.	Not stated.	Not stated.	Dry diet; iodide of potassium. Esmarch's bandage as above; digital compression some hours	Cured.
21	Mr. C. Heath. *Clinical Soc. Trans.* xiii. 151.	Coal miner, æt. 32. No history of syphilis. Pain 10 weeks. Rigidity of brachial artery.	Aneurism size of a hen's egg.	Genuflexion for six hours, no result. Esmarch's bandage above and below aneurism and tubing round thigh 3 hours; Carte's compressors 14 hours.	Cured. No pulsation when bandage was removed; 1 hour later slight pulsation, compression then begun. Anastomotic arteries around knee enlarged.

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No.	Name of Surgeon and Reference.	Clinical History.	Characters of Aneurism.	Treatment.	Result.
22	Mr. C. Heath. *Clinical Soc. Trans.* xiii. 151.	Potman, æt. 37. No history of syphilis, a free drinker; no sign of arterial degeneration; symptoms 2 months.	Aneurism rather larger and softer than the above, oval in shape (?) fusiform.	Esmarch's bandage applied as above 3½ hours—no effect Carte's compressors ineffectually applied. Digital compression 3 hours; pulsation much lessened; flexion 6½ hours; pulsation still forcible; flexion 4 hours; Esmarch's bandage 3½ hours; flexion 6 hours.	Cured. After second sitting the aneurism was much harder, and presented only slight pulsation, which had disappeared next day. An enlarged artery felt on inner side of patella.
23	Dr. O. Bloch, of Copenhagen, Hospitals—Tidende, 1877.	Traumatic.	Not stated.	Esmarch's bandage and tube 1 hour; Carte's compressors 1 hour, followed by digital compression of artery 5½ hours.	Cured.
24	Dr. Freeman. *New York Med. Jour.* xxxi. 638.	Not stated.	Not stated.	Esmarch's bandage up to, lightly over, and firmly above aneurism 80 minutes, followed by digital compression 65 min.; following day some pulsation digital compression again 3½ hrs.	Cured.
25	Dr. T. T. Sabine. *New York Med. Jour.* xxxi. 638.	Not stated.	Not stated.	Esmarch's bandage 75 minutes; Horseshoe tourniquet 2½ hours; pulsation returned next day and increased. Esmarch's bandage re-applied for 3½ hours, followed by horseshoe tourniquet for 3½ hours.	Cured.
26	Mr. M. Baker. St. Bart. Hosp. Rep. xv. 75.	Male, æt. 40. No history of syphilis; symptoms 7 months	Tumour filled the ham; faint pulsation, due to a leakage of blood from the sac.	Esmarch's bandage 35 minutes; compression of femoral artery 25 minutes—no improvement. Four days later bandage re-applied below and above tumour 65 minutes; digital compression 50 minutes; bandage 35 minutes; digital compression 85 minutes; next day swelling in ham was large, soft, and fluctuating, but no pulsation in it.	Cured.
27	Mr. Manifold. *Lancet*, 1878, i. 80; and Mr. Chauncy Puzey *Lancet*, 1879, ii. 575.	Dock labourer, æt. 34. Pain 16 days.	Aneurism size of orange.	Esmarch's bandage applied as high as, then lightly over, and firmly above, the tumour, and tubing affixed; retained ¾ hour, tourniquets 4 hours longer. Two days later bandage re-applied for ¾ hour; next day genuflexion.	Tumour became firmer during first sitting, but pulsation was not abolished until after continuous genuflexion; he was then discharged "cured." Admitted to hospital 15 months later with history of pain, numbness, stiffness of joint and pulsation during the whole of that time. Aneurismal tumour distinct; cured by instrumental compression.
28	Mr. Thornton. *Lancet*, 1879, ii. 238.	Soldier, æt. 37. No history of syphilis or alcoholism. Symptoms of 3 months' duration.	Aneurism measured 3 by 2 in., very expansile, with full pulsation.	Esmarch's bandage and tube applied so as not to stop all pulsation in the tumour, and in combination with Signoroni's tourniquet.	Cured.
29	Dr. R. F. Weir. "Arch. of Med." xxxi. 210.	Negro, æt. 38. Phthisis. Symptoms 4 months' duration, following a strain.	Aneurism larger than the closed fist, pulsation free, with loud sharp bruit.	Esmarch's bandage up to and above tumour, and elastic cord, 2 hours 35 minutes; then cord only removed and Signoroni's tourniquet applied for 1 hour 50 minutes; pulsation then abolished, recurred next day. Bandage and tube re-applied for 2 hours 50 minutes, and bandage only for 4 hours longer, followed by Signoroni's tourniquet ½ hour, no pulsation. Collapse and death 27 hours later.	Death.

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No.	Name of Surgeon and Reference.	Clinical History.	Characters of Aneurism.	Treatment.	Result.
30	Mr. M. Bradley *Br. Med. Jour.* 1876, ii. 571 and 767.	Patient, æt. 35. No history of syphilis. Symptoms 7 weeks.	Aneurism size of an orange.	Rest in bed with Carte's tourniquets for 3 days; then Esmarch's bandage, leaving the whole ham uncovered; no effect. Carte's tourniquets applied for a week. Then Esmarch's bandage, as before, for 65 minutes.	Not cured. Subsequently cured by ligature of the femoral artery.
31	Dr. Campbell. *Lancet*, 1878, i. 86.	Fireman, æt. 39. Well-marked atheroma of other arteries.	Aneurism size of orange.	Esmarch's bandage up to, lightly over, and firmly above the tumour, retained for 50 minutes, followed by tourniquets for some time; repeated 3 times.	Not cured. Subsequently cured by ligature of the femoral artery.
32	Mr. Barwell. *Lancet*, 1878. i. 123.	Soldier, æt. 49. History of syphilis and alcoholic excess, chronic rheumatism, and phthisis, marked dilatation and atheroma of arteries. Symptoms 13 months.	The aneurism formed a large fusiform tumour, 3 by 2 in.	Esmarch's bandage from toes up the thigh; 70 minutes; no effect; 3 days later repeated for 5 hours; the aneurism was then solid, but pulsation soon returned to same extent as before.	Not cured. Subsequently cured by ligature of the femoral artery.
33	Mr. H. W. Page. *Lancet*, 1878. i. 414.	Carman, æt. 28. No other disease; pain noticed 1 month.	Not stated.	Esmarch's bandage up to and above aneurism 90 minutes; gentle manipulation of the aneurism; subsequently, a tourniquet to control, but not to arrest arterial flow; the aneurism became firmer. The application of the bandage was repeated for 1 hour; the tumour was then firmer, and the pulsation less marked and more distant; the bandage again applied for an hour; after this the aneurism increased in size.	Not cured. Ligature of femoral artery was subsequently successful.
34	Mr. James Lane *Lancet*, 1878, i. 682.	Carman, æt. 28. No history of syphilis nor alcoholic excess. History of a slip 2 months before; pain for 3 weeks.	Aneurism the size of a small orange; pulsation not well marked.	Three days' rest in bed. Esmarch's bandage applied "in the usual manner" for 1 hour; followed by digital compression of the artery for 1 hour, and afterwards a tourniquet. Pulsation, with all its original force, was observed quickly after.	Not cured. Subsequently cured by ligature of the femoral artery.
35	Mr. Barwell. *Lancet*, 1878. ii. 880.	Arteries atheromatous, and a fusiform aneurism of axillary artery, albuminuria, phthisis.	Aneurism fusiform.	Esmarch's bandage applied up to and above aneurism, which was protected by a tin plate; retained 1½ hour each time for six sittings. No mention of subsequent compression of artery.	Not cured. Subsequently cured by ligature of femoral artery.
36	Mr. Bryant. *Med. Times & Gazette*, 1878. vol. ii. 101.	Male, æt 45. No syphilis.	Aneurism large and rapidly growing.	Esmarch's bandage up to and above aneurism for 3 hours; sac was then firmer, the pulsation altered, but in 3 days all improvement had disappeared. Bandage was repeated, and with the same result.	Not cured. Subsequent ligature of femoral artery; gangrene of foot fourth day after operation.
37	Mr. Norton. *Lancet*, 1878. ii. 880.	Cardiac and arterial disease. Two femoral aneurisms on opposite sides.	Not stated.	Exact method not stated.	Not cured.
38	Mr. T. Smith. *Lancet*, 1879. ii. 121.	Soldier, æt. 48. History of syphilis. Cardiac disease. Symptoms noticed 8 months.	Not stated.	Esmarch's bandage up to and above aneurism for 1 hour; then digital compression 15 minutes, and tourniquets 2½ hours; the bandage again applied for 1½ hours, and followed by tourniquets; the tumour felt solid, but pulsation returned in a few minutes. Bandage re-applied for 1½ hours; tourniquets, 1½ hours; the pulsation returned next morning, and the tourniquet was tightened for 3 hours, but without permanent result.	Not cured. Subsequent ligature of femoral artery. Death.
39	Mr. E. Bellamy. *Lancet*, 1880. i. 248.	A discharged soldier, æt. 39. History of syphilis and rheumatic fever, a moderate drinker. Symptoms 9 days.	The aneurism as large as a pigeon's egg, rapidly getting larger, sac very thin.	Esmarch's bandage applied for 30 minutes at a time, every 4 hours, for 4 days; no clot, tumour larger, pulsation greater, sac thinner.	Not cured. Cured by ligature of the femoral artery.

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No.	Name of Surgeon and Reference.	Clinical History.	Characters of Aneurism.	Treatment.	Result.
40	Mr. T. Smith. *Lancet*, 1880, i. 289.	Not stated.	Not stated.	Esmarch's bandage and cord 1 to 1½ hours, varied with tourniquet.	Not cured. Subsequently ligature of femoral artery.
41	Mr. T. Smith. *Lancet*, 1880, i. 289.	Not stated.	Not stated.	Esmarch's bandage and cord 1 to 1½ hours, varied with tourniquet.	Not cured. Femoral artery ligatured.
42	Mr. T. Smith. *Lancet*, 1880, i. 289.	Not stated.	Not stated.	Esmarch's bandage and cord 1 to 1½ hours, varied with tourniquet.	Not cured. Femoral artery ligatured.
43	Mr. T. Smith. *Clin. Soc. Trans.* xi. 51.	Stevedore, æt. 43.	Aneurism as large as a fist, pulsation very forcible, bruit loud.	Digital compression of artery—no effect. Ligature of femoral artery with catgut, softening of ligature—no effect on aneurism. Esmarch's bandage applied for 50 minutes; tourniquet for 1½ hours.	Not cured. Subsequently cured by the application of a silk ligature to the femoral artery.
44	Mr. C. Heath. *Clin. Soc. Trans.* xi. 49.	Engine-driver, æt. 36. No history of syphilis. Arteries somewhat tortuous. Pain noticed 8 weeks, pulsation 10 days.	Aneurism filled the ham; a shrill rough murmur.	Carte's tourniquets 5 hours and 4½ hours. Esmarch's bandage 2½ hours, tourniquets 4 hours. Next day pulsation strong, tourniquets 5 hours 50 minutes. Next day Esmarch's bandage 2½ hours, and tourniquets 2 hours; followed by repeated and long applications of genuflexion and instrumental compression.	Not cured. Success followed ligature of femoral artery.
45	Dr. Markoe. Roosevelt *Hosp. Record.*	Spontaneous.	Not stated.	Esmarch's bandage up to, lightly over, and firmly again above tumour, retained for 2 hours. No subsequent compression.	Not cured. Subsequent successful ligature of femoral artery.
46	Mr. Maunder. *Lancet*, 1878, ii. 880.	Not stated.	Not stated.	Exact method not stated.	Not cured. Subsequently cured by ligature of femoral artery.
47	Mr. Maunder. *Lancet*, 1878, ii. 880.	Traumatic.	Not stated.	Exact method not stated.	Not cured. Subsequently cured by digita compression.

ANEURISMS OF FEMORAL ARTERY.

48	Dr. R. F. Weir, *New York Med. Journal*, xxix., 520.	Idiopathic, of a few weeks' duration.	Femoro-popliteal. Aneurism as large as doubled fists; walls thick, pulsation feeble.	Elastic tubing only applied to thigh for 90 minutes, followed by Signoroni's tourniquet for 2½ hours; pulsation was then abolished, but returned feebly next day; the tube was then reapplied, for 1 hour, and Signoroni's tourniquet for 16 hours; but it failed. Esmarch's bandage was then applied below and above aneurism, and the tube fastened above for 2½ hours; Signoroni's tourniquet 2½ hours; and Nicaise's elastic tourniquet for 5 hours; there was no ill effect, and cure took place.	Cured.
49	Mr. T. Wright. *Lancet*, 1877, i. 163.	Fitter, æt. 39. History of syphilis; aneurism attributed to a blow. Symptoms 6–7 months.	Aneurism size of an orange, in Hunter's canal.	Restricted diet. Carte's compressor and shot bag compression—no relief. Then Esmarch's bandage up the limb, loosely over the tumour; tubing applied and retained 2½ hours; bandage 1 hour longer. Shot bag on femoral artery for 5 days.	Cured. Slight pulsation was detected in aneurism for 4 days after sitting.
50	Mr. Hutchinson. *Clinical Soc. Trans.* xii. 55.	Not stated.	Not stated.	Dry diet and iodide of potassium. Esmarch's bandage up to, lightly over, and firmly above aneurism, and tubing 1 hour; digital compression some hours.	Cured.
51	Dr. R. F. Weir. *Amer. Jour. of Med. Science*, lxxvii. 33.	Soldier, æt. 45. Severe pain and pulsation followed an injury from pommel of saddle.	Aneurism at apex of Scarpa's triangle, 6 in. in diameter; pulsation strong; bruit, harsh, and loud.	Esmarch's bandage up to and above aneurism, and elastic tubing retained for 93 minutes: Signoroni's tourniquet 3 hours 37 minutes, and shot bag compression for 15 hours longer.	Cured.

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No.	Name of Surgeon and Reference.	Clinical History.	Characters of Aneurism.	Treatment.	Result.
52	Mr. Clutton. *Br. Med. Jour.* 1880, i. 441.	Waterman, æt. 34. History of syphilis. Symptoms 1 month.	Aneurism at apex of Scarpa's triangle, very large; sac very thin, with little or no clot: femoral artery above palpably diseased.	Esmarch's bandage was applied as high as tumour and a tourniquet placed on artery at brim of pelvis for 1 hour; bandage then removed, and tourniquet continued for a few hours; no change effected. The above treatment was repeated, the tourniquet being retained for 9 hours: the aneurism was then solid. Digital compression for 26 hours longer.	Cured. The aneurism remained free from pulsation and continued to shrink, after a few days; though free from pulsation, it contained fluid. Death occurred suddenly from rupture of an aortic aneurism.
53	Mr. F. A. Heath, *Br. Med. Jour.* 1877, i. 496.	Weaver, æt. 37. History of syphilis. Symptoms 15 mths.	Aneurism of common femoral artery.	Esmarch's bandage up to and over aneurism 1 hour; Lister's abdominal tourniquet applied to aorta 2 hours, and to common iliac artery 10½ hours; digital compression 13 hours. Esmarch's bandage re-applied, with Lister's compressor on aorta, 10½ hours. The sac consolidated, but collapse occurred, and death, from rupture of an aneurism just above the pad of tourniquet.	Death.
54	Dr. G. Poinsot. Bull: et Mem: de Soc.: de Chir. de Paris. 1880, 574.	Male, æt. 36. No history of syphilis or alcoholism. History of strain.	A large aneurism at the apex of Scarpa's triangle	Esmarch's bandage 55 mins. followed by digital and instrumental compression; the tumour became solid but softened again; a repetition of the treatment was attended tended with the same results.	Not cured. Subsequently cured by ligature of the artery in Hunter's canal.
55	Dr. J. Fleming. *Br. Med. Jour.* 1877, ii. 474.	Soldier, æt. 28. Doubtful history of syphilis. Symptoms 3 months.	Femoral aneurism in Scarpa's triangle, measuring 3¼ + 3¾ in., pulsation very strong.	Compression of the ext. iliac artery at intervals for several days. Esmarch's bandage applied up to the aneurism for 1 hour 50 minutes, compressor on the iliac artery.	Not cured. (Subsequently cured by ligature of external iliac artery.)
			ANEURISMS OF OTHER ARTERIES.		
56	Mr. Staples. *Lancet*, 1879, ii. 791.	Soldier, æt. 39. Symptoms 2 years.	Aneurism springing from ext. iliac artery close to Poupart's lig., 3½ in. in diameter.	Esmarch's bandage applied as high as groin, and a pad and Lister's tourniquet applied to common iliac artery. Bandage removed in 30 minutes, and the tourniquet in 60 minutes; pulsation was then feeble, and it entirely ceased next day.	Cured.
57	Mr. S. M. Bradley. *Br. Med. Jour.* 1877, ii. 810.	Male, æt. 37. Previously healthy.	Aneurism of internal circumflex artery, "clearly circumscribed at the inner edge of Scarpa's triangle."	Esmarch's bandage applied so high that the highest "turns" pressed upon the aneurism, retained for 1 hour; the tumour felt harder and was pulseless; next day pulsation returned along the outer side, and the aneurism became diffused.	Not cured. Subsequent ligature of artery, amputation, death.
58	Mr. Cornish. *Lancet*, 1878, i. 235.	Labourer, æt. 20.	Traumatic aneurism of anterior tibial artery, the size of a small hen's egg.	Esmarch's bandage applied up to, and then above, tumour, and with tubing for 1 hour; tourniquet firmly applied to femoral artery 3 hours 10 minutes, loosely applied for 3½ hours longer.	Cured.
59	Mr. Rivington. *Lancet*, 1880, ii. 808.	Labourer, æt. 58. Symptoms 6 weeks.	A very large false aneurism of anterior tibial artery, which had been opened by mistake for an abscess; the calf protruded and pulsated.	Esmarch's bandage up to, lightly over, and firmly beyond, the tumour for 1 hour; tourniquet applied to femoral artery: no effect. Bandage again applied for 1 hour 20 minutes; pulsation much less; digital compression. Pulsation and murmur entirely ceased, but gangrene supervened.	Death.
60	M. J. Debaisieux *Journ. des Sc. Méd. de Louvain*, 1877, Juin.	Male, æt. 22. Gunshot wound.	Varicose aneurism of brachial artery.	Esmarch's bandage applied twice for 55 and 60 minutes respectively: slight alteration in the size of the aneurism and the character of the bruit.	Not cured. Subsequent and spontaneous cure.
61	Mr. S. Jones. *Lancet*, 1880, i. 289.	Not stated.	Axillary aneurism.	Esmarch's bandage applied for 1 hour, followed by digital compression.	Not cured.

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No.	Name of Surgeon and Reference.	Clinical History.	Characters of Aneurism.	Treatment.	Result.
62	Dr. G. A. Peters. *New York Med. Journal*, xxxi. 637.	Not stated.	Subclavian aneurism.	Distal compression (inferred to be Esmarch's bandage) applied up the limb to the aneurism for 3½ hours, on two occasions.	Left hospital apparently cured.

Post-mortem appearances in Case 1.—Artery occluded for 2½ in. by fibrous tissue; sac of aneurism well defined, size of a walnut; its centre and the part next the vessel filled with amorphous, non-laminated coffee-coloured substance, with no appearance of organization or vascularization; opposite the mouth several layers of laminated fibrin, some of them partly displaced inwards, and the spaces between them filled with amorphous material.

Post-mortem appearances in Case 2.—"Path. Soc. Tran.," xxix.-72. Artery blocked above and below aneurism to near a large branch; aneurism plugged, the outer part and clot organized; the central part still in a state of blood-clot; no appearance of laminated fibrin.

Post-mortem appearances in Case 29.—Artery above aneurism empty, rough, and thickened; below filled with a small amount of soft recent clot to some distance below the bifurcation of the popliteal trunk. Aneurism filled with well-formed recent clot; mouth 1½ in. long; heart fatty; phthisis.

Post-mortem appearances in Case 52.—Femoral artery filled with a thrombus, partially decolorized, and adherent to the walls of the vessel. Aneurism almost entirely "occupied by a recent (post-mortem) clot, very little indeed being decolorized in the peripheral part of the sac."

Post-mortem appearances in Case 53. (From Mr. Heath's private notes.)—Condition of artery not mentioned. Aneurism filled with coagulated blood: in front the clot firm and resistant; at the back, loose, and breaking down readily.

Post-mortem appearances in Case 59.—Anterior tibial artery nearly completely divided. Above the false aneurism the artery was filled "with partially decolorized fibrin," and the clot extended into the popliteal artery; the decolorized portion of the clot occupied the popliteal artery. Sac filled with broken-down coagulum.

DISCUSSION.

Mr. Thomas Bryant, London: The papers to which we have listened may be regarded as very excellent expositions of the value of the practice of treating aneurisms of the extremities by means of Esmarch's bandages; they not only contain ample evidence to prove the practice to be good, but to suggest that it should be employed more frequently than it has been; indeed, when practicable, in most cases in which the treatment by pressure can be entertained. With much of what the papers contain I am disposed to agree, but there are some points of danger to which the authors have failed to allude, and it is to them I would draw attention. I should, first, however, like to remind surgeons that when an aneurism is cured, it is so not only by the coagulation of the blood in the sac, but, as often as not, by the coagulation of the blood in the afferent or efferent vessels, and that many preparations demonstrate this; and, secondly, I would recall the fact, that to bring about this cure, either a slowing of the blood-current through, or its absolute arrest in, the aneurismal sac and its arterial contributories is absolutely essential. Surgeons, by the use of Esmarch's bandages for the cure of an aneurism, effect more than they do either by the Hunterian or distal operations, or by digital or instrumental pressure, on the afferent or efferent vessels, for they not only slow, but positively obstruct, the flow of blood simultaneously through both the afferent and efferent vessels of the aneurisms. Under these circumstances, if by the use of Esmarch's bandage surgery can claim good results such as those we have listened to, it must be prepared to meet with failures and even bad results; for history clearly tells us that evils of a serious kind have followed operations which effect a slowing of the blood-current through the aneurismal sac and its supplying or efferent vessels, and if so, an equal, if not a greater, proportion of evil may fairly be expected by a method of treatment which totally obstructs the circulation of the affected limb. In what proportion of cases this evil will be met with, time and experience can alone prove. The danger to which I would draw your attention is that of gangrene. In my own practice, I have employed Esmarch's bandage in the treatment of three cases of popliteal aneurisms, but in no

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instance with success. In one, that of George B——, aged 45, I applied the bandage on February 21, 1880, for one hour, after which, the pain caused by it being unendurable, it was removed, no change having taken place in the aneurism. On the day following its application small capillary ecchymoses, the size of split peas, were seen in the skin over the tumour; later on, when the parts had recovered themselves, the femoral artery was tied, and a good recovery ensued. In the second case, Elias W——, aged 42, I applied the bandage tightly on December 6, 1880, for thirty, and subsequently more loosely, for eighty minutes, with no good results, and on December 7 ecchymoses were seen in the skin over the sac, as in the former case. On December 10, the femoral artery was ligatured, the wound rapidly healed, and the aneurism became cured. In a month, however, acute albuminuria set in, and death on February 17, sixty-nine days after the operation, and thirty-seven after the first appearance of albumen. At the date of the operation the urine was quite healthy. In the third case, which took place in 1877, gangrene of the foot followed the use of the bandage, and the application of a ligature; this was treated by amputation, and recovery took place. It is recorded in my book on the Practice of Surgery. At the amputation, which was performed below the knee, no artery required torsion or a ligature, as all were obstructed. In all these cases the bandage was applied lightly over the sac, and tightly again above it, so as to arrest completely all pulsation in the vessels. If I turn to the recorded experience of other surgeons, it is true that this observation respecting capillary ecchymoses has not been made; nevertheless, such must have occurred, and if it take place in the skin and superficial tissues over the sac, or in parts that are visible, is it not reasonable to conclude that the same ecchymoses take place in the deeper parts? And if they do the inference is clear, that by the employment of the bandage stagnation, and even extravasation, of the blood in the deeper tissues takes place, as well as in the superficial; and that by this means the risks of gangrene are greatly enhanced; for it is not open to dispute that it is through the capillaries of the limb that the collateral circulation is to become re-established, and that anything that interferes with them is likely to prevent such a circulation being perfected. In support of this view, I will refer you to the report of the only case on record in which death has followed closely upon this treatment, that of Dr. Weir, of New York, published in the *New York Archives of Medicine*, vol. iii. 1880, p. 207, the case of a man, aged 38, the subject of popliteal aneurism, on whom the bandage was applied first for two and a half hours, and three days later for four hours. The leg became cold, and threatened to become gangrenous, and the man died twenty-seven hours after the cessation of the compression. At the autopsy, minute capillary hæmorrhages were seen in the superficial, but especially in the deeper, parts. There was advanced fibrous phthisis, and the kidneys were intensely congested, but otherwise normal. From these facts I think the conclusion is inevitable that there is in the use of Esmarch's bandage for the treatment of aneurism a special danger of gangrene, and that this danger is not so much due to the sudden arrest of the circulation through the limb, such as is known to follow the Hunterian operation, but is due to the blocking of the capillaries, and consequent interference with the collateral circulation, upon which the subsequent vitality of the limb depends. How far this truth should tell against the use of the bandage, or suggest that it should be applied to the lower part of the limb when rendered anæmic by elevation, I am not now prepared to state, but of this I am sure, that it should suggest caution

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in its employment; caution as to the period for which it is applied; and caution as to the cases selected for its use. Indeed, I would venture to suggest, that in no case should the bandage be kept on continuously for more than two hours, and that in most, one hour should suffice. That in all cases when the bandage is removed, digital or instrumental pressure on the afferent artery should be substituted, and that in no case should the method be employed in which, by œdema or other signs, there is any evidence of the great vein of the limb being pressed upon or obstructed. It is true that this latter caution is one which should be applied to all cases of aneurism in which the treatment by pressure is considered; but it is particularly so in the method of treatment under consideration. In such cases the application of a well-prepared catgut ligature to the afferent artery, the use of antiseptic lotions and dressings, and the best endeavours to obtain quick repair of the wound is far preferable to any attempt to bring about a cure by pressure in any of its forms. One word more with respect to the possible connection between albuminuria and the employment of the bandage. In the fatal case of Dr. Weir, the kidneys after death were found markedly congested, and in one of my three cases acute albuminuria followed the cure thirty-seven days after the application of the bandage. These may be mere coincidences, but they are worthy of notice.

Professor Oliver Pemberton, Birmingham: I regret to have to submit to the Section a case of aneurism of the popliteal artery, in which the use of Esmarch's bandage has resulted in fatal gangrene. The patient, a man, 45 years of age, the subject of the disease for twelve months, was treated early in 1878 in the Birmingham Hospital. The bandage was applied standing, so as to distend the sac, and above all was cast the elastic tourniquet. At the end of an hour, when these were removed, pulsation had almost ceased, but the aspect of the aneurism was alarmingly livid, and ecchymoses were abundant on the tense integument. For twenty hours subsequently pressure was made at the arch, and the pulsation altogether ceased. Gangrene from the toes upwards—threatening from the first—was now apparent, and it was rapidly spreading; amputation through the thigh was performed, but this failed to save the patient, who sank a few days afterwards. The aneurismal sac was found unruptured, but there were extensive extravasations into the cellular tissue, and capillary ecchymoses generally around. The femoral vein was obliterated by the growth of the aneurism. It is evident that this mode of treatment cannot be advisable in large aneurisms, or where there is any serious venous obstruction; and, indeed, it cannot fail to be remarked that the entire tendency of this mode of treatment is to leave undeveloped the collateral circulation—which constitutes the basis of Hunter's treatment by ligature, and that of the Dublin school by pressure.

Local and General Treatment of Malignant Tumours of the Breast, and other External Parts.

Prof. Semmola, Naples.

Without discussing here to what degree carcinoma and sarcoma are purely local diseases, and to what extent constitutional causes influence their production, it is evident that methods of cure must be at the same time both local and general;

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that is to say, while on one side the neoplastic element must be destroyed, and the perverted direction of the local nutritive processes modified, on the other, the nutritive changes must be energetically stimulated and improved. Thus before electrolysis, I have always given large and increasing doses of iodide of potassium, from a gramme up to three grammes (15 to 45 grains) a day, continued for a very long time. I have a patient who has taken three grammes a day for six months.

I believe that the chief reason for the failures which have been met with up to the present time in treating malignant new growths in accessible situations, has been that the treatment has been exclusively limited to local destruction, without modifying the general state of the organism, or because the attempt has been made to obtain resolution merely by the general action of medicines. This is, perhaps, not impossible, but it is a futile attempt at present, with the drugs which we possess.

The six cases which I have collected in the ten months during which I have occupied myself with these experiments (in which I have been aided by my learned friend, Dr. Vizioli, Professor of Electro-therapeutics in the University of Naples), consist of one case of epithelioma of the right breast of the size of a large orange, fibro-sarcoma of the right breast, two sarcomata in the right breast, sarcoma in the left breast, and cysto-sarcoma on the upper third of the arm. In all these cases excepting one, the diagnosis was confirmed by eminent surgeons who had advised the removal of the tumour as rapidly as possible; and, in one patient, the case was that of a sarcoma produced eighteen months after another sarcoma had been removed from the same side. I know well how many doubts and errors may occur in the diagnosis of tumours, but in these cases, the best guarantees exist that the diagnosis was accurate and certain. In the first place, all the clinical characters were recognized by skilful colleagues, and microscopic examination was made in each case, in which I was assisted by one of our best professors of pathological anatomy, Dr. Petrone.

Apropos of this microscopical examination, I desire to submit a simple method for making one by the juice of a tumour. When a Ciniselli's needle is pushed into the centre of the tumour and withdrawn, a drop may be collected on the slide, more than sufficient for observation. In one case of very remarkable cure, I have been able to make preparations directly from portions of the tumour which were eliminated by a process of dissecting suppuration, and I have these preparations with me for those who care to examine them. The case was one of a spindle-celled sarcoma. The apparatus employed was that of Stöhrer with small elements, and that of Onimus modified by Professor Vizioli. The needles were those of Ciniselli, that is to say, well polished steel needles. The needle was thrust deeply, to the extent of from two to three centimètres, or even more, in proportion to the volume of the tumour; and its direction in all cases was towards the centre of the tumour. The punctures were in circles symmetrical to, that is to say, formed around the nipple, in the two cases in which the tumour occupied nearly the whole breast; and in the other cases, in which the tumours were of a more limited extent, the punctures were placed almost in a network. The needle must be introduced slowly, in consequence of the resistance of the tissue of the tumour, which sometimes calls for considerable force. After several punctures have been made, it may happen that almost insurmountable resistance is met with. This, no doubt, arises from points of sclerosis which have followed the inflammatory foci produced by the preceding punctures. In these circumstances, if the needle have already entered to a depth of from one to two centimètres, it is unnecessary to

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persist; but if the obstacle be superficial, it is better to withdraw the needle and commence the application at a distant point. The same course must be adopted when inflammation has followed the introduction of the needle, so as to render its reintroduction less painful. In five of my cases, it appeared to be sufficient to introduce one needle into the tumour, and this is always the negative pole. The positive pole should be applied to the skin, at a greater or less distance from the puncture, with a charcoal button covered with linen, to prevent the formation of superficial scars; the distance depends upon the size of the tumour. In general, the arrangement should be such that the current may traverse the whole of the neoplasm. The patient complains of a sensation of acute pain corresponding to the point of the needle, where it is often said that a tongue of fire seems to traverse the tumour, and sometimes the sensation invades the whole morbid tissue. Another sensation, which indeed corresponds to reality, is that which the patients describe by saying to the operator, "Oh! the bubbles! the bubbles!" and they indicate with the point of the finger spots at which air-bubbles appear to be forming deeply in the neighbourhood of the punctures. One may feel by the touch, in the neighbourhood of the needles, an emphysematous crepitation, which is doubtless due to the gases that have penetrated into the subcutaneous areolar tissue.

The duration of the application is from ten to fifteen minutes, according to the tolerance of the patient and the object which it is desired to attain. One thing well worthy of remark is that, during the whole time of the application, the needle of the galvanometer undergoes oscillations. This fact seems to indicate change of resistance presented to the passage of the current by the different layers of the tissue; and, secondly, resistance to the chemical action, which recommences more or less strongly. The duration of the treatment is sometimes very long. The electric current is not a knife, and it is in this relation especially that its slow modifying action of local nutritive acts may represent a therapeutical action preferable to that of the knife. In the cases which I have observed, the minimum duration has implied twenty-four electric applications made three times a week. But alongside of this minimum must be noticed another case, in which the treatment lasted eight months. This was the large epithelioma mentioned; it may even be affirmed that the treatment is not yet finished, for at the present moment the patient continues the application once a week. In the cases in which it is urgent to act, as I did in the case in which I plunged both needles into the tumour, one or two sittings at most suffice to determine sharp inflammation, quickly followed by suppuration. These cases are, I believe, extremely rare. In all the cases which I have observed, the state of the lymphatic glands surrounding the tumour was quite healthy. But, even if there had been some glandular engorgement, I think electrolysis should be employed all the same. Only in these cases, instead of the slow action, it would be necessary to act on the latter method—that is to say, to plunge both needles into the tumour, and to use a very strong current, in order to provoke an inflammatory action, which might be followed by dissecting suppuration—a means of cure which Nature exceptionally offers to us.

In this way I arrive at the following conclusions, which I have the honour to submit:—

1. The electrolytic treatment, applied to malignant tumours, such as epithelioma, fibro-sarcoma, sarcoma, is capable of curing them by one of three modes of action; that is to say—(*a.*) By producing small foci of inflammation, with consecutive sclerosis, the tumour is reduced to a minimum volume in relation to its primitive

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size, and there remains in its place a dense innocent induration like a *caput mortuum* (cicatricial tissue). (*b.*) By producing colloid and fatty transformation in the mass of the tumour, especially when it is in course of softening. (*c.*) By exciting inflammation with dissecting suppuration, breaking up of the tumour, and its elimination in pieces.

2. With the electrolytic treatment, I have employed iodide of potassium in large doses, administered with great perseverance, so as to modify profoundly the direction of the nutritive changes.

As I have already had the honour to say, I do not present this as a definitive and completed method. It is rather a problem, for which I have had the good fortune to be able to sketch out an encouraging preliminary formula, the distinct solution of which can only be given by you. I hope that you will be willing to fol low the path which I have indicated.

In the treatment of such diseases, the question comes at once to the lips, Will the tumour reproduce itself after a short time, and thus supply an example only of a new disappointment? I must reply that I do not know, but that it does not appear to me unjustifiable, looking to our biological observation of the action of modifying agents, to hope that it will not.

Recent Advances in the Surgical Treatment of Intra-peritoneal Tumours.

Mr. T. Spencer Wells, London.

The term recent advances, I think, may be considered as including the advances which have been made within the recollection of most of us—the improvements that have been made, the progress that has been secured during our own professional career, during the last twenty to thirty years. And I suppose almost every one present—certainly every one who has reached the age of fifty years—can remember a time when the removal of a tumour of any kind from the abdominal cavity was a surgical curiosity, exciting wonder, calling forth serious condemnation, and only encouraged by a very few of the most hopeful of our body.

Long after ovariotomy had risen from the period of distrust, and its mortality had been reduced below that of other operations of less magnitude—long after ovariotomy had secured the sanction of our Profession and the confidence of the public—other abdominal tumours were left to run their course. Occasionally, perhaps after a mistake in diagnosis, a uterine tumour was removed; sometimes designedly, but at first almost as a forlorn hope; then, after a time, with an amount of increasing success which has led to the admission of the amputation of uterine tumours, or of enlarged portions of the uterus, among those surgical operations which, if not yet thoroughly accepted, are still recognized as worthy of serious consideration.

Still more recently, excision of the whole uterus affected with cancer has been repeatedly accomplished, and Porro's operation is taking the place of the Cæsarean section. The removal of an enlarged spleen has now been effected in a sufficient number of cases to prove that it is more than a surgical possibility. Not only have cysts of the kidney been cured by drainage, and renal calculi removed, but the displaced, enlarged, or diseased kidney has been extirpated; gall stones have been

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removed from the gall bladder; suppurating hydatid cysts of the liver have been drained; peritoneal hydatid tumours have been excised; and, within the last few weeks, authentic reports have reached us of excisions of portions of the stomach, and of recovery after the removal of more than six feet of the small intestine. This has followed excision of smaller portions of intestine during ovariotomy: and a bolder course has been taken of late years in cases of obstructed intestine, of wounds or rupture of the bladder, and of extra-uterine fœtation. Many of these cases will be brought before you almost immediately by Mr. Lawson Tait, whose table of a very remarkable series of cases I have only received this morning, and have not yet had time to examine.

As some of these subjects have been reserved for discussion in this Section, and others in the Section of Obstetric Medicine and Surgery, I make no further allusion to them now, and pass on to refer to three points, on which I trust information will be gained in the discussion which will presently take place. They all appear to me to be intimately associated with recent advances in the surgical treatment of intra-peritoneal tumours, and in abdominal surgery generally. These three points are: 1. The union of divided edges, or separated surfaces, of peritoneum; 2. The use of pressure-forceps in preventing, or stopping, or diminishing the loss of blood during and after operations; 3. The practice of drainage in relation to antiseptics.

1. I should not have thought it necessary, after the progress which has already been made in abdominal surgery, to insist at any length upon what I believe now to be a fundamental principle or rule of practice, accepted and adopted by all the most experienced and successful operators at home and abroad, if I had not been fiercely attacked by that singular society, which, admitting the right of man to kill all the lower animals, to use them for food, their skins for clothing, their feathers for ornament, to make them labour for our convenience, denies our right to use them for any scientific purpose, or in the hope of benefiting the human race. These curious philanthropists persistently attack me because I sacrificed a few rabbits, dogs, and guinea pigs, in the year 1859, in order to demonstrate, beyond any doubt, for all time to come, the fact that human life may be saved, and lifelong suffering prevented, by an improved mode of uniting penetrating wounds of the abdominal cavity. They argue that results observed in animals are no guide to what may happen in man; that my observations were not new, and that some believe they are not true; that some think ovariotomy has, on the whole, rather shortened than lengthened the life of woman; and that its mortality has not been lessened by the knowledge gained by my experiments on animals. Of course, I have not taken any notice of these attacks; but I wish to show you exactly what I did, and leave you to judge whether or not the human race has been served by the death of about a dozen animals twenty years ago. Here are the specimens, which I have been permitted to bring from the Museum of the College of Surgeons. Before I hand them round, let me tell you exactly what led me to make the experiments. The first three women upon whom I performed ovariotomy all recovered. The fourth died, without, as I thought, any good reason why she should have died. I was naturally much interested in the post-mortem examination, and I obtained the invaluable service of my friend, Dr. Aitken, to make it. We found that the state of the inner surface of the wound was far from satisfactory. At that time, instead of the sutures I have used of late years, I used harelip-pins and twisted sutures to unite the wound; and we saw at once that some of the pins on the inner aspect of the abdominal wall were bare; the cut edges of the peritoneum were retracted; a portion of intestine was

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in contact with the wound, the impress of which was obvious on the surface of the gut. Some coagula of blood, and an abundant, consistent, lymphy exudation upon the peritoneal surface of the intestines corresponded to the edges of the incision and the surface of the wound. Recent lymph glued the opposing surfaces of the intestines to each other. I saw at once how much better it might have been if the peritoneal edges had been accurately brought together, and had united; and thought of doing this in my next case. But I found careful instructions in text-books and treatises to avoid the peritoneum, just as some few surgeons, even now, fear that they may set up peritonitis, or that pus from the track of the sutures may get into the peritoneal cavity. So I determined rather to make the experiment *in corpore vili* than on women. Dr. Richardson kindly helped by narcotizing the animals, some with puff-ball and some with chloroform. In these specimens, where the divided edges, or rather surfaces, of peritoneum have been pressed together, you see that the smooth, serous inner coat of the abdominal wall is perfectly restored. You cannot see the stitches on the inside, though plainly visible on the skin; and there is no adhesion of intestine or omentum. When skin or mucous membrane are divided their edges must be brought together to secure direct union. If they be inverted, union is prevented. The exact opposite holds good with serous membrane. The edges should be inverted, and two surfaces of membrane pressed together, so that the sutures are not seen; and the effused lymph makes so smooth a surface that even the line of union cannot be seen. In these specimens you can only discover it by the line of sutures in the skin. But mark the contrast in these other specimens, where the peritoneal edges were purposely excluded from the sutures. In every case where the animal was not killed within a day or two, intestine or omentum adheres to the inner surface of the abdominal wall, thus completing the peritoneal sac, at the great risk of intestinal obstruction, or of permanent discomfort to the survivor; to say nothing of a want of firm union and subsequent ventral hernia. One can hardly understand the so-called reasoning which insists that this should not be demonstrated once for all in a few narcotized animals, but that we should grope our way to the realization of the truth through the sufferings and by the dissection of a series of women. You can examine the specimens now, and they will remain in our College Museum, and will serve, I trust, to dispel any doubt which may still be felt as to the best mode of uniting penetrating wounds of the abdominal wall. And I will go still further, and say that, whenever edges or surfaces of peritoneum are divided or separated, they should, if possible, be reunited. Peritoneum must be opposed to peritoneum; and this can only be done by drawing it over the pedicle, or stump, or divided parts, and fixing or uniting it as an envelope by a line of sutures, or by the uninterrupted suture. I have already urged this so persistently in my works on diseases of the ovaries; in the Hunterian Lectures at the College of Surgeons, in 1878; in a paper on the removal of uterine tumours, brought before the British Medical Association last year; and in some recent papers, that I will at once pass to another improvement which has added to the success of abdominal surgery, namely:—

2. The use of pressure forceps—forcipressure, as it has been termed. I believe I was the first to replace the old "snip," or "clip," or "bull-dog," so easily lost in the abdomen, by larger instruments. Mr. Nunneley arranged for a more permanent occlusion of bleeding vessels by tubular forceps; Kœberlé and Péan contrived the "pinces hémostatiques," which I now show in different shapes and sizes, as well as the smooth, powerful, nickelized instruments, which I prefer. They are here of

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various sizes, straight, and bent at different angles. The smaller are useful in almost any surgical operation, for the temporary stopping of bleeding before ligature, for torsion, or for the suppression of bleeding permanently by crushing together the coats of the bleeding vessels. The larger ones are very useful as means of making an operation almost bloodless—a sort of substitute for Esmarch's bandage, or the elastic ligature, where these cannot be applied. I have found them especially useful when removing uterine tumours. They may be applied as temporary clamps before amputating the growth; and, after this has been cut away, they may be removed one after the other, and all bleeding vessels at once secured by ligature. This may appear to be a little thing, but it stands for much in the sum of daily surgery; it comes in to aid at every moment, and is of almost universal application.

3. The question of antiseptics and drainage is one of far greater difficulty. On the general question of antiseptic surgery I must not enter. Another day, in this Section, is specially devoted to this great subject. But the question of drainage, in relation to antiseptics in abdominal surgery, is so curious that I must devote a few minutes to it before concluding. What appears to me so remarkable is, that while in general antiseptic surgery drainage is so very essential—is, indeed, a fundamental part of the system—in my own experience of ovariotomy, and of the removal of uterine tumours, antiseptics have abolished drainage. I have not used a drainage tube for more than three years. Formerly, in cases where much fluid was likely to collect in the abdominal cavity after operations, drainage was necessary. If the fluid did not escape, it putrefied, and poisoned the patient: or had to be removed by puncture, or by reopening part of the wound. Since adopting antiseptic precautions, either fluids do not form—or, if they do, they do not putrefy—and they are absorbed without doing any harm, without leading even to any febrile rise of temperature. I do not say that I have not seen one or two fatal cases, where it might have been better if I had drained—or that, in one or two, part of the wound has not reopened, and permitted the escape of some fluid—but I do repeat, I have not used a drainage-tube for more than three years; and my present feeling is that the case must be very exceptional indeed in which I shall venture to use one. I trust we shall hear from Mr. Thornton, and others, who have more experience of drainage than I have, what they have learned from this practice. Professional opinion on this point has still to be settled; but I think forcipressure may be accepted as unreservedly as union of peritoneal surfaces in all cases of removal of abdominal tumours; and I feel sure that, by careful attention to all needful antiseptic precautions, these operations may now be undertaken with a far more confident expectation of a successful result than could have been reasonably entertained a very few years ago.

Recent Advances in Abdominal Surgery.

Mr. Lawson Tait, Birmingham.

The great success which has been achieved in the removal of ovarian tumours, since the improvements effected in the operation by Dr. Keith, has of necessity led to a much wider extension of abdominal surgery. Perhaps the first effort in this direction of a striking kind was made by Dr. Marion Sims, in his operation of cholecystotomy, performed in Paris, in 1878, which, however, had an unfortunate

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ending. His attempt emboldened me to perform the same operation, in August, 1879, with a perfectly successful result, the patient remaining up to the present time in excellent health. The case has been published in detail in the "Transactions of the Royal Medical and Chirurgical Society for 1880."

The principle of the operation, as advised by Dr. Sims, was the opening of the cavity of the gall-bladder, the removal of its contents, and the stitching of the edges of the cyst to the edges of the parietal wound by continuous suture, and the subsequent drainage of its cavity. In my case, a large impacted gall-stone was removed from the dilated bile-duct; and that the organ was fully restored to its function was shown by the regurgitation of bile from the wound during the whole time occupied by its healing. So satisfactory a case induced me to apply the same principle in other directions, and I have done so in five cases of hydatids of the liver, and one case of large cystic abscess of the same organ. In none of them was there any adhesion between the liver and the abdominal wall.

I opened the abdomen over the most prominent point of the tumour; then opened the cavity of the cyst, and cleared out its contents completely; the next step was to stitch the edges of the wound accurately by continuous suture, to the edges of the abdominal wound, and to fasten in a drainage tube. Every one of these patients made a rapid recovery, and their health is now everything that could be desired.

In six cases of cystic disease of the kidney I have adopted a proceeding in every respect similar, save that in two of them the tumours have been multicystic; and in them I broke down the walls between the cysts, and drained them into a common cavity.

In cases of operation upon the liver I have not been troubled much with hæmorrhage, and it has always speedily yielded to pressure, either by the use of Kœberlé's forceps, or the application of a sponge. The only case in which I had any anxiety on this score was in one of the cases of multicystic tumours of the kidney, where I feared I should have to leave a sponge in the cavity for some time, a proceeding which I do not think would be accompanied by any particular risk; and although I have never yet had it to do, I have more than once had to contemplate its possibility in troublesome cases of hæmorrhage from pelvic adhesions in ovariotomy. What I should do under such circumstances would be to stuff the cavity full of sponges, and close the wound over it by sutures tied in a double bow-knot, so that it could easily be reopened, in the course of three or four hours, for the removal of the sponges.

I have three times opened the abdomen for the purpose of removing the spleen, but in none of the cases were the conditions such as to warrant me in proceeding with the operation. One of the cases, operated upon in February last, was not, I believe, an ordinary case of splenic enlargement, but probably a case of abscess in the centre of the enlarged organ; but whether this abscess was or was not in existence before the abdominal section, about three weeks after, it became quite evident that there was pus in the organ, so I proceeded to operate as in the other cases, and emptied 2¼ pints of pus from it. Then I fastened in a drainage tube, and the patient is now so far recovered that she has gained flesh very considerably, is able to get up occasionally, and will, I think, ultimately make a perfectly satisfactory recovery. The spleen is about half the size it was previous to the abdominal section.

In the "Transactions of the Royal Medical and Chirurgical Society" for 1880, I have described a method of dealing with pelvic suppuration which is quite

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analogous to the operations I have described upon the gall-bladder, the liver, and the kidneys; and up to the date of that paper I had treated six cases upon the following principle. I opened the abdomen in the middle line, in the usual manner, and after having ascertained the most prominent point of the abscess cavity, I completely emptied it by means of the aspirator, washed it out, then enlarged the incision for about 1½ inches to 2 inches, then stitched its margins to the margins of the abdominal wound, so as to completely close the peritoneal cavity, and, finally, I fastened in a glass drainage tube. All these patients made recoveries far more rapid and more complete than if I had opened the abscess by the vagina; and in the majority of cases—certainly in four of them—it would have been perfectly impossible to have reached the suppurating cavity from the vaginal canal. Since that date I have operated upon six other cases, and in all but one the result was eminently satisfactory. In that case the patient suffers from what is perfectly well known as "recurrent hæmatocele;" that is to say, at intervals in her menstruation she has effusions of blood into the broad ligament; this breaks down and suppurates, so that she has to go through the whole trouble of a pelvic abscess, and this she has done some seven or eight times. On the last occasion, I submitted her to the proceeding I have just described, and I further advised that at the same time her ovaries and Fallopian tubes should be removed, and to this both she and her medical attendant, Dr. Drummond, of Birmingham, gave a ready consent. She is a widow, has never borne any children, and for years has led a wretchedly invalid life. At every fresh attack there can be no doubt that she runs great risk of her life. When I opened the abdomen, I failed to find either the ovaries or Fallopian tubes; but I found a large parovarian cyst, which I could not remove. I laid this freely open, and I dealt with her suppurating hæmatocele as described, with perfect success; for within four weeks of the operation she was able to get up and go about. Within only the last few days her menstruation came on; she had a fresh pelvic effusion, which has again suppurated, and has burst into the rectum; and Dr. Drummond and myself are both of opinion that the chances of her recovery from this last attack are extremely small. It is to me a matter of great regret that I was unable to arrest her menstruation by removing the uterine appendages.

In addition to these cases, what seems to me to be another advance in abdominal surgery is to be dealt with in the cases of diseases of the Fallopian tube. Many of these were very well known years ago, but very few efforts are on record of attempts for their surgical treatment. Removals of Fallopian pregnancies were not very successful until within the last ten years, and since that time a very fair number have been recorded. Of these, I have myself published six, with only one death, though even in that case the child was saved, and is still alive. The principles of the operation are exactly the same as those already laid down. In addition, however, the rule of leaving the placenta absolutely untouched, first laid down with sufficient emphasis by Kœberlé, must be acted upon. In the first case in which I operated, I was unfortunately tempted to make use of the vaginal section, and I removed the placenta, with the result that my patient died in a few hours. Where I have operated by abdominal section, and left the placenta alone, I have had five recoveries out of six cases; and in that sixth case recovery would, I think, have been likewise effected if the patient had not delayed the operation until she was quite moribund.

Collections of fluid in the Fallopian tube, have, in a few instances, been operated upon, when they were large enough to attract attention by their appear-

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ance as tumours. But in a large number of cases, the patients' lives are endangered, or at least they are rendered intensely miserable by small collections of fluid, either serous or bloody, or purulent, enclosed in the Fallopian tubes when they are occluded at both extremities. Some years ago I operated upon two cases of hæmato-salpinx, one of which recovered, and still remains in good health, but the other proved fatal. Both of these cases have been published. Since 1877—that is since the new departure made by abdominal surgery, I have operated in twelve cases of collections of fluid in the Fallopian tubes, causing intolerable pain. In every one of these cases the patients had wandered about from hospital to hospital, or from surgeon to surgeon, seeking relief in vain; and, if the preparations of some of them, now in the museum of the Royal College of Surgeons, be examined, it will not be difficult to see why they could not be relieved. In all of them I opened the abdomen, and either removed the tubes and ovaries affected, or I drained the tube in the manner already described. The tubes were removed in eleven cases, and I had recourse to drainage in one of them. The success of these operations was most satisfactory, for in every one of them recovery was complete.

We have here, therefore, a collection of forty-four operations which would have been regarded five years ago, not only as most serious, but almost wholly indefensible and impracticable; and yet, amongst them all, there has been but one solitary death, and in not a single instance have the secondary results been anything but entirely satisfactory. In very few of the cases was the Listerian method employed, and in those in which it was, it seemed to do more harm than good. In my own practice it is entirely discontinued, and I am pleased to learn that the experience I have had of it, and the predictions I ventured upon two years ago concerning it, have been completely substantiated by so great an authority as Dr. Keith, who tells me he has given it up because he has had two deaths from carbolic acid poisoning from it, and that he has now performed forty-six operations for the removal of large ovarian tumours, without Listerism, and with only one death. Since I gave it up, I have performed thirty-one consecutive completed operations without the use of any carbolic acid, and I have not lost a patient.

Another of the unquestionable advances in abdominal surgery is the treatment of uterine myoma, by removal of the uterine appendages. Since May, 1879, I have performed this operation twenty-two times with two deaths, and one death in another uncompleted operation. In all the recoveries the arrest of the hæmorrhage has been complete, with one exception, a case already published, which died of cancer about six months after the operation. Even in this case the hæmorrhage was arrested for three months. In most of the cases the tumours are disappearing, and in three they have almost entirely vanished. For success in this operation it is necessary to remove the Fallopian tubes as well as the ovaries, according to the experience I have had so far. Here then is a list of sixty-six serious operations, with three deaths (1-22), or about 4·5 per cent. mortality.

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Modification of Dr. Péan's Operation for the Removal of the Uterus.

Dr. DE ZWAAN, The Hague.

The subject of my communication is an account of the removal of a large interstitial fibro-myoma of the uterus, by Dr. Péan's method.

The modifications introduced in this method are:—

1.—An abdominal incision is made in the belly large enough to allow of the tumour being easily drawn out of the abdominal cavity without any injury to the peritoneum.

2.—The abdominal cavity is immediately but temporarily closed.

3.—The tumour is surrounded with an elastic band.

4.—The temporary stitches are replaced, one by one, by permanent peritoneal and surface sutures.

The purpose and aim of these modifications are:—

1.—To protect the organs of the abdominal cavity against a sudden chill.

2.—To obviate all risk of the bowels being forced from their position.

3.—The prevention of hæmorrhage.

On Excision of the Pylorus.

Professor CZERNY, Heidelberg.

I will only detain the Section a few minutes, while I refer to one of the most recent advances in intra-peritoneal surgery, and one which the sceptics still regard with a good deal of suspicion; I mean excision of the pylorus. I am the more desirous to do so, not only because I have performed the operation myself on a patient, whom I can at all events for the present describe as cured, but because I have the kind permission of Professor Nicolaysen, of Christiana, to mention a case which was operated upon by him.*

Professor Nicolaysen's patient was a woman thirty-seven years of age, who had suffered since last summer from pain in the region of the stomach, which at last became so severe, and the vomiting so constant, that she was completely deprived of sleep. On May 17, 1881, excision of the pylorus was performed, the rule laid down in Wölfler's work on the subject being strictly followed during the operation. It lasted two hours and a half, although no difficulties were met with, the tumour being, fortunately, freely moveable. But unfortunately the strength of the patient had been too much reduced, and she died collapsed, fifteen hours after the operation.

My case was that of a man, aged twenty-eight, who had suffered from symptoms of disease of the stomach for fifteen weeks, and who for the last ten weeks had been so reduced by the vomiting, in spite of the methodical use of the stomach pump, and careful feeding, which, however, gave him temporary relief, that his weight had been reduced to ninety-four pounds. Still, he was able to get about, and to this circumstance alone I attribute the good result of the operation. The photo-

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* Partial resection of the pylorus was first performed experimentally on animals, then on the human subject, by Péan, of Paris; the first successful results were obtained by Billroth, and his assistant, Wölfler.

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graphs give a very good idea of the condition of my patient: An enormously distended stomach, the great curvature of which reached actually to the pubes, and which contained a tumour in the neighbourhood of the pylorus as large as a child's fist, tender to the touch, and changing its position considerably, according to the greater or less distension of the stomach. An incision was made, four inches long, in the middle line; the strictest antiseptic precautions were taken, and the suture employed was my own "zwei reihige Naht" (double-row suture), antiseptic silk being used. Although all the positive signs of cancer were present, I was not quite certain of the diagnosis, as the patient was so young; and accordingly, although I considered the evidently-incurable stenosis of the pylorus a sufficiently good indication for the operation, I drew out the stomach, and made an exploratory incision into it; after which, on introducing the finger, I found the entrance to the duodenum completely obstructed by hard, knobby excrescences, which seemed to confirm the diagnosis of carcinoma.* The operation lasted more than two hours. The wound healed without a sign of inflammation, the temperature only reaching 38° C. (100 F.) on the first evening, and remaining afterwards absolutely normal. There was no vomiting and no pain. The patient took some soup on the second day, meat on the fifth, and got up on the twentieth. In the next fourteen days he gained eleven pounds in weight, and has now returned to his usual occupation.

On looking at the completely simple course of this case towards a cure, I would maintain that excision of the pylorus ought not merely to be confined to the cases of cancer of the stomach, but that a great future is opened up by it for those cases of simple stricture of the pylorus, in which an incurable dilatation of the stomach is found. And this is an argument already put forward by Gussenbauer and Winiwarter.

I would further point out, that the fact that such a tedious operation, and one in which forty ligatures and sixty silk sutures were left in the wound, was followed by no symptoms of inflammation, is another proof of the truth of the teaching of Spencer Wells; so that, if operations on the abdomen are antiseptically performed, drainage is not absolutely necessary. I have used antiseptics rigidly in all my cases of abdominal section, now over a hundred in number, and have only employed drainage in them when, from the foul nature of the contents of the tumour, from bleeding, or from some other cause, it was doubtful whether a completely aseptic condition had been attained; and I have reason to be content with the results, as out of my first fifty completed ovariotomies I only lost five. The case may be different in those operations in which an extensive opening is made from without inwards, into the floor of the pelvis, as in extirpation of the uterus. In these cases the best antiseptic results will be obtained by drainage and irrigation.†

DISCUSSION.

Dr. Marcy, Boston: I arise to describe certain modifications in the mode of performing abdominal section which I have recently put in practice. At the outset, let me say, my gastrotomies have all been done under the most careful antiseptic precautions. I would give my last case of gastrotomy in illustration. Mrs. S., aged 44, a semi-

* It was a cancer with small alveoli, starting from the submucous tissue, involving the muscular coat, but leaving the mucous membrane intact.

† I have quite recently heard by letter from Professor Billroth that the patient operated upon by Dr. Wölfler in Vienna four months ago, is, at the present time, well and free from any recurrence.

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invalid for years from abdominal trouble, and confined to bed nearly entirely for the last six months from pain and extreme emaciation, has menstruated slightly once or twice during the year. The operation was performed upon the 19th of June last. An abdominal incision of about seven inches was sufficient to expose and lift out a multi-lobular fibroid tumour involving the entire uterus. Further examination showed cystic degeneration of both ovaries. I placed below both ovaries and tumour a double turn of rubber tubing, which I tied tightly, as a temporary means of controlling hæmorrhage. Then with a needle, without a cutting point I tied the pedicle by the so-called shoemaker's stitch, with a single antiseptically prepared tendon of the Caribean cariboo. The advantage of this ligature is that, taken from such a large active animal, it possesses parallel fibres sufficiently long, which are very strong, and are, I think, much more reliable than the catgut ligature. The stitches were inserted very closely together, and were ten or twelve in number. The amputation was made by a double flap, and the peritoneal edges were carefully approximated by a continuous suture. The temporary modified Esmarch bandage was removed, and the string returned to the abdominal cavity. There was only a little venous hæmorrhage from the tumour itself upon removal. The abdominal wall was closed as in ovariotomy. Hot water was kept circulating in a rubber coil, sufficiently large for the body of the patient to lie upon, in order to lessen shock during the entire operation. Only for a few hours during the next two weeks did the temperature exceed 100° Fahr.; it was usually about 99°. And almost the only suffering which supervened was caused by a mild cystitis. A daily hot-water rectal douche, continued for half an hour, was given by means of one of my double rubber tubes, in order to avoid gaseous distension and fæcal accumulation in the rectum. The great dangers to be feared in these very severe abdominal operations would appear to be shock, hæmorrhage, and septic peritonitis. Thanks to the labours of Professor Noeggerath, of New York, and others, it may be believed the first is very largely dependent upon the rapid abstraction of heat from the exposure of the large peritoneal surfaces. This can be compensated by heat artificially applied. The second, hæmorrhage, now seems the difficult factor. My belief is, that this may be most safely controlled by a ligature carried double through one opening made by an instrument without cutting edges. This also allows of only one knot, thus reducing to the minimum the greatest danger from the animal ligature. The rubber cord previously applied renders the operation in a large degree bloodless. Again, I would first carry my tumour through a central opening, cut in a thin sheet of pure rubber, and thus entirely prevent the introduction of blood or cystic fluid into the abdominal cavity. The advantage of the close approximation of the peritoneal surfaces, leaving no open wound shut within the abdominal cavity, still further lessening the danger from bleeding. The temporary compression by the elastic band does not devitalise the tissues, and, as I believe, I have histologically demonstrated, in relation to my operation for the cure of hernia, the animal fibres of the ligature are incorporated into, or replaced by, living tissue. The third great danger, that of septic peritonitis, is certainly reduced to a minimum, if not entirely avoided, by strict antiseptic attention and care.

Mr. Knowsley Thornton, London: I agree with Mr. Wells as to the importance of thoroughly closing the peritoneum, and also as to the use of the pressure forceps. The disadvantages of the employment of drainage I consider are the following:—

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1. The danger of admission through the tube of infective material. 2. The danger of removing serum and blood from the peritoneum in an exhausted patient after a severe operation. If the peritoneal contents are aseptic they are rapidly absorbed, and feed the patient. 3. The tube leaves a weak place in the wound, through which ventral hernia is very apt to take place. The slow healing of the tube-opening prolongs convalescence, and exposes the patient to the additional risks of an open wound. Drainage is then something to be avoided. Can we avoid it by perfect asepsis? I reply, from my own experience, that we can. Then arises the question, can we obtain perfect asepsis? And again, I reply yes, undoubtedly. Why is drainage so necessary in ordinary wounds treated antiseptically? Because the tissues in which they are made do not readily absorb, and such absorbents as are present are injured and crippled by the operation, but the peritoneum is one large absorbing sac, and only a very small portion of it is injured by our operations. In other wounds nerve tension comes into play. But tension is impossible in the peritoneum, and if the fluids we leave are aseptic it is quite capable of taking care of them without injury to the patient. Passing from this subject I would earnestly invite the attention of the Congress to the possibility of rendering operations perfectly aseptic, when the uterine cavity has to be opened in the removal of large uterine tumours. If we can once overcome this difficulty, I believe operations for removal of the uterus or uterine tumours will be as successful as ovariotomy.

Dr. A. Martin, Berlin: Most German gynæcologists, and especially those of Berlin, adhere to the Listerian system, and I have not seen any instance of fatal consequences from its use. All cases of carbolic intoxication recovered after a short period of disturbance, and, moreover, our success is such as persuade us to continue its use. Schröder, out of his last 67 cases (they are amongst his third hundred), had but three deaths. I myself have employed the antiseptic system in 1 splenotomy (recovered), 8 kidneys (5 recovered), 63 ovariotomies (50 recovered), 7 salpingotomies (6 recovered), 6 Freund's operation (1 recovered), 4 extra-uterine pregnancies (1 recovered), 21 sub-serous tumours, among which were 9 supra-vaginal amputations (11 recovered), 4 enucleations after laparotomy (2 recovered). Then 4 cases I operated upon in a special manner. They were cases of intra-mural tumours. I opened the abdomen and took out the tumour, put on an Esmarch's elastic band round the neck, opened the space in which the tumour lay, enucleated it, and after cleaning it, closed the uterine wound; then removed the elastic band, and cleaned the peritoneum, and closed the abdominal cavity. One patient was almost moribund from septicæmia before I removed the tumour, which was already sloughing; the other died in the fifth week of peritonitis. As to the remarks of Mr. Spencer Wells, I believe it is of great importance to unite the peritoneal wound accurately, especially in the case of the uterus and intestines. It is wise in cases of nephrotomy to unite the peritoneal edges if they do not fall closely together after removing the kidney. I do not think the pressure forceps are always applicable; as, *e.g.*, in attempting to compress the vessels in the neck of the uterus, such forceps are apt to injure the peritoneum, are not easily fixed, and do more harm than good. It is easy to avoid hæmorrhage. I prefer Esmarch's elastic band, or the use of silk ligatures. Drainage I only employ when I am obliged to leave sloughing parts in the abdominal cavity, or when the peritoneum has been as much injured as is unavoidable in vaginal extirpation of the uterus by ligatures. Then I drain

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through the vagina, and I find that the large tube falls out spontaneously from the fifth to the eighth day, and the wound generally heals well.

Dr. THOMAS KEITH, Edinburgh: I have for a long time drained by a glass tube placed at the lower end of the incision. I believe that drainage is essential to obtain the best results in very bad cases. In these, where bleeding could not be stopped, or when the patient was too feeble to admit of waiting till oozing should cease, it seems to me that drainage saves many a case. Sometimes the secretion, when the wound is closed, is very large, and surely it is better to remove this than to allow fluid to stagnate in the pelvis, and throw the absorption of it on a feeble patient. Then drainage helps convalescence enormously. If properly used, the tube not too large, and not kept in too long, no after inconveniences need be feared. When I began to use carbolic spray I tried to do without drainage, but for long have gone back to it. For some time I have not found the carbolic spray necessary, and have not used it in my last twenty-seven cases, all of whom have recovered easily. With every possible care, the spray has not in my hands prevented the mildest septicæmia, and its effects on the kidney were sometimes disastrous. I have frequently seen kidney hæmorrhage follow long operations, and two deaths in hospital patients were occasioned, I believe, by carbolic acid poisoning. Though I had at one time a series of eighty recoveries under the spray, I have reluctantly given it up, believing that on the whole it did more harm than good.

Dr. D. NELSON, Chicago, believed that shock and hæmorrhage were two of the great dangers of abdominal surgery.

1. Shock, with all its accompanying symptoms, was explained by irritation and injury applied to the sympathetic nerves and ganglia. The speaker suggested that, in order to prevent shock, the following precautions might be taken:—To expose as little of the peritoneum, and for as short a time as possible, to any source of irritation, and to avoid including the Fallopian tubes when practicable. The spray may have some effect in causing shock. Such medicinal agents should be selected as are least likely to produce secondary effects, and opium especially should be very sparingly employed.

2. Hæmorrhage may be diminished by employing temporarily Esmarch's band. He preferred carbolized French silk for ligatures, and applied them by means of a blunt needle; the ends of the ligature are passed from opposite sides through a single opening in the tissues made with the needle. One end is introduced with the entrance of the needle and then unthreaded, then the other end is threaded on the same needle, which is then withdrawn. Then, passing the entering thread over and under the exit thread, a knot is tied sufficiently firmly to stop bleeding, but not to crush the tissues. If the stump is very thick he recommended the "shoemakers' stitch," passed from serous membrane to mucous membrane, if the cavity of the uterus has been widely opened; and later from serous membrane to serous membrane to close the uterine cavity; and, last of all, the peritoneal wound is closed by the over-and-over stitch.

Professor VOLKMANN, Halle, pointed out how, in abdominal surgery, a truly antiseptic result might be obtained, even if the spray were discarded, if hands and implements were first purified, and primary union of the abdominal incision were

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obtained; but if drainage were employed, the septic element would certainly be introduced if antiseptic dressings were not employed. He illustrated his remarks by comparisons with similar facts, which may be observed in the treatment of amputations or other surgical wounds.

Dr. J. Marion Sims, New York: The papers just read mark an era in the progress of Surgery, and will lay the foundation for still greater results. Mr. Lawson Tait's paper is hardly open to criticism, or discussion. We have simply to accept the teachings of his experience as he has presented them. Of Mr. Spencer Wells's admirable paper, I may venture to say a few words. I was not aware till now that any one, in this enlightened day, thought it unnecessary to unite the severed edges of the peritoneum made in abdominal sections. I thought that the profession everywhere accepted Mr. Spencer Wells's experiments as conclusive, and his teachings on this subject as final. We have the best of reasons why we should unite the severed edges of the peritoneum. For clinical experience, independently of Mr. Wells's well-directed philosophic experiments on the lower animals, demonstrates most conclusively the importance of this procedure. If the cut edges of the peritoneum are not included in the sutures which are to close the abdominal incision, we shall necessarily have a raw surface, more or less long according to the length of the incision, and of varying width, a half-inch or more, according to the distance of the sutures from the cut edges of the peritoneum. Nature protects the peritoneal cavity from the dangers of this raw surface (which would otherwise granulate and suppurate) by its immediate union with the subjacent parts. If it unites with the omentum, well and good: but if with the intestine, there is sometimes danger. For, if the adhering portion of intestine should happen to be convoluted in such way as to obstruct the bowel, then serious and even fatal results may follow. This has happened, and, therefore, we should guard against the possibility of such an accident. Besides this, clinical experience furnishes us with another forcible argument why the divided edges of the peritoneum should always be carefully brought together by suture. Some years ago I had a case of ovariotomy, in which it was found on removal of the sutures that the peritoneal edges of the incision were firmly united, while there was no union at all between the edges of the wound leading down to the peritoneum. Now in this case if I had failed to unite the edges of the peritoneum, the wound would have remained open through its whole extent down to the peritoneal cavity. The wound here healed by granulation from the bottom, and was eventually closed by a firm cicatrix. In two other cases that died from septicæmia (one after Battey's operation), the same thing occurred. The only union in each was that of the divided edges of the peritoneum, while the abdominal parietes gaped widely open when the sutures were removed. The post-mortem in each case showed, that but for the union of the divided edges of the peritoneum, there would have been no union whatever of the parietal section. Do not these facts go far to prove the importance of bringing the divided edges of the peritoneum together in closing the abdominal incision by suture? As to drainage, in abdominal surgery, I believe it to be of vital importance under certain conditions. In general surgery no one pretends to dispute its value. Listerism and drainage came into use about the same time. They seem to be mutual helpers. Indeed, drainage is the handmaid of Listerism. Chassaignac was the first, I believe, to prove the value of drainage. He was the first to sound the alarm of septicism, and at the same time to designate a preventive by means of his *tubes*

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à drainage. When I left Paris in August, 1870, at the head of the Anglo-American Ambulance, Chassaignac walked with me from the Palais de l'Industrie to the Depôt of the Northern Railroad, talking all the time about the importance of drainage in all wounds, whether gun-shot or surgical, and insisted that I should give it a trial in my new prospective field of labour. Accordingly, when at Sedan, I spoke to my colleague, MacCormac, on the subject, but unfortunately we had no facilities for trying it till our ambulance was completely septicized. The teachings and practice of Chassaignac are now accepted everywhere. And the name of this great French Surgeon seems to be forgotten in this relation. But the time must soon come when full justice will be done to the honoured name of Chassaignac. In amputations, it seems that complete drainage is of vital importance; more important even than antiseptic precautions. If I were obliged to give up one or the other in general surgery, I would not hesitate a moment to sacrifice antiseptics. If, then, drainage is so important in general surgery, why should we be so afraid of it in abdominal sections? I do not share the fears expressed by some on this point. In abdominal surgery we have a large and active absorbing surface. Some patients will survive the absorption and elimination of a large amount of septic serous fluid; while others will succumb rapidly to the absorption of a small quantity. There is no special danger in introducing a glass tube into the peritoneal cavity through the lower angle of the abdominal incision. For Nature protects the peritoneum by sacculating the tube completely, by the exudation of lymph which immediately becomes organized. If there is no bloody serum to drain off, the tube may be removed in a few hours, and no harm is done. But if there is something to be drained it soon makes its appearance at the open end of the tube and is readily absorbed by sponges placed to receive it. My friend Spencer Wells no longer uses drainage tubes in abdominal surgery. But I think he may occasionally find cases in which it might be useful. The tube is now excluded on the theory that Listerism renders the peritoneal effusion aseptic, and, therefore, that its absorption will not be attended with risk to life. But is this always so? I fear not—and let me illustrate this uncertainty by a case, one that is by no means unique. In December, 1878, I assisted my friend Spencer Wells with an ovariotomy in one of the suburbs of London. The case was altogether a very bad one. Knowing well its difficulties and dangers, he had to put off the operation till at last it was obliged to be done to save the life of the patient. Adhesions in the bottom of the pelvis were universal and strong. When the external wound was closed, Mr. Wells saw that there was some bloody exudation going on, but supposing that Listerism had rendered this aseptic he had no fears for the result. The patient went on well enough for thirty-six hours, but after that, fears were felt for her safety. Fortunately at this period of anxiety bloody serum was found exuding from the lower angle of the wound. And Mr. Spencer Wells removed the two lower sutures, and opened the wound sufficiently for the free discharge of the septic bloody serum, and the patient made a rapid recovery. Now I do not pretend to say that the patient would necessarily have died if the accidental discharge had not so positively pointed out the method of immediate relief to urgent symptoms. All this poisonous fluid might possibly have been absorbed and eliminated; but was there not great danger in waiting for Nature's effort in this direction? But of this I am sure—If the drainage tube had been used, this fluid would have been drained off, as it was extravasated, and there would not have been the least cause for alarm for the safety of the patient.

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The only objection that can be urged against the drainage tube is, not in any immediate danger, but in its ultimately predisposing to the production of ventral hernia. And this is a serious objection, one that we who advocate its use must learn to obviate. Some one must work out this problem, and I am sure that it can and will be done.

Dr. BANTOCK, London: confining his observations to the treatment of fibroid tumours of the uterus, said, that though various measures had been used in the treatment of these cases, both medical and surgical, he was of opinion that there were certain cases which called for surgical treatment by abdominal section as the only appropriate one. He would content himself with relying on the authoritative exposition of the disadvantages of Listerism, and the value of drainage, given by Dr. Keith. Ovariotomy having cleared up the other steps of the operation, the most important question was the treatment of the pedicle. Mr. Wells claimed as one of the most recent advances in this operation, the treatment of the pedicle by ligature in a particular way, but he would venture to say that the method was of limited application. When the pedicle was small and of little vascularity, the ligature might be relied on, but when thick, fleshy and very vascular, it was not to be trusted. The reason for this was to be found in the nature of the uterine structure; for there were no proper blood-vessels, and from the peculiar combination of muscular cell, yellow elastic, and white fibrous tissues, there is so much yielding of these tissues that in a few hours the stump is found to be bleeding freely. A few years ago, Mr. Wells published a very interesting case of this kind, with a successful result, but only because the pedicle was kept outside. Referring to his own practice, he had lost all the cases he had treated by the intra-peritoneal method—four in number, chiefly from hæmorrhage, while all the cases treated extra-peritoneally recovered. The method he employed was by Koeberle's *serre nœud* aided by transversely placed pins to support the pedicle. The value of this method was confirmed by the results of the practice of Professor Hegar, of Freiburg, Dr. Wahl, of Dorpat, and others. He was convinced that not one of the five cases of recovery in his practice could have been treated by the method advocated by Mr. Wells. His first case, three years ago, was treated in this way, but a small quantity of blood escaped from between the flaps, and the patient died of septicæmia. The conclusions at which he arrived were these:—1. That, as a rule, pediculated tumours are alone suitable for surgical treatment by abdominal section. 2. That when the pedicle is small and of little vascularity, the ligature may be relied on with the greatest confidence. 3. That when the uterine body is not involved, and the pedicle is very short and thick, it is not possible to stitch the peritoneum over the raw surface; that the raw surface can only be covered by a flap amputation of the pedicle. 4. That when the pedicle is thick and fleshy, and of great vascularity, the extra-peritoneal method offers the best hopes of success, and that this is especially true of those cases in which the greater part of the uterine body is involved.

Dr. A. DUNLAP, Springfield, Ohio: The subject of abdominal surgery, now before the Section, is one of peculiar interest to me. Ever since I ventured in September, 1843, to perform my first ovariotomy in opposition to the then received opinion of the Profession, I have given it my careful attention. In that and the six following cases, it was peritonitis that I feared, as a result of opening the abdominal cavity. They were all fearfully adherent to the walls of

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the abdomen and omentum, and consequently a considerable quantity of blood was discharged into the cavity. These clots I determined to remove at all hazards to prevent inflammation; in doing so I dried the cavity of all its fluids. The first one recovered so far as to be free from all dangers of the operation, but soon died from disease of the kidneys; the other six made complete recoveries. The eighth case was a young girl in good condition. I found a simple cyst, free from all adhesion, with a long pedicle. I made an opening not more than 2½ inches in the peritoneum, tapped the cyst, withdrew the pedicle and ligated it, and closed the wound. There had been no hæmorrhage that had escaped into the cavity, and by keeping the cut edges of the wound in close contact with the sac, no air had entered the peritoneum. Here, thought I, is a case that will give me no trouble; but, to my astonishment, my patient began to sink, and died the fourth day. What was wrong in the operation? was the question that immediately came to mind. In analyzing my operations, it became evident that cleansing the abdominal cavity of all its fluids was as important as the removal of blood-clots. And to-day I consider the important point in the success of ovariotomy to be the thorough cleansing of the abdominal cavity. Neither spray nor drainage tubes can take its place. I look upon them as temptations to close the abdomen, leaving them to do the work that we should have done when we were tired, and anxious to get through such a fearful surgical procedure. I believe in antiseptics. I had been using a weak solution of carbolic acid for some years before the use of the spray, to cleanse my wounds in surgical operations. I have never favoured the spray. I did not believe in the theory that the germs floating through the atmosphere were the source of putrefaction and decomposition. That form of surgical interference brought before the Section by Mr. Tait is well worth their consideration. In 1869, I had a case which exhibited the wonderful tolerance with which the peritoneal cavity bears wounds. I was called to a young man, aged twenty, robust constitution, and of uninterrupted good health. He had fallen on a rapidly revolving shaft, just where it was fastened by a half-inch iron bolt, which projected about an inch. His clothing caught, and held him tightly against it, whilst the revolving bolt cut the wall of the abdomen and stomach, drawing it out of the cavity, and making a wound 5 inches long in the wall, and 4½ inches long in the stomach. It occurred just after he had eaten a hearty meal. I saw him about an hour after the accident. There was no appearance of shock in his pulse, nor of pain in his countenance. The stomach was partly drawn out of the wound, and a very considerable portion of the left lobe of the liver. The stomach was partially emptied of its contents, but none had entered the abdominal cavity; the food that remained was just as he had eaten it. The bleeding had entirely ceased, and there was trickling, from the rent in the stomach, a perfectly clear fluid. In about another hour I had chloroform administered, and then took the stomach up, in order to empty it completely, but the chloroform sickened him, and he made efforts to vomit. I could feel the vermicular motion of the stomach, as I held it, the pyloric orifice approaching towards the cardiac. The muscular contractions gaped the wound, and the pyloric orifice was forced out until it came plainly to view, the stomach being almost completely turned inside out. After several efforts at vomiting, the contents of the duodenum began to come in jets. It was of that dark bilious colour, which we see in vomiting from an empty stomach. After the vomiting ceased, I cleansed the stomach, and accurately stitched the edges of the wound in that organ to the edges of the peritoneum of the wound in the wall of the abdomen, by closely applied uninterrupted sutures, and then closed the

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wound. He came from under the chloroform without any appreciable shock. For thirteen days the pulse never rose above 90, and there was only a slight increase of temperature. There was no pain nor tenderness over the abdomen. The peritoneum united rapidly, and the stitches came away, leaving a healthy granulating surface. But the patient was rapidly weakening. The food taken was sufficient to nourish him, and was digested, as I could see it through the opening remaining in the walls, but it was forced out through this opening by the action of the stomach, and not down into the duodenum. He remained bright and cheerful, with all these symptoms, with the exception of nourishment, till the thirteenth day, when he had a large watery stool; his pulse immediately rose from 90 to 120, and then to 140. In six hours he had another evacuation, and died in a few minutes, in collapse. A post-mortem examination was refused. The man certainly did not die from inflammatory action, but from rapid exhaustion, and the collapse caused by the large watery stool. The extensive surface of the stomach that had become adherent to the walls of the abdomen prevented the vermicular motion from passing the nourishment into the duodenum.

Dr. Heywood Smith, London: The point under discussion seems to be the salvation of life in cases that involved abdominal section. It is necessary to weigh well the relative value of Listerism on the one hand, and skill combined with extreme care, and occasionally combined with drainage on the other. No doubt the rapid healing of the abdominal wound is a great element of success, but cases arise where mischief supervened even where the abdominal wound healed well. We must be careful how we ascribe success to any particular method of treatment, lest there should be some fallacy in our argument. It by no means follows because such brilliant operators as Keith, Lawson Tait, and Bantock obtain good results without Listerism, that Listerism is useless—it was rather that their success was due to their large experience and the most scrupulous care which such experience had given them. Facts speak better than theory, and it is a fact that at the Hospital for Women in this city, since they had used antiseptics the mortality in ovariotomy had greatly diminished. In my first case with antiseptics no spray was used, but carbolic water was poured into the cavity of the peritoneum, and the patient then was turned on her side, and the fluid emptied out of her. No doubt the use of antiseptics enabled us to do away with the drainage tube to a great extent, though we should never tie ourselves down to a hard-and-fast line with any method of treatment. I consider that grumous exudation, to a greater or less extent, is the fatal factor in most cases of septic peritonitis—that the use of antisepticism renders such exudation innocuous; and should symptoms of septicæmia arise, it would be better to re-open the abdominal cavity, rather than use a drainage tube. With regard to the condition of the uterine contractile tissue, as referred to by Dr. Bantock, I would mention that in a case where I removed the uterus at the neck for malignant disease of the fundus, and where the patient died because she was alcoholic, the stump pushed itself up through the double ligature, which had transfixed it, until it assumed the form of a cone, with its apex towards the abdominal cavity. I consider that Listerism enables unskilled operators to save life which would otherwise be sacrificed, and therefore it is not to be lightly discarded.

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Ueber Nierenexstirpation.

Prof. Czerny, Heidelberg.

Hochansehnliche Versammlung! Als das hochgeehrte Comité mir den Auftrag ertheilte, die Debatte über die Chirurgie der Niere einzuleiten, verdankte ich diese Ehre lediglich dem Umstande, der Nachfolger *Gustav Simons* auf der Heidelberger Lehrkanzel zu sein, des Mannes, den wir mit Recht den Begründer der modernen Nierenchirurgie nennen dürfen. Leider gestattet es mir die Kürze der zugemessenen Zeit nicht, dieser Aufgabe in dem Umfange gerecht zuwerden, wie sie es verdiente, und wenn vielleicht meine Worte einige Beachtung finden sollten, so verdanke ich es dem glücklichen Zufalle, welcher mir Gelegenheit gab, auf dem von Simon so erfolgreich betretenen Wege weitere Erfahrungen zu sammeln.

Es kann hier nicht meine Aufgabe sein, die Diagnostik der chirurgisch wichtigen Nierenkrankheiten ausführlich abzuhandeln, da ja dieselbe mit den epochemachenden Arbeiten von Bright und Rayer so vielfache und ausgezeichnete Bearbeitung gefunden haben, dass es schwer sein dürfte, etwas Neues hinzuzufügen.

Wie Sie, hochverehrte Herren, aus den von mir zur Discussion vorgeschlagenen Thesen ersehen, sind es besonders zwei operative Fragen für deren Besprechung ich mir Ihre Aufmerksamkeit erbitten möchte. Ich meine die Nephrectomie und die Behandlung cystischer Nierengeschwülste.

Die Besichtigung der Nierenexstirpation kann wohl nicht mehr ernstlich discutirt werden, nachdem ich aus der Literatur allein schon 72 Fälle in der hier vorliegenden Tabelle zusammenstellen konnte, von denen die grosse Mehrzahl wegen einer Indicatio vitalis unternommen worden ist. 13 dieser Fälle, davon 6 mit tödtlichem Ausgange, gehören meiner eignen Praxis an. Ich habe für die Tabelle das Schema des Herrn Barker benützt, welche alle wesentlichen Momente berücksichtigt. Von diesen 72 Fällen, zu denen noch zwei nach persönlichen Mittheilungen des Herrn Martin kommen, sind 32 nach der Operation gestorben, die Uebrigen geheilt oder auf dem Wege der Heilung begriffen.

Die erste Patientin, welche nach der Nephrectomie genesen ist, der Fall Simons, starb, nachdem sie acht Jahre in ungetrübter Gesundheit gelebt hatte, an Phthise. Bei der Section war das Herz nicht vergrössert, dagegen die vorhandene Niere bedeutend vergrössert. Es entspricht wohl der Häufigkeit der Phthise überhaupt, dass unter meinen 13 Operirten 2 schon bei der Operation tuberculös gewesen sind und einer nach der Genesung von derselben phthisisch geworden ist, wenn wir nicht annehmen wollen, dass Individuen mit mangelhafter Urinsecretion leichter phthisisch werden als ganz Gesunde.

Zur Besprechung der Frage, ob Leute mit einer Niere eine kürzere Lebensdauer zu erwarten haben, als solche mit zwei Nieren, ist die Dauer und Zahl der Beobachtungen noch zu kurz. Fast möchte man jedoch diese Frage bejahen, wenn man bedenkt, dass nach der Zusammenstellung von Reimer (Virchow's *Archiv*, 72 Band) unter 48 Fällen von rudimentärer oder fehlender Niere die vorhandene 22 mal meist an chronischer Entzündung erkrankt war. Die Möglichkeit, dass nach Exstirpation einer Niere die übriggebliebene in höherem Grade zur Erkrankung geneigt ist, kann desshalb nicht ganz von der Hand gewiesen werden und ist jedenfalls bei der Indicationsstellung zu berücksichtigen.

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Dass der Mensch jedoch nach Exstirpation einer Niere in voller Gesundheit alle Funktionen verrichten kann, beweist mir am besten eine Frau, welche ich am 22. Mai 1879 in sehr elendem Zustande wegen Pyonephrose operirt habe. Dieselbe concipirte schon im August desselben Jahres, abortirte am 12 November, 1879 und gebar im Dezember 1880 ein vollkommen entwickeltes Kind, welches sie jetzt noch bei strotzender Gesundheit selbst stillt. (Dr. Bauch.) Es wäre sehr wünschenswerth zu erfahren, was aus den älteren Fällen von Gilmore, Brandt, Campbell, Marvaud geworden ist, da die aus der neueren Periode der Nephrectomie (nach 1878) stammenden Fälle noch zu jung sind um uns über die mögliche Lebensdauer der Operirten Neues zu lehren.

Als oberste Bedingung für die Zulässigkeit der Nephrectomie muss die Gesundheit der zurückbleibenden Niere gelten. Von den 32 Todesfällen sind 7 mal (v. Bruns, Lange, Merkel, Bardenheuer, Czerny 2), eine Erkrankung oder Mangel der anderen Niere als Todesursache angegeben. Ich selbst begegnete einmal, bei dem Versuche eine vereiterte Steinniere zu entfernen, einer Hufeisenniere und hätte natürlich die Operation nach Erkennung dieser Sachlage nicht vollenden können, auch wenn keine tödtliche Blutung aus der Vene renalis dazwischen getreten wäre. In einem anderen Falle exstirpirte ich eine rechtseitige Hydronephrose, woran der Mann an completer Anurie 37 Stunden nach der Operation starb. Bei der Section war die linke Niere durch einen kleinen Fettklumpen vertreten, die Ureter obliterirt. Und dennoch schienen alle Zeichen dafür zu sprechen, dass die linke Niere gesund sein dürfte; der Urin wurde quantitativ und qualitativ normal secernirt, es waren nie Schmerzen oder sonstige Beschwerden der linken Nierengegend vorhergegangen.

Leider giebt es kein Zeichen, welches mit absoluter Sicherheit die Gesundheit der zurückzulassenden Niere beweisen würde. Alle diagnostischen Hülfsmittel; absolute Unempfindlichkeit, normale Percussions und Palpationsverhältnisse der gesunden Nierengegend, wenigstens zeitweise normale Urinsecretion, ja selbst der probatorische Bauchschnitt, lassen blos die Wahrscheinlichkeits-Diagnose zu. Der Katheterismus der Ureteren nach Dilatation der weiblichen Harnröhre von G. Simon, die Abklemmung der Ureteren von Trichmann sind zu schwierige und zu eingreifende Methoden, um am Krankenbette Verwendung zu finden. In einem zweifelhaften Falle von Pyonephrose führte mich der probatorische Lendenschnitt in die kranke Niere zur vollständigen Sicherheit, indem nach Ableitung des Eiters aus der kranken Niere der, aus der Blase secernirte Urin, vollkommen normal wurde.

Wenn im Ganzen dennoch verhältnissmässig häufig nach der Operation die andere Niere gesund gewesen ist, so liegt es in der Wahrscheinlichkeit, dass eine Niere wenigstens noch normal secerniren muss, wenn die andere so erkrankt ist, um die Exstirpation zu erheischen und dennoch die Kräfte des Menschen so gute sind, um ihm ein derartige Operation zuzumuthen.

Was nun die specielleren Indicationen der Nephrectomie betrifft, so unterliegt es nach den Erfahrungen von Brandt, Marvaud, und Cartwright wohl keinem Zweifel, dass die durch eine Lendenwunde vorgefallene Niere entfernt werden soll, wenn sie entweder so verletzt ist, dass ihre Reposition gefährlich erscheint, oder wenn sie schon auffällige Zeichen der Entzündung und Gangrän darbietet. Schwieriger ist die Indication bei subcutanen Zerquetschungen der Niere. Ich möchte auf dieselben nicht mehr eingehen, da ich den ausführlichen Erörterungen Simons aus neueren Erfahrungen nichts Wesentliches hinzuzufügen weiss.

Bei Wanderniere ist die Exstirpation von Martin (7), Smith (1), Merkel(1), Martin

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(1), und Langenbuch (2), also im Ganzen 12 mal ausgeführt worden. Davon endeten 4 tödtlich, 3 mal in Folge von Peritonitis, einmal wegen Erkrankung der anderen Niere; während in den anderen Fällen nicht nur Genesung von der Operation, sondern auch Heilung der Symptome, wegen deren sie vorgenommen worden ist, mitgetheilt wird. Unzweifelhaft hat Martin durch seine glänzenden ersten Erfolge bei Lendenniere zu dem raschen Aufschwunge, welchen die Nephrectomie seit 1878, besonders in Deutschland gewonnen hat, sehr wesentlich beigetragen. Allein wir werden dennoch heute, nach dem einmal Todesfälle constatirt sind, bei dieser an und für sich nicht lebensgefährlichen Erkrankung die Indication möglichst einschränken müssen. Die Aufgabe der Zukunft muss es jedenfalls sein, die durch ihre Beweglichkeit lästige Niere durch chirurgische Mittel zu fixiren, nicht aber das normal funktionirende Organ zu vernichten. Bekanntlich hat schon Smith in New Orleans durch ein Haarseil, welches er durch die bewegliche Niere zog, temporäre Linderung der Beschwerden erzielt. In neuester Zeit hat Hahn in Berlin die Niere durch den Lendenschnitt blosgelegt und mit Nähten in der Wunde fixirt. Ob durch diese Operationen oder vielleicht durch Einspritzungen von Alkohol in das paranephritische Zellgewebe bei ruhiger Rückenlage Heilung der lästigen und auch palliative Mittel oft nicht zu beseitigender Symptome der Wanderniere gelingen wird, kann erst die Zukunft entscheiden.

Pyonephrose und Stein der Niere gaben 19 mal Veranlassung zur Operation; 7 mal war Stein entweder in der exstirpirten Niere vorhanden oder von dort sicher der Pyonephrose vorausgegangen und musste als wesentliche Ursache derselben angesehen werden. Ausserdem wurde jedoch 3 mal die Diagnose auf Stein gestellt, ohne dass sich ein solcher in der exstirpirten Niere gefunden hätte. Unzweifelhaft muss es als die vollkommene Methode angesehen werden, wenn es gelingt das Nierenbecken anzuschneiden und den Stein zu entfernen. Allein in meinem Falle, dem einzigen bei welchem die Nierensubstanz noch normal war, gelang es mir erst den Stein zu entdecken, als ich die Niere enucleirt hatte.

Es ist mir auch zweifelhaft, ob man es wagen soll bei nicht ausgedehntem Becken in der Tiefe der Lendenwunde die Incision zu versuchen, da die Blutung kaum beherrscht werden könnte, wenn man das Unglück hätte grosse Nierengefässe dabei zu verletzen.

Bei der Unsicherheit ob die andere Niere gesund ist, empfiehlt es sich freilich wohl immer zunächst eine Nierenbeckenfistel anzulegen, weil man aus den dann eintretenden Veränderungen des Urins wichtige Schlüsse über die Beschaffenheit der anderen Niere ziehen kann. Freilich ist es bei dieser Methode sehr schwierig den aseptischen Wundverlauf bei der späterfolgenden Nephrectomie zu erzwingen.

Wenn nach Eröffnung einer Pyonephrose die Gesundheitsverhältnisse des Patienten nicht bald besser werden, ist die Exstirpation der Niere indicirt, da diese Kranken sonst fast immer an der langwierigen Eiterung zu Grunde gehen. In der That sind von 14 Pyonephrosen blos 4 nach der Operation gestorben und davon 2 wegen Erkrankung der anderen Niere (v. Bruns, Lange), einer wegen Eröffnung der Pleura bei der Operation (von Dumreicher) und der vierte an Shock. Von 5 Operationen, welche wegen richtig erkannter Nierensteine vorgenommen worden sind starben 3, 1 an Pyaemie, und bei den 2 anderen war die Exstirpation der sehr verwachsenen Niere nicht vollständig gelungen.

Ich rechne nach meinen drei Erfahrungen die Nephrectomie wegen Pyonephrose und Stein zu den segensreichsten Operationen, die wir überhaupt besitzen, indem

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wir dadurch, einen kranken Menschen dem elendsten Siechthum vollständig entreissen können.

Wegen Cysten und Hydronephrosen ist die Nephrectomie zwölfmal, und zwar siebenmal mit tödlichem Ausgange, unternommen worden.

Es gehören hieher begreiflicher Weise die ersten Versuche der Nephrectomie, da die Operation allein 5 mal versucht wurde, in der Meinung eine Eierstockcyste zu entfernen. Es ist desshalb nicht zu wundern, dass die Mortalität so gross war. Uebrigens habe ich in meinen vier Fällen die cystische Nierengeschwulst vor der Operation richtig erkannt und dennoch drei Todesfälle erlebt. Im ersten Falle sollte ich eine colossale Hydronophrose durch die Incision behandeln, erkannte jedoch nach Eröffnung der Cyste, dass sie mit malignen sarkomatösen Wucherungen an der Innenfläche besetzt war. Ich entschloss mich desshalb sofort zur Exstirpation des sehr fest verwachsenen Sackes, eine Operation zu welcher die Kräfte des leider phthisischen jungen Mannes nicht mehr hinreichten. In einem Falle von Nierencyste war die andere Niere granulös entartet, und war wohl die disponirende Ursache, dass der Tod an Pyaemie 48 Tage nach der Operation erfolgte. In einem dritten Falle fehlte die andere Niere vollständig und der Mann hatte aus einer einzigen hydronephrotisch erkrankten Niere ganz normalen—Urin in normalen Mengen secernirt.

Es scheint gerade bei cystischen Erkrankungen nicht selten auch die andere Niere zu leiden, und wenn man auch nicht mehr die Exstirpation cystischer Nierengeschwülste für unausführbar halten darf, so wird man doch mit der Indication zurückhaltend sein müssen, und wenn man sich dennoch für Operation entschliesst, beim medianen Bauchschnitt vorher durch Palpation sich von der normalen Beschaffenheit der anderen Niere zu überzeugen suchen, oder doch besser vorher die Incision der Cyste der eigentlichen Exstirpation vorausschicken, wie es Spiegelberg gethan hat.

Bei festen Tumoren der Niere sollte man eigentlich noch schlechtere Resultate erwarten, da die Diagnose öfters gemacht werden kann, wenn die Geschwulst nicht nur die Kapsel, sondern auch das Mesenterium durchbrochen hat, oder wenn schon Metastasen vorhanden sind. Insofern als diese Complicationen, welche eigentlich jeden operativen Heilversuch verbieten müssten, niemals mit Sicherheit ausgeschlossen werden können, werden die Exstirpationen von Nierengeschwülsten bis zu einem gewissen Grade Versuchsoperationen bleiben. So lange wir aber keine anderen Mittel besitzen, werden wir zu operativen Versuchen in dieser Richtung um so mehr berechtigt sein, wenn wir erfahren, dass von 13 derartigen Nephrectomien doch sechs einen schlimmen Ausgang genommen haben. Drei von diesen Operation (Kocher, Mynter, Czerny) blieben unvollendet und wären sicher unterlassen worden, wenn man die Schwierigkeiten hätte voraussehen können. In drei anderen erfolgte der Tod an septischer Peritonitis (Kocher, Czerny, Bardenheuer). Was die Recidive betrifft so werden wir, so wie bei der Operation maligner Geschwülste überhaupt die Erwartungen nicht zu hoch spannen dürfen. Der schöne Fall von Jessop starb acht Monate nach der Operation an Recidive. Die Kranken von Martin and Lossen sind jedoch über zwei Jahre nach Operation noch gesund, was ein sehr erfreulicher Erfolg genannt werden muss. Meine zwei Fälle (beide waren sehr grosse gefässreiche Sarkome) sind noch zu frische, um ein Urtheil in dieser Beziehung zuzulassen. Allein wenn ich bedenke, dass der erste ein 53 jähriger Herr, den ich am 22. April, d. J. operirt habe, vor der Operation ganz kachektisch und ikterisch ausgesehen hat, dass er vor der Operation jegliche Nahrung erbrochen hat, und dass

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derselbe zwei Monate nach der Operation die Klinik in blühendem Zustande verlassen hat, dass die Zahl seiner rothen Blutkörperchen, mit dem Malassez'schen Apparate gezählt, auf das Doppelte (von 2½ auf 5 Millionen in Cub. millimeter) gestiegen war, so muss man zugeben, dass ein ähnliches Resultat gegenwärtig durch kein anderes Mittel, als durch die Nephrectomie erzielt werden kann.

Wegen Ureterfisteln, sei es, dass dieselben in der äusseren Bauchwand, in dem Uterus, oder in der Scheide mündeten, wurde die Operation bisher fünfmal, davon viermal mit bestem Erfolge (Simon, Zweifel, Czerny, Credé), einmal mit Unglück (Lefort) ausgeführt. Obgleich Simon mit dieser Indication die Nephrectomie glänzend inaugurirt hat, and obgleich die Gefahr der Exstirpation einer normalen Niere bei einem sonst gesunden Menschen unter antiseptischer Wundbehandlung sehr gering erscheint, so gilt doch für diese Indication dasselbe, was ich oben über die Exstirpation der Wanderniere gesagt habe. Es ist schade, dass wir nicht immer im Stande sind, diese sehr lästige, aber nicht lebensgefährliche Krankheit mit Erhaltung der normal secessirenden Niere zu heilen. Vorschläge and Versuche existiren ja genug, aber sie scheinen sehr häufig unsicherer und gefährlicher zu sein, als die Nephrectomie. Auch in meinem Falle war die Patientin ungeduldig, und wollte von langwierigen und zweifelhaften Operationsversuchen nichts wissen, so dass ich mich zu der Entfernung der Niere entschliessen musste, wodurch dann auch die Kranke in drei Wochen hergestellt wurde.

Endlich wären noch 9 Nephrectomien zu erwähnen, die unter keine der obengenannten Rubriken fallen; zunächst die Fälle von Gilmore, Durham und Peters, die wegen schmerzhafter granulöser Nieren, zum Theil wohl im Verdacht auf Stein, operirten; Spencer Wells und Billroth, welche genöthigt waren, die mit einer fibro-cystischen Geschwulst untrennbar verwachsene Niere zu entfernen und endlich Bardenheuer, welcher bei Uteruscarcinom, welches den Ureter ergriffen hatte, so dass seine Verletzung bei der Exstirpation nicht zu umgehen war, sofort die betreffende Niere entfernte. Diese Operationen sind durch die Mangelhaftigkeit unserer Diagnostik wohl entschuldigt, geben jedoch keine eigentliche Indication für die Nephrectomie ab.

Wie aus dieser Zusammenstellung zu ersehen ist, hat die Nephrectomie schon bei Verletzungen, Wanderniere, Pyonephrose, Nierensteinen, Cysten und Hydronephrosen, Tumoren und Ureterfisteln schöne Erfolge gefeiert und sind sicher noch bessere zu erwarten, wenn die Diagnostik noch weitere Fortschritte macht. Da aber die Diagnose noch immer viel zu wünschen übrig lässt, so ist ein zu heroischer Eingriff sicher erst dann gerechtfertigt, wenn das Leiden nach Erschöpfung aller medizinischen Hülfsmittel, das Leben des Kranken in Gefahr bringt, oder doch unerträglich macht.

Bezüglich der Methode der Nierenexstirpation darf ich mich wohl kürzer fassen. Darüber, dass der Wundverlauf bei den glatten Fällen von Nierenexstirpation unter strenger Anwendung der antiseptischen Wundbehandlung sehr viel einfacher und gefahrloser ist, als bei nicht antiseptischer Behandlung, kann bei vorurtheilsfreier Würdigung der einzelnen Fälle kein Zweifel sein. Allein ich muss aus eigner Erfahrung bekennen, dass es gerade bei dieser Operation ungemein schwer ist, einen vollkommen aseptischen Verlauf zu erzielen, besonders wenn die Niere durch entzündliche Prozesse schon vor der Operation erkrankt war. Nur selbst bei normalen Wandernieren sind drei Fälle von tödtlicher Sepsis vorgekommen. Es muss desshalb bei dieser Operation für möglichst sorgfältige Ableitung der zersetzungsfähigen Wundsecrete d. h. für gute Drainage Sorge getragen werden. Das gelingt aber viel leichter bei dem

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Lumbalschnitt, als bei dem medianen Bauchschnitt. Schon aus diesem Grunde ist im Allgemeinen der Lumbalschnitt vorzuziehen, selbst wenn die Statistik welche mir für den Lendenschnitt nur 36 per Cent. (14 von 38) für den Bauchschnitt aber 58 per cent. (18 von 31) Mortalität ergab, nicht auch für diese Argumente sprechen würde. Da jedoch der von Simon angegebene Lendenschnitt besonders bei untersetzten Menschens selbst mit Resektion eines Rippenstückes nur wenig Platz bietet, so bin ich allmählig zum schiefen und endlich zu einem queren Lendenschnitt übergegangen, welcher fingerbreit unter dem Rippenbogen vom Sacrolumbalis bis zur linea semicircularis Spigelii reicht und sämmtliche breiten Bauchmuskeln, mit Ausnahme der Transversus mehr oder weniger quer durchschneidet. Ich würde diese ohnehin sehr verletzende Schnittführung nicht gewagt haben, wenn wir nicht durch versenkte Catgutnähte die tieferen Muskellager für sich zusammennähen könnten, so weit, dass für dicke Drainröhren in der erforderlichen Zahl genügend Platz bleibt. Bei voller Seitenlagerung und Unterstützung der Lendengegend durch ein dickes Rollkissen kann man auch bei gedrungenen Menschen bequem mit der ganzen Hand unter den Rippenbogen eindringen, und auf diese Weise kopfgrosse, festsitzende Nierentumoren heraushohlen. Bei dieser Schnittführung trifft man auch bei grossen Geschwülsten, immer auf das Colon und nach vorne auch die Umschlagsstelle des Bauchfelles. Um diese Gebilde zu vermeiden, muss man nach ihrer Bloslegung stumpf progressirend nach einwärts vom Colon auf die Nierenkapsel vordringen und bei ihrer Auslösung das Colon und Bauchfell nach vorne verschieben. Den vorderen Bauchschnitt möchte ich nur für die Entfernung sehr beweglicher Nieren reserviren. Er ist gewöhnlich in der Mittellinie gemacht worden, nur Hüter führt denselben über die Geschwulst und Langenbuch an der Aussenseite des graden Bauchmuskels.

Ob bei der Nephrectomie die Fettkapsel mit genommen werden soll, hängt von dem Falle ab. Bei bösartigen Tumoren ist es sicher wünschenswerth, auch die Fettkapsel mitzunehmen, bei der Exstirpation normaler Nieren wird man am leichtesten die Niere aus der Fettkapsel aushülsen und bei chronisch eitriger Entzündung sind alle Umhüllungen so fest verwachsen, dass man die erweichte Nierensubstanz aus der fibrösen Hülle herausgraben muss. Dabei geht es nicht ohne parenchymatöse Blutung ab, die übrigens gewöhnlich keinen bedrohlichen Charakter annimmt.

Die Blutung kann in den Kapselgefässen, aus der zerissenen Nierensubstanz, aus den Stielgefässen, und endlich aus naheliegenden, fest verwachsenen, grossen Gefässen (der untern Hohlvene) entstehen und war in 5 Fällen die directe Todesursache, während sie in 4 Fällen sicher an dem unglücklichen Ausgange wesentlichen Antheil hatte. Auch Spätblutungen am 6. (Meadows) bis 12. Tage (C. Lucas) und bei Abgang der Stielligaturen wurden beobachtet, jedoch blos in dem ersten Falle mit tödtlichem Ausgange. Ich halte desshalb die sorgfältige Abbindung aller blutenden Gefässe während der Operation für durchaus nothwendig. Für den Stiel, welcher am besten in der Mitte durchstochen und in zwei Portionen dann nochmals in toto unterbunden wird, empfiehlt sich für diesen Zweck als verlässlichstes Material meine antiseptische Seide. Wenn er sehr dick ist, muss man ihn vor der Ligatur mit einem Ecraseur zusammenschnüren.

Wenn wir die Todesursachen zusammenfassen, so starben 12 an Peritonitis, Septicaemie, oder langwieriger Eiterung, dazu kommen wohl noch mehrere Fälle von den 5, bei welchen eine genaue Todesursache nicht angegeben ist; 9 starben an

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Shock oder Haemorrhagie, 4 an Lungen und Pleuraaffectionen. Wir lernen ja noch tagtäglich die Hauptfeinde der Chirurgen, Septicaemie und Haemorrhagie mehr und mehr bekämpfen, es wird sicher gelingen, auch für die Nierenexstirpation diese Todesursachen immer seltener zu machen, auch mit der Zunahme unserer Erfahrungen auf diesem Gebiete, und wo auch die Fälle, welche sich bei der Operation als unausführbar bewiesen haben, immer seltener werden.

Nur wenige Worte lassen Sie mich noch über die Behandlung cystischer Geschwülste hinzufügen. Die Sache scheint mir so weit entschieden zu sein, dass ich es nicht für nöthig hielt, Sie mit casuistischem Material hier zu langweilen.

Die einfache Punction mit dem Dieulafoy'schen Aspirateur unter antiseptischen Massregeln, ist nach meiner Erfahrung, absolut ungefährlich und ist zu diagnostischen Zwecken und zur temporären Erleichterung des Patienten oft durchaus nöthig.

Bei einfachen Cysten mag manchmal die Jodinjection in die entleerte Cyste dauernde Heilung herbeiführen. Für Hydronephrosen und solche Cysten, welche der Jodinjection widerstehen, ist, wenn sie gefährlich werden, die Eröffnung und Drainirung am besten in 2 Zeiten, wie es Volkmann beim Leberechinococcus gemacht hat, durchaus zu empfehlen. Unter strenger, antiseptischer Cautelen wird man wohl auch die einzeitige Eröffnung und Vermehrung des Cystensackes mit der Haut oft ungestraft ausführen dürfen. Ich habe inder ersten Weise zweimal Hydronephrosen ohne jegliche Reaction eröffnet und als Beweis, dass solche Menschen mit künstlicher Nierenfistel in voller Gesundheit alle Thätigkeit ausüben können, möchte ich auf einen Mann verweisen, der von G. Simon vor 10 Jahren wegen Hydronephrose in seiner Art mit Doppelpunction operirt worden ist, und der seit dieser Zeit ohne jeglichen Schaden für die antiseptische Thätigkeit der chirurgischen Klinik in Heidelberg dort als Krankenwärter fungirt.

Nur für die schlimmsten Fälle würde ich, unter den oben angegebenen Cautelen, die Exstirpation für gerechtfertigt halten. Wenn man mir dagegen vielleicht meine eigenen vier Operationen vorhalten würde, so kann ich darauf nur antworten, dass wir leider durch Schaden klug werden.

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TABELLE ALLER FÄLLE VON NEPHRECTOMIE.

No.	Operateur und ursprünglicher Bericht	Geschlecht und Alter	Diagnose oder Zweck der Operation	Alter des Leidens	Datum der Operation	Sitz des Schnittes	Art des Verbandes	Behandlung des Stieles	Symptome nach der Operation	Folgen, unmittelbare und mögliche	Hämorrhagie bei oder nach der Operation	Beschaffenheit des entfernten Organs	Bemerkungen
1	Wolcot (America) *Med. and Surg. Rep.*, Phil., 1861, vol. vii.	Männl. 58	Geschwulst der Leber	6 Jahre	1861	—	—	—	—	Tod nach 15 Tagen in Folge von Erschöpfung und Eiterung	—	Encephaloid der rechten Niere, Gewicht 2½ Pfund.	
2	Peaslee (America) "Ovarian Tumours," p. 158	Weibl.	Schwellung des Eierstocks	—	1868 April	Medianer Bauchschnitt	—	—	Keine Schlafsucht. Freie Entleerung des Urins	Tod 50 Stunden an Peritonitis	—	—	Der ursprüngliche Bericht ist sehr dürftig, indem er wenig mehr als diese Angaben enthält.
3	Simon (Heidelberg) "Chir. d. Nieren," Erlangen, 1871. Th. i. p. 33	Weibl. 46	Ureterfistel nach einer Ovariohysterotomie	18 Mon.	1869 2 Aug.	Lumbal extraperitoneal	Nicht antiseptisch	3 Seidenligaturen, dick, No. 2.	"Weder Kopfschmerz noch Schlafsucht, noch Delirium, noch Krämpfe; Patientin brach in Folge des Chloroform; Urin anfangs vermindert, aber kein Eiweis darin"	Ausser Bett nach 6 Wochen; die 3 Ligaturen blieben liegen bis zum 6ten Monat, als Patientin ganz wohl zu sein schien; nach ihrer Entfernung heilte die Fistel in wenigen Tagen	Während der Operation aber leicht gestillt	"Ganz gesund in allen Theilen"	Der ursprüngliche Bericht ist sehr werthvoll.—N.B. Eine Untersuchung des Ausgebrochenen zeigte keinen Harnstoff. Die Patientin starb am 1. Mai 1877 an Lungenphthise. Die vorhandene Niere hypertrophisch, Herz normal. (*Deutsches Med. Woch.* 1881).
4	Esmarch (Kiel) *Archiv für Gynäk.*, Bd. i. Heft 3, p. 413	Weibl. 19	Ovariencyste	12 Jahre	1860 15 Nov.	—	—	16 Ligaturen im Ganzen	Nur Durst auffallend. Schmerz im Rücken fortwährend	Tod 36 Stunden später; Infarct der Lungen u. schweres Athmen auffallend	Während heftig aus Adhäsionen, später keine	Grosse Cyste der linken Niere	Diese Operation wird von einigen Schriftstellern irrthümlich Schetelig zugeschrieben; Schetelig schilderte sie nur mit Esmarch's Erlaubniss.
5	Gilmore (America) *Amer. Jour. of Obstet.*, May, 1871	Weibl. 33	Schmerzhafte Nieren; Patientin 5 Monat schwanger	4 Jahre	1870 Dec.	Lumbal	—	1 Ligatur eines Gefässes nach der Entfernung	—	Genas wohl, hatte keine Fehlgeburt	—	Geschrumpft, fibrös, mit Rippeneindrücken.	Die Patientin war eine Negerin, und litt vor der Operation in hohem Grade. Die Beschaffenheit des entfernten Körpers war nicht deutlich erkennbar bis nach der Entfernung.

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TABELLE ALLER FÄLLE VON NEPHRECTOMIE.—(*Fortsetzung.*)

No.	Operateur und ursprünglicher Bericht	Geschlecht und Alter	Diagnose oder Zweck der Operation	Alter des Leidens	Datum der Operation	Sitz des Schnittes	Art des Verbandes	Behandlung des Stieles	Symptome nach der Operation	Folgen, unmittelbare und mögliche	Hämorrhagie bei oder nach der Operation	Beschaffenheit des entfernten Organs	Bemerkungen
6	Bruns (Deutschland) *Würtemb. Med. Corr. Bl.*, 1871, No. 14	Männl.	Schusswunde, 2 Dec. 1870	4 Mon.	1871 23 März	Lumbal 9 Zoll lang	Nicht antiseptisch	Unterbindung sehr schwierig	Gross. Schmerz und Verlangen zu uriniren. Sprach sehr deutlich	Tod nach 10 Stunden in Folge von Schok	Unbedeutend	Verwandelt in einen grossen Abscess	Der Patient war sehr schwach vor der Operation, und ertrug das Chloroform sehr schwer. Die Operation dauerte fast 2 Stunden. Die rechte Niere war ebenfalls mit kleinen Abscessen durchsetzt.
7	Meadows (England) *British Med. Journal*, 1871	Weibl.	Geschwulst des Eierstocks	—	1871 24 Juni	Bauchschnitt	—	Unterbindung.	Nirgends Entzündung	Tod am 6ten Tage an Hämorrhagie aus dem Stiele	—	Grosse Cyste	Der ursprüngliche Bericht ist sehr dürftig.
8	Simon (Heidelberg) "Chr. d. Nieren," Thl. ii. p. 148	Weibl. 30	Pyelitis calculosa	12 Jahre	1871 8 Aug.	Lumbal	Charpie leicht in die Wunde gedrückt	Seidenligatur No. 2	Erbrach sich, aber keine urœmischen Symptome. Schmerz im Unterleib, nahe der Harnröhre	Tod am 31sten Tage in Folge von Pyämie, beginnend am 21sten	Keine in Folge des Schnittes; starke aus dem Stiele bei der Operation	Klein, fibrös, mit Steinen und Blutklumpchen	"Sehr günstige Zustände bis zum 21sten Tage, als Pyämie eintrat." Der ursprüngliche Bericht ist sehr sorgfältig gemacht und werthvoll. Die Operation dauerte ½ Stunde.
9	Durham (London) *Brit. Med. Journal*, 18 May, 1872	Weibl. 43	Schmerzhafte Nieren	2 Jahre	1872 14 Mai	Lumbal	—	—	Keine schlechten	Tod innerhalb einer Woche	—	—	Ursprünglicher Bericht sehr dürftig.
10	Peters (New York) *N. Y. Med. Journal*, Nov. 1872	Männl. 36	Pyelitis calculosa	19 Mon.	1872 16 Mai	Lumbal schwierig	Lint	Ligatur No. 1 starke Seide	Erbrechen, aber keine urœmischen Symptome	Tod 65 Stunden später	Sehr schwach während u. nachher	Vergrössert u. granulös	Die Operation dauerte 2½ Stunden.
11	Brandt (Oestreich) *Wien. Med. Wochenschrift*, Nov. 1873, p. 1079	Männl. 25	Verletzung durch ein Messer, das die Niere zum Vorfall in der Lendengegend brachte	4 Tage	1873 7 Juni	Lumbal mit Benutzung der Wunde	Nicht antiseptisch	Doppelte Seide	Keine urœmischen Symptome vorher oder nachher, aber schwitzte stark	Genesung; verliess das Hospital am 16ten Tage; Verband wurde am 6ten Tage abgenommen	Keine, weder vorher noch nachher	Gesund; Gewicht 58 Gr.	Der ursprüngliche Bericht sehr belehrend. Nach der ersten Schnittwunde war der Patient im Stande ohne Beschwerde umherzugehen, ungeachtet dessen, dass die Nieren aus der Oeffnung hervorstanden, in die sie durch Husten gedrängt worden waren.

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TABELLE ALLER FALLE VON NEPHRECTOMIE.—(*Fortsetzung.*)

No.	Operateur und ursprünglicher Bericht	Geschlecht und Alter	Diagnose oder Zweck der Operation	Alter des Leidens	Datum der Operation	Sitz des Schnittes	Art des Verbandes	Behandlung des Stiels	Symptome nach der Operation	Folgen, unmittelbare und mögliche	Hämorrhagie bei oder nach der Operation	Beschaffenheit des entfernten Organs	Bemerkungen
12	Campbell (Dundee) *Edin. Medic. Journ.*, 1874, Bd. xx. p. 36	Weibl. 49	Erkrankung der Ovarien	18 Mon.	1873 2 Dec.	Median	Gemässigt antiseptisch	Hanfligatur	Nicht urämisch, freier Urinfluss von Anfang	Genesung, aber langsam; die Wunde völlig geheilt am 84sten Tage; verliess das Hospital am 113ten Tage	Unbedeutend	Cyste im unteren Viertel der Niere.	
13	Marvaud (Algier) *Revue de Méd. Milit.*, Oct., 1873	Weibl. junge Araberin.	Wunde durch einen Yatagan	Wenige Tage	1875	Lumbal mit Benutzung der Wunde	—	Seidenligatur	Keine Urämie; guter Urinabfluss von Anfang	Genesung; verliess das Hospital in 2 Monaten ganz wohl	Stark vor der Ligatur	[illegible]	Die Niere und Ligatur wurde der spontanen Abstossung überlassen, welche in einigen Wochen erfolgte.
14	Langenbuch (Deutschland) *Berl. Klinisch. Wochenschrift*, No. 24, 1877	Weibl. 32	Sarcom der Lumbalmuskeln oder der Niere	18 Mon.	1875 7 Dec.	Lumbal	—	Ligatur schwer, fiel ab	Keine schlechten, kein Fieber	Genesung; verliess das Hospital Jan. 1876	Schwache, als die Ligatur abfiel	Zweifelhaft, ein [illegible] farbiges Hohlgebilde	Es herrscht Zweifel in Bezug auf die Beschaffenheit der entfernten kranken Masse, da dieselbe durch Unachtsamkeit eines Dieners weggeworfen wurde.
15	Kocher (Bern) *Deutsche Zeitschr. f. Chir.*, Bd. ix., 1878	Weibl. 35	Sarcom einer leicht beweglichen Niere	18 Mon.	1876 20 April	Bauchschnitt	Antiseptisch; schlug fehl	Nicht erreicht	Cystitis am 2ten Tage	Tod am 3ten Tage in Folge von Peritonitis	Keine wahrgenommen	Ausgedehntes Sarcom	Die Operation wurde aufgegeben, da das Gewächs sich zu weit in das quer laufende Grimmdarmsgekröse erstreckte, um davon abgelöst werden zu können. Bauchwunde genäht.
16	Jessop (Leeds) *Lancet*, 1877, vol. i.	Männl. 2½	Nierenencephaloid	1 Jahr	1877 7 Jan.	Lumbal	Nicht antiseptisch	Peitschenschnur Ligatur	Keine bösen, kein Erbrechen, guter Abfluss des Urins	Genesung; das Kind auf und umherlaufend bald darauf	—	—	Herr Jessop benachrichtigt mich, dass er sich 8 Monate später durch Untersuchung davon überzeugte, dass die Rückkehr des Uebels wahrscheinlich in den lumbalen Drüsen entstand; das Kind starb wenige Wochen später zu Hause; keine Sektion. (Barker.)
17	Kocher (Bern) *Deutsche Zeitschr. f. Chir.*, Bd. ix., 1878	Männl. 2½	Nierengeschwulst	2½ Jahre	1877 27 Sept.	Bauchschnitt	Antiseptisch	Doppelte Catgut Ligatur	Nicht urämisch, kein Erbrechen, freier Abfluss des Urins bald nachher	Tod am 3ten Tage in Folge von septischer Peritonitis	Bedeutend während; keine aus dem Stiele	Adeno-Sarkoma 16 × 10 × 10 Ctm.	

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18	Martin (Berlin) *Langenb. Arch.*, Bd. xxiii. 1879, p. 54	Weibl. 49	Schmerzhafte Wanderniere	8 Jahre	1878 15 Mai	Bauchschnitt	Antiseptisch, streng.	Ligatur 1 en masse, 2 mit Durchstich	Schüttelfrost Schmerz, Uebelkeit, Schlafsucht	Genesung; ausser Bett am 17ten Tage; Wunde heilte am 26sten; kräftig, arbeitete 3 Monate	Keine in Folge des Stieles, flüssig vom Peritoneum	Ganz, gesund	Die heftigen Schmerzen der Patientin wurden durch die Operation ganz beseitigt; letztere dauerte 36 Minuten.
19	Martin (Berlin) a. a. O.	Weibl. 30	Schmerzhafte Wanderniere	5 Mon.	1878 25 Aug.	Bauchschnitt	Streng antiseptisch	4 Durchstich eine Massen Ligatur	Etwas Erbrechen und Schmerz	Genesung; ausser Bett am 13ten Tage umhergehend; Wunde heilte am 17ten; entlassen	—	Ganz gesund	Ein noch überraschenderer Fall als der vorige; die Reaction war sehr schwach, die Genesung sehr schnell; die Operation dauerte 50 Minuten.
20	Martin (Berlin) a. a. O. und Privatbrief	Weibl. 25	Schmerzhafte Wanderniere	—	1878	Bauchschnitt	Streng antiseptisch	—	—	Genesung, vollständig.			
21	Martin (Berlin) *Berlin Klinisch. Wochenschr.*, 1879, No. 22	Weibl. 53	Sarcom der Nieren	2 Mon.	1878 9 Dec.	Bauchschnitt	Streng antiseptisch	9 Seidenligaturen	—	Genesung, vollständig; ausser Bett am 8ten Tage, ging heim am 17ten Tage	Keine	Bösartiges Neugewächs; Gewicht 28 Unzen	Die Operation wurde besonders schwierig durch das Vorhandensein von grossen Venen, welche über die Geschwulst liefen; die Operation dauerte 45 Minuten.
22	Czerny (Heidelberg) *Centrlbl. f. Chirurg.* No. 45, 1879	Männl. 50	Bösartige Nierengeschwulst	2 Jahre	1879 19 Jan.	Bauchschnitt	—	Nicht erreicht	—	Tod 10 Stunden später an Shok	Starke bei der Operation aus den Nieren-Arterien	Weiche schwammige Masse. blieb zurück Medullarer Nierenkrebs	Nach Einbrechen in die weiche Masse der Geschwulst begann heftige Blutung in Folge von zerrissenen Nierenarterien; die Aorta wurde dann unterbunden zwischen den zwei Nierenarterien.
23	Smith (N. Orleans) *N. Orleans Medical and Chirurg. Journal*, Aug. 1879	Weibl. 35	Schmerzhafte Wanderniere	8 Jahre	1879 3 Juni	Lumbal nicht schwierig	Nicht antiseptisch	Ligatur stark	Keine urämischen; leichtes Fieber; frei von Eiterung	Genesung ununterbrochen; ging umher am 11ten Tage	—	Gesund; zeigte Narben vom Haarseil	Eine Ovariotomie wurde versucht 1873, jedoch ohne Erfolg; ein Haarseil wurde 1874 durch die Bauchwandungen und den Tumor gezogen um Vernarbungen zu erzeugen; vorübergehende Erleichterung.

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24	Zweifel (Erlangen) *Arch. f. Gynäk.*, Bd. xv. Heft 1, p. 1	Weibl. 29	Ureter-Uterusfistel nach Entbindung	2½ Jahre	1879 9 Jan.	Lumbal	Antiseptisch	Seidenligatur lang gelassen	Erbrechen, Schmerz und Unruhe am 1sten Tage, fast verschwunden am 2ten	Genesung; Verbleiben im Hospitale verlängert wegen Lösung der Ligaturen	Mässig aus der zerrissenen Niere	Klein, atrophisch, Ureter erweitert	Die Entfernung des Organs hatte keine üble Wirkung.
25	Czerny (Heidelberg) a. a. O.	Weibl. 32	Pyonephrosis	4 Jahre	1879 22 Mai	Lumbal	Chlorzink, Thymol-Gaze	Seide, und elastische Ligatur	Keine beachtet	Genesung, kaum eine Spur von Fieber, auf am 25sten, entlassen am 42sten Tage	Ziemlich stark während der Operation	Starker Abscess; brach bei der Operation auf	Ein Theil der 11ten Rippe wurde bei der Operation entfernt, um zu dem Stiel gelangen zu können. Es war dort vorher eine perinephritische Entzündung und Fistel gewesen.
26	Angeführt von Mr. Wells, *Med. Times & Gaz.*, 1870, 8ten Jan.	—	Fibrocystische Geschwulst der Gebärmutter	—	—	Bauchschnitt	—	—	Keine urämischen Symptome oder solche die auf den Verlust der Niere zu beziehen wären	Tod am 3ten Tage	—	Gesund	Der ursprüngliche Bericht ist sehr kurz, aber die Niere scheint zufällig entfernt worden zu sein, da sie der Geschwulst anhing.
27	Martin (Berlin) Privatbrief	Weibl. 48	Schmerzhafte Wanderniere	—	—	Bauchschnitt	Streng antiseptisch	—	—	Tod in Folge von Peritonitis.			
28	Martin (Berlin) Privatbrief	Weib 24	Schmerzhafte Wanderniere	—	—	Bauchschnitt	Antiseptisch	—	—	Tod in Folge von septischer Peritonitis.			
29	Billroth, *Arch. f. Klin. Chir.*, Bd. xxi. S. 694	Weibl. 46	Ovariencyste	5 Jahre	18 Juli 1876	Bauchschnitt	Antiseptisch	Hanf Ligaturen	Singultus, Erbrechen	Tod an septische Peritonitis am 2ten Tage	Stark aus der Vena spermatica	Hydronephrose rechts.	
30	Czerny (Heidelberg) *Arch. f. Klin. Chir.*, Bd. xxv. s. 860	Männl. 23	Hydronephrose	Vielleicht angeboren	9 März 1880	Lumbalschnit	Antiseptisch	Seiden Ligaturen	Collaps	Tod ½ Stunde nach der Operation	Während der Operation mässig	Spindelzellensarkom in der Hydronephrose; kein Harnstoff im Inhalte	Trotz der vor der Operation schon diagnosticirten Phthise wurde eine Radikalincision geplant. Als sich bei der Eröffnung des Sackes knollige Sarkommassen fanden, wurde die Exstirpation, welche sehr schwierig war, sofort ausgeführt.

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31	Czerny, l. c. S. 859	Weibl. 37	Hydronephrose vielleicht malignen Charakters	8 Mon.	6 Okt. 1879	Medianer Bauchschnitt	Antiseptisch	Seiden Ligaturen in 2 Portionen	Keine	Heilung fast ohne Fieber, auf am 30sten Tage	Keine	Hydronephrose rechts mit 0·7 p. Cent Harnstoff ohne Complicationen	Die Hydronephrose war mässig verschiebbar u. wurde extirpirt, weil sie in den letzten 8 Wochen rasch wuchs und Schmerzen verursachte.
32	Czerny (Heidelberg) *Arch. f. Klin. Chir.*, Bd. xxv. S. 858	Männl. 23	Nierenstein links	Seit früher Jugend	15 Mai 1880	Lumbal in der Richtung der 12 Rippe	Antiseptisch	11 Seidenligaturen	Anfangs bloss 504 G. Urin, aber bald normal	Schmerzen verschwunden; Heilung ohne Fieber und ohne Eiterung in 14 Tagen	Gering	Niere gesund, im Becken ein harnsaurer Stein	Die Entdeckung des Steines (er wog bloss 1,08 Gramm) gelang erst, nachdem das ganze Organ ausgeschält und die Nierenvene unterbunden war. Die Schmerzen sind nie wiedergekehrt, allein im Juni 1881 musste Patient nach Soden, wo er sich von beginnender Phthise sichtlich erholte.
33	Czerny, l. c.	Weibl. 27	Ureterscheidenfistel r hts	Entbindung am 23 Oktober 1879	3 April 1880	Lumbal mit Resection der 12 Rippe	Antiseptisch	10 Seidenligaturen	Anfangs bedeutende Urinverminderung, die bald normal wurde	Heilung ohne Fieber und ohne Eiterung in 14 Tagen	Ziemlich stark aus einem Vas aberrans	Niere gesund	Der Katheterismus des Ureters von der Blase war misslungen. Im Juli 1881 ganz wohl und arbeitsfähig.
34	Czerny, l. c.	Weibl. 40	Hydronephrose rechts	—	3 Mai 1880	Medianer Bauchschnitt	Antiseptisch	3 Seidenligaturen	In der zweiten Woche Parotitis	Tod am 20. Juni 1880	Gering	Cyste am unteren Pole der Niere von Kindskopfgrösse	Die Bauchwunde war ganz reactionslos verklebt, descissirte jedoch etwa 8 Tage vor dem Tode. Metastatische Abscesse in der Lunge. Die linke Niere atrophisch, granulös.
35	Czerny, *Deutsche Med. Wochenschrift*, 1881, mitgetheilt von F. Braun	Weibl. 11 Mon.	Nierentumor links	6 Wochen.	2 Aug. 1880	Medianer Bauchschnitt	Antiseptisch	6 Seidenligaturen	Ziemlich viel Erbrechen und Diarrhoe	Tod am 5. August an septischer Peritonitis	Gering	Adenom von mehr als Kopfgrösse	Wegen der Gefahr der Carbolintoxication musste mit Carbolsäure sehr gespart werden. Uebrigens war das Kind schon vor der Operation ziemlich schwach.

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36	Czerny, *Deutsche Med. Wochenschrift*, 1881	Weibl. 45	Sarkom der rechten Niere	—	7 Mai 1881	Medianer Bauchschnitt	Antiseptisch	Nicht erreicht	—	Tod an Verblutung aus der Vena renalis	Bedeutend aus der Vena renalis	—	Schon bei der Operation merkte man, dass es sich um eine *Hufeisenniere* handle; leider war schon die profuse Blutung eingetreten, welche bloss durch ausgiebige Umstechung, wobei die Vena cava mitgefasst wurde, gestillt wurde. In den vereiterten Nierenkelchen lagen viele Steine. Grosse Mengen Eiter im Urin hatten vielleicht wenigstens die Diagnose Pyonephrose zugelassen. Allein es fehlte Fieber, die Punction war erfolglos.
37	Czerny, l. c.	Weibl. 35	Pyonephrose rechts	Ueber Jahres Frist	10 Jan. 1881	Lumbalschnitt	Antiseptisch 5 p. Cent Chlorzink	Doppelte Seidenligatur	Wegen Carbolerbrechen musste der Lister-Verband am 3ten Tage verlassen werden	Genesung mit mässigem Fieber	Ziemlich starke parenchymatöse Blutung aus dem zerrissenen Organ	Mit nussgrossen Abscessen durchsetzt, Nierenbecken ausgedehnt	Da die Diagnose unsicher erschien, wurde zunächst am 22. Oktober eine antiseptische Incision in die kranke Niere gemacht. Darauf hörte das starke Fieber auf, der Urin wurde eiterfrei, und erst als wieder Fieber eintrat, wurde zur Exstirpation geschritten. Befand sich im Juli 1881 ganz wohl.
38	Czerny, l. c.	Männl. 51	Nierentumor links	Ueber ein Jahr	23 April 1881	Querer Lumbalschnitt	Antiseptisch	10 Seidenligaturen	Wegen Carbolerbrechen wurde am 3ten Tage Thymolverband angewandt	Genesung ohne Fieber mit mässiger Eiterung	Mässig bei der Operation	Angio-Sarkom a. unteren Pole der Niere von Kopfgrösse	Am Colon descendens hatte die Geschwulst die Kapsel durchbrochen, und mussten hier schwammige Massen abgeschabt und mit Chlorzink geätzt werden.
39	Czerny, l. c.	Männl. 32	Hydronephrose rechts mit Stein	2 Jahre	17 Juni 1881	Schiefer Lendenschnitt	Antiseptisch	18 Seidenligaturen, wovon 8 in den Bauchdecken	Vollkommene Anurie, Erbrechen von sauren Massen ohne Harnstoff	Tod 37 Stunden nach der Operation	Mässig	Hydronephrose, aber ohne Stein	Da die Urinsecretion vor der Operation ganz normal war, nie Beschwerden links geklagt wurden, hielt man die linke Niere für gesund. Sie war jedoch vollkommen atrophirt, der Ureter impermeabel.

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40	Thornton (London) *Lancet*, 1880, vol. i.	Weibl. 7	Hydronephrose	5 Jahre	3 Jan. 1880	Bauchschnitt.	Antiseptisch	Arterien Venen und Stiel extra unterbunden	—	Genesung	—	Linke Hydronephrose	Wandungen sehr dünn, daher die Exstirpation schwierig
41	C. Hüter (Greifswald) *Deutsche Ztsch. f. Chir.*, Bd. ix. S. 507	Weibl. 4	Sarkom der Milz oder des Ovarium	1 Jahr	4 Juli 1876	Bauchschnitt über den Tumor	Antiseptisch	Nicht erreicht	—	Tod durch Verblutung bei der Operation	Sehr stark aus den Stielgefässen	Paranephritisches Sarkom links	Die Diagnose wurde nicht gestellt, weil der Urin nichts Abnormes enthielt, und der Tumor vor den Därmen lag
42	Merkel (Nürnberg) *Bairisches Intelligenzbl.* 1872, No. 2	Weibl. 28	Rechtsseitige Wanderniere	—	24 Okt. 1879	Bauchschnitt	Antiseptisch	Carbolseideligatur	Unstillbares Erbrechen, verminderte Harnsecretion	Tod 5 Tage nach der Operation	—	Fettig degenerirte Wanderniere	Auch die rechte Niere war fettig degenerirt, daher Tod durch Urämie, obgleich im Urin vor der Operation kein Eiweiss war.
43	Lossen (Heidelberg) *Deutsche Ztschr. f. Chir.* Bd. xiii. S. 199	Weibl. 37	Ovexialtumor Gravidität im 3 Monate	1 Jahr	11 Aug. 1879	Medianschnitt	Antiseptisch	Extraperitoneal mit Klammer	Abortus 12 Stunden nach der Operation	Heilung trotz septischer Endometritis in 6 Wochen	Sehr stark aus den Stielgefässen während der Operation	Rechtsseitiges Angiosarkom in einer Wanderniere	Der Tumor sass in der Rinde ohne die Nierensubstanz wesentlich zu alteriren, daher auch alle Erscheinungen im Urin fehlten. Soll noch im Juni 1881 gesund sein.
44	Barker, *Lancet*, 1880, vol. i. p. 405. *Med. Chir. Trans.*, vol. lxiii.	Weibl. 21	Rechtsseitige Wanderniere, vielleicht bösartiger Natur	8 Mon.	23 Dec. 1879	Medianschnitt	Antiseptisch	Doppelte Massenligatur mit starker Seide	Erbrechen	Tod 45 Stunden nach der Operation durch Lungenembolie	Gering	Alveolares Bindzellensarkom von einer Pyramide ausgehend	Bei der Section fanden sich Metastasen in den Lungen; Geschwulstthromben in den Nierenvenen.
45	Billroth, *Wiener Med. Wochenschrift*, 1880, No. 28	Weibl. 35	—	3 Jahre	1 April 1879	Medianschnitt	Antiseptisch	Ligatur	Peritonitis. Harnsecretion nach der Operation nicht wesentlich gestört.	Tod am 5ten Tage an septischer Peritonitis	Bedeutend aus dem Tumor	—	Es handelte sich um ein 18 Kilo wiegendes retroperitoneales Myofibrome, das mit der linken Niere so fest verwachsen war, dass dieselbe mitgenommen werden musste, obgleich sie gesund war.

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46	Dumreicher (Wien) *Arch. f. Klin. Chir.*, Bd. xxv. S. 224	Männl. 33	Nierenstein links mit Pyelitis und Perinephritis	Ueber 10 Jahre	6 Aug. 1877	Lumbalschnitt	Nicht antiseptisch	Ligaturen, Operation nicht vollendet	Links Pyopneumothorax, weil die Pleura eröffnet wurde	Tod am folgenden Tage durch Collaps	Bedeutend aus den Resten der Niere	Niere in einen Eitersack umgewandelt, kein Stein	*Schon 1873 wurde ein perinephritischer* Abscess links durch Incision geheilt. Am 10. August 1876 Entfernung eines Blasensteins durch den seitlichen Steinschnitt. Da die elfte Rippe für die zwölfte gehalten wurde, passirte eine Verletzung des Zwerchfells, welche den tödtlichen Pneumothorax zur Folge hatte.
47	Müller (Oldenburg) *Berl. Kl. Wochenschrift*, 1880, No. 24	Männl. 21	Nierenstein mit Hydronephrosenbildung	4 Jahre	18 Feb. 1878	Lumbalschnitt	Nicht antiseptisch	—	Im September eine Senkung gegen den Mastdarm, welche von diesem aus entleert wurde	Vollständige Genesung nach 16 Monaten	Unbedeutend	Niere in einen Eitersack umgewandelt	Schon am 9. September 1877 wurde ein perinephritischer Abscess durch Incision geheilt.
48	F. Lange, *New York Med. Rec.*, vol. xviii. No. 6	Weibl. 47	Rechte Pyonephrose	3 Jahre	24 Mai 1880	Schiefer Lumbalschnitt	Antiseptisch	4 Ligaturen	Viel Erbrechen, Koma, kein Tropfen Urin nach der Operation entleert	Tod an Anurie	Gering	Die rechte Niere hatte die doppelte Grösse einer normalen, und war mit Abscessen und Cysten, die Excremente enthielten, durchsetzt	Der Urin war manchmal vollkommen klar, und enthielt manchmal viel Eiter unter Erleichterung für den Patienten. Die linke Niere war vollkommen cystisch entartet, der Ureter ganz undurchgängig.
49	Clement Lucas, *British Med. Jour.*, 12 March, 1880; *Lancet*, 1880, vol. i. p. 403	Männl. 36	Linke Niere vereitert mit Fistelbildung	6 Jahre	17 Feb. 1880	Lumbalschnitt	Antiseptisch	Ligaturen des Ureters und der Gefässes separirt	—	Heilung	Nachblutung am 12ten Tage, die mit Eisenchlorid gestillt wurde	—	Acht Monate nach der Operation hatte der Mann an Gewicht zugenommen, und befand sich wohl bis auf etwas Eiterabgang im Urin. Befindet sich im Juli 1881 wohl, und hat, seit der Operation, um 28 Pfunde zugenommen.

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50	Hamilton Cartwright (China) *Lancet*, 1880, vol. i. p. 403	—	Vorfall der Niere durch eine Lendenwunde	—	—	—	Verband mit Vogelmist	—	—	Heilung	—	—	Starke Eiterung. Die Niere halb vereitert.
51	Spiegelberg, *Arch. f. Gynäk.* Bd. i. S. 146	Weibl.	Ovariencyste	—	—	—	—	—	—	Tod nach 26 Stunden	—	Echinococcus der rechten Niere	Exstirpation gelang nicht vollständig.
52	Le Fort (Paris) *Gaz. de Paris*, No. 50 u. 52, 1880	Männl. 23	Ureterbauchfistel nach Verletzung	1 Jahr	20 Mai 1880	Lumbalschnitt	Camphergeist	Massenligatur	Viel Erbrechen, starke Schmerzen	Tod 50 Stunden nach der Operation	—	Scheinbar gesund	Um Platz zu schaffen musste die zwölfte Rippe resecirt werden.
53	Th. Savage, *Lancet*, April 1880, p. 601	Weibl. 56	Hydronephrose	3-4 Jahre	16 Jan. 1880	Medianschnitt	Carbollint	Drahtklammer	—	Heilung mit Zurücklassung einer Fistel, die täglich einen Esslöffel Urin entleerte	Keine	—	Die Hydronephrose sass breit gestielt auf, und wurde desshalb der entleerte Sack etwa in seiner Mitte mit Klammen an der Bauchwand befestigt.
54	B. Credé (Dresden) *Arch. f. Gynak.*, Bd. xiii. p. 312	Weibl. 26	Ureter Uterusfistel rechts	4 Mon.	19 Mai 1880	Lumbalschnitt mit Resection von 2 Cm. der 12 Rippe	Antiseptisch	Massenligaturen	Anfangs Abends Fieber. Harnmenge steigt von 560 Gr. auf 1200	Geheilt nach 6 Wochen	Mässig aus dem Stiele	Interstitielle Entzündung der exstirpirten Niere	Schwierigkeiten bei der Entscheidung ob der rechte Ureter betheiligt sei. Ist neun Monate nach der Operation vollkommen gesund.
55	Byford (Chicago) *Amer. Journal of Obstet.* 1880	Weibl. 39	—	—	—	—	—	—	—	Geheilt	—	4½ Pfunde schweres Nierenencephaloid	
56	Martini, E. (Hamburg) *Arch. f. Chir.*, Bd. xxvi. S. 513, mitgetheilt von C. Löwenstein	Weibl. 37	Wandernicre rechts	Seit 20 März 1878 durch Fall entstanden	17 Juli 1879	Medianschnitt	Antiseptisch	2 Seidenligaturen en masse	Urinsecretion nach der Operation gering	Heilung nach der Operation, und auch Beseitigung der Beschwerden; kein Fieber	Mässig aus einem Kapselgefässe	Klein, aber gesund	Die Bauchfellstasche wurde nicht genäht.

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TABELLE ALLER FÄLLE VON NEPHRECTOMIE.—(*Fortsetzung.*)

No.	Operateur und ursprünglicher Bericht	Geschlecht und Alter	Diagnose oder Zweck der Operation	Alter des Leidens	Datum der Operation	Sitz des Schnittes	Art des Verbandes	Behandlung des Stieles	Symptome nach der Operation	Folgen, unmittelbare und mögliche	Hämorrhagie bei oder nach der Operation	Beschaffenheit des entfernten Organs	Bemerkungen
57	Couper (London) *Revue de Chir.*, 1881, No. 4. *Brit. Med. Jour.*, 1880, vol. ii. p. 850. *C.f. Chir.*, 1881, No. 2	Junges Mädchen	Pyonephrose rechts	1 Jahr	24 April 1880	Lumbal	Antiseptisch	Massenligaturen Gefässe, der Ureter für sich mit Catgut	Verlauf aseptisch	Heilung ungestört; steht am 36sten Tage auf	Fast keine	—	Bei der Operation war das Peritoneum eingerissen.
58	E. Barker (London) *Cent. f. Med.*, 1881, No. 2	Weibl. 32	Pyonephrose vielleicht durch Stein links	Seit 5 Wochen	5 Juli 1880	Lumbal	Antiseptisch	Massenligaturen in 2 Portionen	—	Tod 3 Stunden nach der Operation an Shok	Nicht unbedeutend aus Stielgefässen	Ein Stein im Nierenbecken; Niere ganz vereitert	Am 4. Juli Entleerung von 500 Grammen Eiter durch die Punction.
59	E. Barker (London) l. c.	Weibl. 38	Grosser Nierenstein durch Punction erwiesen	Ueber 7 Monate	5 Okt. 1880	Schräger Lumbalschnitt	Antiseptisch	Massenligaturen	—	Tod 12 Stunden nach der Operation an Shok	—	Grosser verstelter Stein in der vereiterten Niere, welche mit dem Colon verwachsen war. Letzteres eingerissen	Sehr elend vor der Operation. Die Abtragung gelang nicht, da in der Nähe des Stieles zu viele Adhäsionen bestanden. Die ausgelöste ganz vereiterte Niere mit dem Stein musste desshalb zurückgelassen werden, nachdem sie abgebunden war.
60	Langenbuch (Berlin) *C. f. Chir.*, 1881, 10. "Chirurgen Congress," S. 44	Weibl. 30	Wanderniere rechts	—	Ende 1880	Bauchschnitt rechts vom Rectus	—	Doppelte Massenligatur mit Seide	Alle Beschwerden verschwunden	Vollkommen geheilt (persönliche Mittheilung)			
61	Langenbuch, l. c.	Mannl. 20	Wanderniere rechts	—	—	Bauchschnitt rechts vom Rectus, später noch Querschnitt	—	Ebenso	Ebenso	Vollkommen geheilt (ebenso)	Stark aus einer übersehenen Nebenarterie.		

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TABELLE ALLER FÄLLE VON NEPHRECTOMIE —(*Fortsetzung.*)

No.	Operateur und ursprünglicher Bericht	Geschlecht und Alter	Diagnose oder Zweck der Operation	Alter des Leidens	Datum der Operation	Sitz des Schnittes	Art des Verbandes	Behandlung des Stieles	Symptome nach der Operation	Folgen, unmittelbare und mögliche	Hämorrhagie bei oder nach der Operation	Beschaffenheit des entfernten Organs	Bemerkungen
62	N. Mynter, *C. f. Chir.*, No. 19, 1881	Weibl. 45	Nierenstein mit Fistel	—	—	Lumbalschnitt mit Resection der 11 Rippe	—	—	Zunächst erholte sich Patientin	Tod 20 Tage nach der Operation an Pneumonie	—	—	Die Exstirpation der Niere gelang nicht, da sie zu ausgedehnt verwachsen und zerstört war.
63	Bardenheuer (Cöln), "Die Drainirung der Peritonealhöhle," Stuttgart, 1881, S. 242	Weibl. 48	Uteruscarcinom, welches auf den linken Ureter übergriff	—	3 Okt. 1880	Vertikaler Lumbalschnitt am vordern Ende der 11 Rippe	Antiseptisch	Massenligatur	Mangelhafte Urinsecretion, angeblich in Folge der Herzschwäche	Tod am 2ten Tage an Collaps	Mässig	—	Herz und Nierensubstanz waren verfettet.
64	Bardenheuer, l. c., S. 253	Manul. 46	Pyonephrose mit Fistel links	3 Jahre	4 Nov. 1879	Lumbalschnitt	Antiseptisch	2 Massenligaturen	In den ersten 14 Tagen Carbolerscheinungen, die erst verschwanden nach Aussetzung der Ausspülung	Geheilt nach 8 Wochen	Starke Blutung bei der Excision der Nierenkapsel	—	Mai 1879 wurde ein paranephritischer Abscess entleert. Der Versuch die Nierenkapsel zu entfernen war vergeblich und kostete viel Zeit. Das Peritoneum riss dabei ein.
65	Bardenheuer, l. c., S. 257	Weibl. 22	Perinephritis über Abcess nach Nierenstein oder Geschwulst	Seit Juni 1879	23 Nov. 1879	Lumbalschnitt	—	Massenligatur	Starker Collaps, ziemlich viel Fieber	Tod am 3. Dezember vermuthlich an Sepsis, die schon vor der Operation bestand	Stark aus der Abscesshöhle	Hühnerei grosses Spindelzellensarkom am Hilus	Urin zeigte keine fremdartigen Bestandtheile; starkes Fieber vor der Operation.
66	Bardenheuer, l. c., S. 261	5	Links Hydronephrose durch Probeschnitt festgestellt	Vor 3 Monatn. überfahren, seitdem entstand die Geschwulst	—	Lumbalschnitt	Antiseptisch	In 2 Portionen ligirt	Carbolerbrechen durch 8 Tage, mehrmals Fiebersteigerung durch Eiterverhaltung	Heilung in 6 Wochen	Wohl gering	In der Niere ein Abscess. Sie war umgeben von 5–6 durch Urin colossal ausgedehnten Kapselschalen.	B. erklärt die Erkrankung durch einen Riss in der Niere, welcher vereiterte, und zugleich dem Urin die geborstene Kapsel lieferte, welche dadurch aufgeblättert wurde. Die Kapselblätter wurden zurückgelassen.

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TABELLE ALLER FÄLLE VON NEPHRECTOMIE.—(*Fortsetzung.*)

No.	Operateur und ursprünglicher Bericht	Geschlecht und Alter	Diagnose oder Zweck der Operation	Alter der Leidens	Datum der Operation	Sitz des Schnittes	Art des Verbandes	Behandlung des Stieles	Symptome nach der Operation	Folgen, unmittelbare und mögliche	Hämorrhagie bei oder nach der Operation.	Beschaffenheit des entfernten Organs	Bemerkungen
67	Bardenheuer, (Cöln), "Die Drainirung der Peritonealhöhle," Stuttgart, 1881, S. 267	—	Nierenblutungen und Koliken links vielleicht von Stein herrührend.	—	—	Lumbal	—	In 2 Partieen ligirt	—	Die Nierenkoliken sind verschwunden, der Urin ist blutfrei.			
68	Bardenheuer, l. c., S. 267	Männl. 20	Perinephritischer Abscess Nierenstein	—	—	Lumbal	—	In 2 Partieen ligirt	—	Noch in Behandlung.			
69	Bardenheuer, l. c., S. 267	Männl. 26	Perinephritischer und nephritischer Abscess mit unerträglichen Schmerzen	—	—	Lumbal	—	In 2 Partieen ligirt	--	Noch in Behandlung.			
70	Czerny, noch nicht publicirt	Männl. 40	Links Nierensarkom mit Blutungen	½ Jahr	25 Juli 1881	Querer Lumbalschnitt	Antiseptisch	En masse mehrmals umschnürt	Urin wird klar, Fieber mässig, keine Urämie	In Behandlung	Ziemlich stark aus der eingerissenen Kapsel und Geschwulst	Kindskopfgrosses Sarkom der unteren Nierenhälfte	Vor mehreren Monaten soll Harngries abgegangen sein. Der Tumor war kopfgross; füllte die linke Lumbalgegend aus; verursachte, nebst den Blutungen, oft unerträgliche Schmerzen.
71	Morrant Baker (London), *British Med. Jour.*, 5 March, 1881, 23 April, 1881	Weibl. 7	Pyelitis, 3 Monate vorher incidirt	18 Mon.	22 Febr. 1881	Lumbal förmig	—	—	Synkope nach der Operation	Drei Monate später war die Wunde nahezu geheilt, jedoch der Eiter im Urin nur etwa um die Hälfte vermindert	Wenig	—	Die Kapsel wurde nicht mitgenommen.
72	Barwell (London), *British Med. Jour.*, 23 April, 1881	Weibl. 18	Pyonephrose links durch Incision sicher gestellt	—	—	Lumbalschnitt	—	En masse mit Seide	Unterbindungsfaden mit gangrenösem Schaumstück wurde am 46sten Tage abgestossen.	Befindet sich am 22. Juni wohl, und ist geheilt bis auf eine kleine Fistel	—	—	Die Entfernung der Niere ist leicht gewesen.

Nos. 73 and 74—Dazu noch 2 Fälle von Wandernieren, die nach persönlicher Mittheilung von Martin operirt worden Sind mit einer Heilung und einem Todesfall.

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Diseases of the Kidney which require Surgical Operation. Illustrated by three cases.

I.—A Case of Nephrotomy, in which Nephrectomy was subsequently performed.
II.—A Case of Nephrotomy.
III.—A Case of Renal Lithotomy.

Mr. W. Morrant Baker, London.

Case I.—*A Case of Nephrotomy, in which Nephrectomy was subsequently performed.*—[For the detailed notes from which the following abstract has been made, I am indebted to Dr. Hale White, House-surgeon, and Mr. H. Fraser Stokes, Registrar to the Evelina Hospital.] The patient, a girl (A. P.), seven years old, was admitted into the Evelina Hospital for Sick Children, under the the care of Dr. Buchanan Baxter, October 8, 1880. She was said to have been always delicate, but free from any serious illness until about eighteen months before her admission into the hospital, when a large quantity of blood, in part coagulated, was passed with the urine. No more blood was noticed until about a year after the first hæmorrhage, when she again had an attack of hæmaturia, but less severe. No blood has been passed since; but at about the same time it was noticed that the urine became purulent; and this condition remained up to the date of admission. For several months, and before the second attack of hæmaturia, she had become languid, and complained of pain in the chest. She also perspired much at night and lost flesh.

At the time of the child's admission into the hospital, a rounded swelling of somewhat indefinite character was found in the abdomen—deeply seated in the region of the right kidney—dull to percussion, and not very tender, varying in size from time to time. The urine was pale and contained a large quantity of pus and sometimes blood. It was passed frequently, and in small quantities at a time.

Hectic fever was very marked. Temperature 104° F. at night, 97° F. in the morning.

Six weeks after admission the quantity of pus in the urine had become doubled; and on one occasion, at about this period, half-a-pint of blood-red urine was passed.

Eight weeks after admission her appetite failed, hectic fever became more marked, and the patient was evidently becoming weaker.

December 7, 1880.—As the result of a consultation with Dr. Baxter, I performed nephrotomy at this date, an incision being made parallel to and below the last rib, as for lumbar colotomy. The kidney was exposed without any special difficulty and incised; and the interior of the organ, which was found much sacculated, was explored with the finger. No stone was detected.

A drainage-tube was inserted in the kidney, and the wound, which had been made under the carbolic spray, was dressed in the usual manner.

It may be mentioned that the distinction between the kidney and its capsule could be very readily made out, and the organ could have been enucleated at this time without any difficulty.

At first, some benefit seemed to accrue from the operation; no shock attending it, and the quantity of pus in the urine becoming somewhat less. But the notes of about a month's date after the operation record that but little pus

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drains away through the tube, while the amount in the urine is almost as great as before the operation. There has been no repetition of the hæmaturia.

The carbolic dressings had been discontinued on account of carboluria. At a date two-and-a-half months after the nephrotomy, it was noted that hectic fever had been very marked of late, the evening temperature rising sometimes to 104·2° F. The wound showed no disposition to contract.

As it seemed obvious that the patient was losing ground, it was decided to remove the kidney, and I performed nephrectomy on the 22nd of February, 1881, being assisted by my colleagues, Mr. Howse and Mr. Clement Lucas.

The operation was somewhat complicated by the condition of the parts which had resulted from the previous nephrotomy. On exploring with the finger, in the first place, through the old incision, which was still at the surface widely open, it was found that all the soft parts around the kidney had become much condensed and matted together; and it was at first very difficult to distinguish the kidney itself from the tissues which embraced it. The conditions presented a marked contrast to those which were present at the time of the nephrotomy ten weeks previously. The outline of the kidney was, however, detected after a few minutes' search, and with the finger the organ was carefully separated from within its capsule, until the line was reached apparently at which the capsule became continuous at the hilus with the upper dilated portion of the ureter. A double strong silk ligature was now passed around the pedicle, with the help of a special blunt needle, and one loop of this was tied, the other being left in reserve, and tied after the removal of the kidney, which was effected by snipping the pedicle with strong curved blunt-pointed scissors.

The first part of the operation was performed under the carbolic spray, but this was discontinued as soon as it became clear that there was no risk of wounding the peritoneum.

During the operation, and especially at the time at which the pedicle was ligatured, the patient became very faint, but revived under artificial respiration and the subcutaneous injection of brandy.

The kidney removed weighed 2¾ ounces; and I am indebted to Dr. Hale White, House-surgeon, for the following account of it:—"The specimen presented the usual characters of a so-called scrofulous kidney. The kidney-substance was riddled with cavities of various size, each being lined by a thick, whitish, pus-secreting membrane. No healthy structure could be detected anywhere, and no stone was discovered. Under the microscope it was found that almost all trace of normal kidney structure had disappeared, the only remnant visible being a few Malpighian bodies, some of which appeared to have undergone fibroid degeneration. The destruction of the normal texture appears to be due to extensive cellular infiltration of the whole of the intertubular connective tissue, which has encroached upon and destroyed the intervening structures. The infiltration is most marked at the surface of the pus-secreting cavities. Many of the blood-vessels are thickened."

The shock produced by the operation was of short duration, and within a few days it was obvious that much improvement in the general condition of the patient had resulted from it. The quantity of pus in the urine had diminished, though by only about one-half of its previous amount. The ligatures were found loose in the wound on the ninth day after the operation.

A slow but steady improvement in the condition of the patient has continued since this date, and on April 20 (two months after the operation) it is noted

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that the child eats and sleeps well, that she has no pain, and that she looks better and is in much better spirits than formerly. She has gained slightly in weight.

The amount of pus in the urine is less than half of what it was before the operation. On one occasion it has contained a little blood. The wound is looking healthy and healing soundly from the bottom.

The notes of July 15, 1881, record that at this date—five months after the removal of the kidney—a most marked improvement in every way is perceptible. The child has gained in colour, flesh, and strength, is much more lively, and plays about all day long, frequently going out of doors for a walk. The appetite is good, and she sleeps well. The temperature ranges between 98° and 99° F.

The wound is slowly cicatrizing from the edges, and is now about a quarter of an inch only in width. There are still a few sinuses about a quarter of an inch in depth, leading off from the main wound.

The quantity of pus in the urine is less. At the present date it averages about half-an-ounce a day. Six weeks ago it was three-quarters of an ounce. Occasionally, a little blood is passed in the urine, and this is usually accompanied by some pain in the region of the bladder.

The total amount of urine passed is about half-a-pint a day.

Case II.—*A Case of Nephrotomy.*—[For the details of the notes I am indebted to Mr. Hewer, Surgical Dresser.] The patient, a lad sixteen years old, was admitted into St. Bartholomew's Hospital, February 26, 1881, under my care, on account of a large swelling in the region of the left kidney. He had been for some time previously under the care of Dr. Dyce Duckworth. He was in perfect health until about four years before admission into the hospital, when his illness began with a sudden sharp pain, in the left renal region, which obliged him to go home and lie in bed. By the next morning, however, the pain had ceased, and he was as well as ever. A second attack, with like symptoms, occurred a fortnight afterwards; and, subsequently, attacks came on at almost uniform intervals of a week, and these have continued to the date of his admission into the hospital; the attacks lasting usually from eight to sixteen hours, and the pain being very acute and always felt in the same region—that of the left kidney. During an attack a large swelling, fluctuating to pressure, has been perceptible on examination in the painful region. The swelling has however, subsided soon afterwards, and no trace of it can be discovered until the onset of a fresh attack.

Before an attack it is said the urine is clear; immediately afterwards it becomes thick with pus, the thickness then again disappearing gradually, and the urine remaining clear until the patient has suffered from another onset of pain. During an attack he always lies on his back, with the knees drawn up; and while the pain lasts he is said to take no food, nor does he pass urine, even when the attack is prolonged for many hours.

In June, 1879, he was admitted into a medical ward, under the care of Dr. Andrew, and remained there for two months, but without permanent benefit; the attacks of pain and the accompanying symptoms appearing with great regularity.

The general health was somewhat impaired by the repeated attacks; but the patient was able to attend as an out-patient, under the care of Dr. Duckworth until the date of his readmission into the hospital.

March 15, 1881.—At this date, rather more than a fortnight after the patient's readmission, he had a severe attack of pain, and the left renal region being occu-

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pied by a large, fluctuating and tense swelling, I decided on seizing the opportunity and exploring the tumour by operation.

The incisions made resembled those commonly adopted for lumbar colotomy, the wound being enlarged subsequently by making an incision upwards at right angles to the first which lay parallel to the last rib. After exposure of the tumour, which proved to be a largely dilated kidney, it was punctured; and about a pint and a half of pale slightly turbid fluid escaped. [Sp. gr. 1005, neutral in reaction, and containing pus-cells and a trace of albumen.]

The opening was then enlarged, and the dilated kidney was explored as thoroughly as possible by the finger and, afterwards, a metal sound. The lower boundaries, which seemed to extend into the pelvis, could not be satisfactorily reached. No calculus was discovered.

The edges of the wound in the kidney-cyst were now stitched, as to part of their extent, to the edges of the wound in the abdominal wall, and an india-rubber tracheotomy tube was afterwards inserted to ensure good drainage. The wound was dressed lightly with carbolized oil.

Carbolic spray was used during the early stages of the operation, but was discontinued as soon as all danger of wounding the peritoneum was over.

The subsequent history of this case is only one of almost uninterrupted progress towards recovery—that is, so far as the operation and its immediate results are concerned. The patient has completely regained his health and is free from pain. He still wears a drainage tube, through which a considerable quantity of urinary fluid is daily discharged. Quite recently Mr. Buckland, one of the surgical dressers, has arranged a simple apparatus, by means of which the fluid is discharged into a bottle which the patient, when walking about, carries slung to his waist.

Mr. Buckland has kindly furnished me with the following account of the urinary fluid passed through the renal fistula, and of the urine passed per urethram:

Fluid discharged by the renal fistula: average amount (daily) 21 ounces, sp. gr. 1005.

Albumen:—July 22, $\frac{1}{4}$.
July 29, $\frac{1}{8}$.
August 1, $\frac{1}{9}$.

Urea:—July 22, 0·2 per cent.
July 29, 0·1 „
August 1, 0·1 „

Acid, slightly; blood, a trace; sugar, none.

Urine discharged per urethram, average amount daily, 28 ounces, sp. gr. 1015.

Albumen, none.

Urea:—July 22, 1·0 per cent.
July 29, 1·4 „
August 1, 2·0 „

Acid, slightly; sugar, none; blood, none.

Case III.—*A Case of Renal Lithotomy.*—A poorly nourished, rather feeble woman, aged forty-three, was admitted into St. Bartholomew's Hospital under the care of Dr. Dyce Duckworth, in September, 1874, on account of a tumour in the right renal region. "Pulse, 100; temperature, 98° F.; appetite, bad; frequent vomiting in the morning."

The urine was at all times very turbid, and a considerable quantity of pus was deposited in it on standing.

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The right renal region was occupied by a large swelling, which extended to the middle line of the body, and was estimated to be of about the size of a full-grown fœtal head. The swelling was dull to percussion, but not over its whole extent, and very tender. Its size was variable.

The patient's illness began with pain in the right side, accompanied by slight rigors, eight months before her admission into the hospital, when she was in the fifth month of pregnancy. Immediately after her confinement she first noticed the swelling, which was at that time of about the size of a hen's egg. The urine had been very thick for some time before this date.

About a fortnight after the patient's admission into the hospital, I saw her in consultation with Dr. Duckworth and punctured the swelling, drawing off about eight ounces of pus.

Some relief was experienced from the operation; the temperature, which was previously raised, becoming normal, and no pain ensuing. The tumour also became smaller. Three weeks afterwards, however, the symptoms had become the the same as before the operation, and it was determined to explore the swelling more thoroughly.

October, 28, 1874.—An incision was made in the right loin, parallel with the last rib, as for lumbar colotomy, and the tissues carefully divided, until the peritoneum was reached. This was then gently pushed forward and lifted, so to speak, off the subjacent structures, until, having been in this way safely placed out of the way of injury, the outer surface of a tense sac, corresponding to the tumour previously felt, was exposed to view. Only one blood-vessel had required a ligature. The cyst was now punctured with a trocar and cannula, and on the withdrawal of the former a quantity of thick offensive pus streamed out. A sensation, as of the point of the trocar coming down on a calculus, was perceived as it entered deeply into the cyst. The wound in the cyst-wall having been now enlarged with a scalpel, the forefinger was introduced and a large branched calculus was at once discovered. The opening in the wall of the kidney was now enlarged, and the greater part of the calculus was extracted in pieces, with some difficulty. A very free oozing of dark blood from the vascular walls of the sacculated kidney occurred during the removal of the stone, but no hæmorrhage from any special blood-vessel. A small fragment of stone, deeply lodged in an outlying pouch of the kidney, and barely within reach of the tip of the finger, even when pressure was made on the front wall of the abdomen, was left, as the patient began to show signs of failing strength, and it seemed unadvisable further to prolong the operation.

Drainage tubes were inserted, and the wound was syringed out with warm water containing permanganate of potash. No sutures were inserted; the edges of the wound in the abdominal walls being only in part approximated by strapping.

The patient was much collapsed immediately after the operation, and never fairly rallied. She died on the third day after the operation, with no other symptoms (excepting chloroform-vomiting) than those of exhaustion. Her temperature did not rise above 101·1° F.

Permission could not be obtained to make a post-mortem examination.

The calculus removed weighed nearly two ounces, and consisted almost entirely of phosphate of calcium, with much organic matter. Its shape was irregular and characteristic of the situation in which it was lodged—forming, as it did, a cast of the sacculated kidney which had contained it.

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The short time at my disposal will permit only a very brief consideration of the points raised by the foregoing cases.

With respect to Case I., the chief interest, I think, is to be found in the following facts:—

1.—The small amount of benefit, if any, which was derived from merely incising and draining the diseased kidney. The drainage was maintained as efficiently as it is likely to be under similar circumstances; but, notwithstanding this, the quantity of pus in the urine was, after a short period, practically unaltered. The wound, moreover, although granulating freely, showed but little indication, even two months after the operation, of healing or closing in at any part of its boundaries. The exposed kidney seemed, if one may so express it, to act as a foreign body.

2.—The removal of the kidney was made much more difficult by the previous nephrotomy. It is true that, after the boundary line of the kidney-substance was once found, not much trouble was afterwards experienced in the enucleation of the organ. But it seemed, at the time, as if it would be quite possible to miss altogether the outer surface of the kidney; and, in this case, had an attempt been made to separate anything outside the kidney itself or, in other words, to remove the capsule or condensed tissue adherent to the outer surface of the latter, the troubles attendant on the operation would have been multiplied tenfold, and the peritoneal cavity would have been almost certainly opened.

3.—So far as one case can prove anything, this case proves that the operation is greatly facilitated by removing the kidney from within its capsule.

4.—The peril of the operation (nephrectomy) seemed, during its performance, very great; and without such energetic assistance as was rendered at the time, in the performance of artificial respiration, by Mr. Lucas and others, I think the patient would have died. The syncope occurred at about the period at which the ligature was applied; but the child had been much reduced in strength before the operation, and I am disposed to doubt whether, under more favourable constitutional conditions, there would have occurred the shock which made us anxious at this particular time with regard to the life of the patient. It is certain that the effects of the shock, properly so-called, disappeared very rapidly. Twenty-four hours afterwards, the child was very pale and seemed exhausted, but she was quite composed, and on being asked if she wished for anything, replied that she would like a little meat. And within a few days it was clear that she was in a better constitutional condition than before the operation.

5.—The continued presence of pus in the urine, and the occasional though slight attacks of hæmaturia, prove that recovery is not yet complete, and seem to indicate the presence of disease either in the ureter on the affected side or possibly in some part of the opposite renal tract, if not in the bladder itself to the region of which the pain is sometimes referred. It is impossible to decide this question yet. But the case does not, I believe, stand alone in respect of long continuance of symptoms in a modified degree after the operation.

There is one point with reference to the operation itself to which I would draw attention, and that is the advisability of being provided with a special blunt pointed needle, for passing the ligature around the pedicle. The depth of the wound and the narrow space in which one has to work render an instrument of the sort more necessary than would be believed by one who had not had occasion to perform the operation.

With regard to Case II.—that of nephrotomy for hydronephrosis—I have but

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little to say. The relief has been very great. All pain has disappeared, and the lad's health has been restored. Sooner or later, however, it will be necessary to explore again the condition of the large urinary cyst which still remains, and to decide on the desirability of leaving matters as they are or of extirpation.

Case III.—that of renal lithotomy—illustrates only too well the danger of removing a large branched calculus from the kidney, even when the operation has not been complicated by a wound of the peritoneum or other mishap not of necessity attendant on the operation. It is true that the patient was in this instance but a weakly subject, and a better result might have accrued had the operation been undertaken at an earlier period of her illness. But I believe any operation for the removal of a stone similar to that which was taken away from this patient must be necessarily very perilous, both from shock and, perhaps still more, from the loss of blood from the inner surface of the dilated and vascular kidney. Of course these remarks would not be applicable to any case of renal calculus of small or even moderate size. But, in the case of a large branched stone, I am disposed to think that it would be a safer procedure to remove the kidney forthwith, with the calculus, than to make a prolonged attempt to remove the stone. And this seems more especially advisable when one considers the diseased condition of the kidney, of which the stone is, perhaps, rather a representative than a cause. One case is, of course, of but little value in determining a surgical procedure, and can prove nothing. But the present case may help with others in establishing the right method of treatment in these cases, so irremediable except by surgical operation, and for this reason it seems right to place it on record.

Some points in connection with Operations on the Kidney.

Mr. Arthur E. J. Barker, London.

The following questions are the basis of this paper:—

1.—How early in the course of its formation can a calculus in the kidney be accurately diagnosed?

2.—How early is it justifiable to operate on the organ for this condition?

3.—In what should such an operation consist; whether in simple incision of the kidney, with extraction of the stone, or in nephrectomy?

4.—How late is it justifiable to attempt nephrectomy for renal calculus?

The importance and difficulty of answering these questions decisively have been keenly felt by the author in several cases already recorded. Firstly, because though renal calculus is often the cause of distressing and even rapid and dangerous disease, instances are familiar to us in which stone has lain in the kidney for years without either much suffering or danger. In the first class of cases early diagnosis would be most desirable with a view to operation, and in the latter two in order that we might at once decide not to interfere. Manifestly, then, we must set ourselves to establish rules for diagnosis between these two classes. Again, a small stone often causes intense and chronic distress, but little damage to the kidney, while a large one may produce complete destruction of the organ with but slight suffering: and *vice versa*.

To the operator, then, two groups of cases are at once suggested:—(1) Early

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calculous disease with little or no disorganization of the kidney. (2) Stone, with much damage to the organ and perinephritic structures. Admitting gradations between these two groups, it is nevertheless essential that, from an operator's point of view, they should be considered as well defined, for reasons which perhaps I may be allowed to formulate briefly.

In the first our patients are generally younger, therefore less likely to have concomitant diseases. In them the remaining kidney, after destruction of one by disease or operation, probably hypertrophies more readily than in those who have passed maturity. Again, the perinephritic tissues are still healthy. If the organ be incised or removed, then the latter are far more likely to heal kindly than if deeply implicated in inflammatory disease, as in the second group. Further operations at an early stage are easier, more rapid, and less bloody than later. Finally, the first group of patients have not been worn out by suppuration and fever, as is too often the case with the second.

These patients are, as a rule, older and more liable to concomitant diseases. If operated on, the state of chronic inflammation or actual suppuration of all the tissues dealt with renders the probability of septic sequelæ far greater than where all are sound. The other kidney is here less likely to be healthy. Again, in some cases, the pedicle of the organ is so encased in tough, fibrous inflammatory tissue that it cannot be isolated with any reasonable expenditure of time and force, and safe removal of the kidney is consequently impossible. An incompleted or exploratory operation is here almost as bad as nephrectomy, in view of the condition of the perinephritic structures. Finally, these patients have freqently suffered from prolonged pain, suppuration, and fever.

Theoretically, then, early operations on the kidney for calculus hold out far better prospects than if performed later. And experience confirms this deduction, whether we examine nephrotomy or nephrectomy. The brilliant cases of Professor Simon, Professor Czerny, and Mr. Henry Morris could never be compared with the slow, difficult operations and tedious recoveries in those interfered with at a later stage of the disease. As to nephrectomy generally, from a study of about sixty cases of the operation for various causes now before me, it is clear that the procedure has been most fortunate among young people, specially so in children.

In answer, then, to Question 1, the above cases may first be cited. In these, early operation demonstrated the presence of small calculi in fairly healthy kidneys. In Professor Simon's case nephrectomy was performed on account of the pain produced, but even when he had cut down upon the organ, and had it actually under his fingers, he could hardly convince himself that there was a calculus present. On removing the organ the correctness of his first diagnosis was proved. The same precisely may be said of Professor Czerny's case, which was crowned by brilliant success. But a clearer answer still is to be found in the very perfect diagnosis made by Dr. Sidney Coupland and Mr. Morris in the case in which the latter removed a small stone from a healthy kidney with complete success, and without producing a urinary fistula in the loin. The importance of this case cannot be over-estimated.

But all physicians and surgeons of experience are familiar with instances where, from well-marked subjective symptoms, with perhaps a little hæmaturia, they are satisfied that a calculus exists in the kidney, though unable to discover any enlargement of the organ, notable pus formation, or other objective evidence of advanced disease.

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Now, as an aid to diagnosis in any doubtful cases of early disease where the suffering justified interference, I would venture to suggest a free antiseptic exploratory incision in the loin until the kidney is reached; that the latter should then be examined with a fine needle thrust into its convexity at various points, and aimed towards its pelvis. This procedure I have found of use myself. It has the advantage over simple puncture through the loin without incision that the whole kidney can be accurately and systematically explored with the needle, as it lies under the finger. There appears to be no reason why this should not be done in the case of fairly healthy kidneys, gently of course, but freely. The uninflamed tissues around do not bleed much, as I can testify from experience of my first nephrectomy. Nor, is it clear how repeated puncture of the organ with a very fine needle, well directed, could injure it. If this exploration should negative calculus, the lumbar wound could be closed again and would not be found to be serious. Ordinary palpation in early calculous disease often affords only negative evidence. The kidney lies high up, almost beyond reach. But when the stone is large the organ will generally be found lower than usual and well within reach of the hand.

In answer, then, to Question 2, I would venture to say the earlier an operation is done the better, provided the symptoms are clear and the patient's distress marked and increasing. Great caution, of course, is necessary in deciding in such cases. They should be watched closely for some time to see whether the symptoms are aggravated or relieved. If the former, and especially if the organ begin to descend towards the pelvis and to become very tender, there should be no hesitation in making an exploratory incision as above, and in removing the stone or whole organ if necessary. If, on the other hand, the distress is trifling, not interfering with exertion and diminishing in course of time; if the kidney be not clearly felt with the hand, and the latter discover little tenderness, we may wait, still watching the case for any return of pain, tenderness, descent, and enlargement of the organ.

In answer to Question 3, I venture to think that the whole matter is much simplified, if we adopt the idea of the antiseptic exploratory lumbar incision, with careful but, if necessary, free puncture of the organ. If by these means the latter is shown to contain several small or one large stone or to have its pelvis encrusted with calculous matter, it should, I submit, be removed *in toto*. If, on the other hand, a small single calculus were found either in the substance of the kidney or in its pelvis, it could be removed by simple incision of the organ as in Mr. Morris's case. A clean cut into the substance of the kidney along its convex margin does not necessarily produce severe bleeding, as Professor Brandt's case proves, and as I have seen in one of my own operations. Such hæmorrhage, too, has been shown to be easily arrested by pressure.

As to Question 4, it is more difficult to answer than either of the other three. Between nephrotomy with extraction of stone and nephrectomy there is, I venture to assert, but little ultimate choice in very advanced calculous renal disease, if I have interpreted recorded cases aright after careful study. Nephrectomy appears particularly formidable late in the disease, both as regards the mere procedure itself, and in its effect upon the economy for reasons already briefly given, and for others which, though interesting, are beyond the scope of this paper, but especially because these patients are generally older. It is a significant fact, as far as the numbers go, that, out of the cases of nephrectomy for stone in the kidney which I have been able to collect, seven died; all of whom were above thirty years of

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age. The three patients who survived were all under twenty-four. Nephrectomy for other causes, however, has been performed successfully on individuals as old as fifty-three.

If permitted to attempt an answer to the question myself, it would be this. Should the affected kidney be still only moderately enlarged, very movable and very hard, giving no sense of fluctuation; if there be but little tenderness or palpitation over it, and the pus passed *per urethram* be fairly constant in quantity, not frequently and suddenly disappearing, while severe attacks of pain, swelling of the tumour, and rise of temperature set in, to reappear with relief of these symptoms, the case is less unfavourable for operation, provided the patient be not too old and be in fairly good health. But if the organ be much enlarged, fixed in the abdomen, soft and doughy or fluctuating; should there, above all, be much local tenderness on pressure, rise of temperature synchronously with attacks of pain and swelling in the tumour, I submit that the case is most unfavourable for primary nephrectomy even though the patient be young, much more if he have passed maturity.

The question of secondary nephrectomy, after previous antiseptic incision into the organ and prolonged drainage under continued antiseptic treatment, is, however, well worthy of consideration, but is beyond the scope of this paper.

A Successful Case of Nephrectomy.

Mr. R. Clement Lucas, London.

H. L., aged 36, was admitted into Guy's Hospital on November 26, 1879. He was a fair-haired, spare man of average height. His father died of phthisis.

He was at one time a sailor, and eighteen years ago was in India, where he suffered from fever. Of late he has been employed as a bricklayer or dock labourer.

History of the present disease.—On the 2nd of September, 1874, he was admitted into a medical ward of Guy's Hospital, suffering from pain in the back and in the region of the left kidney. He first felt the pain three months before. There was a history of his having fallen from a height on to a brick wall some nine months before, but from this he does not appear to have suffered much. His urine was found to contain pus and albumen; but he had never passed blood, nor did he pass any during his stay in the hospital. A swelling appeared in the left loin which was opened, and eight ounces of fœtid pus escaped. He left the hospital a few weeks later, some pus still escaping from a sinus in the loin, and some being still discharged with the urine. The diagnosis made at this time was scrofulous kidney with perinephritic abscess.

The sinus never healed completely, but continued to discharge during the five years intervening between his leaving the hospital and his second admission. About seven weeks before he again applied for relief, urine commenced to escape through the sinus, and the pain and discomfort occasioned by this, together with the frequent and painful micturition, caused him to give up work. There was no history of his having passed stone or gravel. His urine generally contained much sediment, which was occasionally streaked with blood.

Condition on admission in 1879.—He is very weak, and much wasted. There is a sinus in his left loin, situated one inch below, and one inch behind, the end of the last rib, through which urine and pus escape. He has great pain in passing

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water, and still more after he has passed it, the pain lasting for about twenty minutes. He can only pass it in the sitting posture. The urine contains pus.

He requires to pass it three or four times during the night, and frequently, and in small quantity, during the day. His temperature is normal, tongue uniformly red; heart, lungs, liver, and spleen healthy. A sufficient quantity of urine and pus passes from the sinus to keep him uncomfortably wet. A flannel bandage and pad are used to soak it up.

December 10, 1879.—About 11.30 P.M. yesterday, the patient experienced violent pain in the kidney, groin, and urethra, which continued till 5 A.M. this morning, since which time urine has ceased to pass from the sinus. There is an increased quantity of pus in the urine. At 10 A.M. urine commenced again to pass through the sinus.

December 12.—He is unable to get any rest at night, owing to the pain. He was ordered morphia, gr. ¼, by hypodermic injection, every night.

December 31.—He has remained in much the same condition, with occasional exacerbations of pain, generally to be attributed to some stoppage of the sinus.

In January, 1880, his urine was more completely examined, and the quantity passed in twenty-four hours recorded. On Jan. 7 he passed 59 oz.; Jan. 8 50 oz.; Jan. 9, 44 oz.; Jan. 10, 48 oz.; Jan. 11, 52 oz.; Jan. 12, 60 oz.; Jan. 13 68 oz. The sp. gravity varied from 1017 to 1020; was acid, contained lithates and a considerable sediment of pus. On Jan. 14 the sinus stopped discharging, and he passed 49 oz. of urine. It was closed on Jan. 15, and he passed 53 oz. On the 16th the sinus reopened, and only 38 oz. were collected, but some doubt is expressed as to whether all was measured on this date. On the 17th he passed 44 oz.; on the 18th, 45 oz.; on the 19th, 48 oz.; on the 21st, 70 oz. of urine; sp. gr., 1017; on the 22nd, 58 oz., 23rd, 50 oz., &c. Judging from the quantity and character of the urine passed, that the right kidney must be in a fairly healthy condition, whilst the suppuration which had existed nearly six years in the left kidney could scarcely have failed to destroy the greater part of its secreting substance; finding, too, that the stopping up of the sinus in the loin made no material difference in the quantity of urine passed, I determined, after watching him three months, to attempt to remove the diseased organ.

The operation was performed on February 17, 1879. The patient was placed on his right side, with a pillow beneath the loin, and ether administered. A vertical incision was then made through the sinus, from the last rib to the crest of the ilium. The muscles were divided till the abdominal cavity was reached. The lower end of the kidney could now be felt, but it could not be separated from the lower ribs and parietes, on account of extensive adhesions. The capsule was opened, and the adhesions were cut through with a curved, probe-pointed bistoury. When the pedicle was reached an indurated lump was felt, which proved to be the upper end of the ureter, enormously thickened. It was found necessary to enlarge the wound transversely above, before the pedicle could be secured. Carbolized silk ligatures were used, and the vessels were tied apart from the ureter, which was afterwards ligatured. The transverse wound was closed with wire sutures, the vertical being loosely brought together to prevent the protrusion of intestines. Antiseptic spray and dressings were used. He recovered quickly from the effects of the ether and shock of the operation, and at 9 P.M. his temperature was 98·2. He was slightly sick on waking from a short sleep. At 10 P.M. he was redressed, as there was slight oozing from the wound. On the following day his temperature

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rose to 102·2° in the morning, and 102·4° in the evening; but he felt well, and passed 28 ounces of urine, of the sp. gr. 1022. On the second day he passed as much as 48½ ounces of urine, of sp. gr. 1020, in twenty-four hours. His temperature fell to 100·8° in the morning, and 101·6° in the evening. On the third day he complained of pain in the side and groin, and lay with his left leg drawn up. After being dressed his temperature was 102·1°, but in the evening it fell to 100·4°. He passed 43 ounces of urine of sp. gr. 1018. On the fourth day the urine was noticed to be dark-coloured, from carbolic dressings, but they were, nevertheless, continued. His temperature was 100°, morning and evening. On the fifth day after the operation his temperature was normal. He passed a motion, which caused him great pain in the side and groin, relieved by an opium pill.

From the fifth to the fifteenth day he progressed favourably, his temperature being normal in the morning, and rising to 99° or 100° at night. The amount of urea excreted in the urine was calculated on several occasions, and varied from 225 grains to 337·60 grains in twenty-four hours; pus remained. He was able to eat chicken and rabbit. On the fourteenth day a little blood was noticed on the dressings, and attributed to his straining at stool. On the fifteenth day hæmorrhage occurred after the dressing, and seemed to be the consequence of the ligature having been dragged upon in lifting him. He was redressed, and a morphia injection administered. The bleeding recurred at 5 A.M. on the sixteenth day. The wound was redressed and plugged with gauze, and morphia again administered subcutaneously. Hæmorrhage recurred during the day, and in the afternoon I opened up the wound, under chloroform, and attempted to slip a ligature along the old ligatures and re-tie the pedicle. This appeared to check the hæmorrhage, which welled up from the bottom of the wound. Another injection of morphia was afterwards administered. On the seventeenth day, hæmorrhage having again occurred, and the patient being in a most precarious state from loss of blood, I plugged the wound tightly and forcibly with two large sponges, steeped in perchloride of iron, and bound the abdomen tightly round with a flannel bandage. This completely succeeded in stopping the hæmorrhage. He was ordered a quarter of a grain of morphia, subcutaneously, every six hours, for twenty-four hours.

March 6. He slept fairly well at intervals, passed 43 oz. of urine, temp. normal.
„ 7. Slept well, temperature 96·6 to 99·4. Had two injections of morphia.

„ 8.	Temperature:	M.	97·8°	E.	99·0°	Urine:	42 oz.
„ 9.	„	„	97·8°	„	99·8°	„	30 oz.
„ 10.	„	„	97·8°	„	99·4°	„	36 oz.
„ 11.	„	„	97·0°	„	98·6°	„	
„ 12.	„	„	98·8°	„	99·4°	„	34 oz.

I commenced to remove the sponges by cutting away the protruding part. Patient gaining strength, and eating better. No recurrence of bleeding since the plugging.

March 13.	Temperature:	M.	98·0°	E.	99·4°	Urine:	38 oz.
„ 14.	„	„	99·0°	„	102·0°	„	36 oz.
„ 15.	„	„	97·8°	„	100·6°		

„ 17. 98·2. Was sick, and had an opium pill.
„ 18. The greater part of the sponge has been cut away.
„ 19. Temperature: M. 101·4°. E. 103·4°. Sick and complains of pain.
The remainder of the last sponge and the ligatures were removed.

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March 20. Morning temperature 99·4° E. T. 101·3°. He had a slight rigor.
" 21. " " 99·2° " " 100·2°. Progressing favourably.
" 22. " " 97·6° " " 100·2°.
" 23. " " 98·1° " " 100·6°. Progressing favourably. No hæmorrhage since removal of sponges.
" 24. Morning temperature 99·0°. E. T. 99·2°. Granulations look red and healthy. Wound rapidly healing.
" 25. At 7 P.M., hæmorrhage again took place, consequent upon his straining to pass a motion. The wound was again plugged by the house-surgeon. Temperature 97·1°. Evening temperature 98·1°.
" 26. Slight hæmorrhage occurred at 8 A.M., and more 3.30 P.M. Patient much weaker.
" 27. Patient more comfortable, but has lost appetite. Morning temperature 97·2°. Evening temperature 99·8°. Severe hæmorrhage occurred at 1.15.
" 28. Temperature 98·8°. Passed 46 oz. of urine, at 5 P.M., dressing saturated with blood. Fresh sponges steeped in perchloride of iron were inserted.
" 29. Temperature 98°. Evening temperature 99·8°. Patient much exhausted, wandering, especially restless during sleep.
" 30. More comfortable. Outside sponge removed, and a fresh carbolized sponge put in its place.
" 31. Vomited an egg. Fresh sponge on the outside. No bleeding. Patient very restless during sleep.

April 1.	Temperature 97·8°	Evening temp.	99·4.
" 2.	" 99·0°	" "	101·6.
" 3.	" 97·8°	" "	99·4.
" 4.	" 99·4°	" "	100·8.
" 5.	" 98·4°	" "	99·4.

The sponges were removed without any recurrence of hæmorrhage to-day. There was no hæmorrhage after this date.

April 6. Temperature 97·8°. Evening temperature 98·6°. Urine 44 oz.
" 7. He sat up to-day for twenty minutes and was not fatigued.
" 8. Sat up thirty-five minutes.
" 19. He has been gradually improving, his temperature remains about normal; to-day, he was carried into the garden.
" 20. His temperature to-day gradually rose, and at 7 P.M. reached 104·2°, followed by sweating and rapid fall of temperature. He did not get up to-day.
" 21. He passed 60 oz. of urine. Morning temperature 97·2°. Evening temperature 101·8°.
" 22. Morning temperature 97·6°. Evening temperature 100·4°.
" 23. " " 98·0° " " 98·6°.

He gradually gained strength during May, and was removed into a general ward.

On June 25, a sinus still remaining, under chloroform I explored the wound, and found the sinus travelled along the ureter. A drainage tube was inserted in this direction. No constitutional disturbance followed this operation.

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On July 2, the drainage tube, having caused him pain and discomfort, was removed. He remained in the hospital till the end of June, with a discharging wound, but able to get about a little.

At the end of June my friend Mr. Couling, of Brighton, very kindly took him into the Brighton Hospital, where he remained two months, and derived great benefit from the change and treatment he received there. The sinus however continued to discharge, and he still passed pus in the urine. He came back to London in October, and, after a short stay in Guy's, went home. He then began more rapidly to gain strength as less pus escaped from the sinus and less was passed in the urine. It was about a fortnight after Christmas that the sinus finally soundly healed. Since then he has been able to do a little work, and has grown much stouter, and is quite free from pain.

The kidney removed was much puckered on the surface, and on section was found to have its secreting substance almost entirely destroyed and occupied by suppurating cavities. One of these communicated through the puckered cortex with the sinus, and allowed the contents of the cloaca to escape through the loin. The ureter was greatly thickened. The man left his work as a bricklayer to show himself to the Members of the Congress. The wound is soundly healed, and the urine remains free from pus.

Lumbar Nephrectomy for Nephrolithiasis.

Mr. Richard Barwell, London.

I have twice removed the kidney. My first case (for pyonephrosis) turned out to be tuberculous. The girl, aged 16, appeared for thirty-six hours to be benefited but afterwards she became weaker, and sank on the sixth day.

The second case, the subject of this paper, was operated on for nephrolithiasis. The symptoms which to me appear to justify, or rather to call for, this operation are—pain, nausea, hectic, and continuous emaciation; also remittent hæmaturia and increasing pyelitis. When these symptoms are marked, I believe nephrolithotomy will (unless the stone be quite small) prove useless or only temporarily useful, and that nephrectomy should be the operation chosen.

October 12, 1880.—Dennis Fenton, aged 18, juvenile and undeveloped for that age, came under my care in Charing Cross Hospital. The lad was of healthy parentage, and has suffered from no illness except measles. At the end of July he suffered severe paroxysmal pain about the left loin, frequently having to sit down and bend himself to get relief. Six weeks ago—*i.e.*, about the beginning of September—he first observed a swelling in the left loin.

October 14.—During the night, and previously to my seeing him, the lumbar swelling burst, discharging a small quantity of thin watery pus.

October 15.—I examined him carefully. A probe passed into the orifice of the abscess went upward, forward, and a little inward to a depth of four inches; it impinged only on soft parts. The swelling occupied a considerable space, being a general enlargement of all parts about the loin. There was dulness at the lower chest behind, running up to the sixth rib. Respiratory murmur, though rather distant, was distinct. The urine was normal in quantity, sp. gr. 1015, was cloudy, with muco-pus, therefore (or also) containing some albumen.

October 18.—I made an incision as for lumbar colotomy, and let out at least

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six ounces of pus, and passing in my finger felt the kidney, which was entire, but appeared to contain some hard substance set down as in all probability a stone.

The rest of the case at this time consists in watching the lad's fluctuating condition. In the middle of November about one-eighth of the urine in a glass was a pus deposit. The wound was re-opened. No permission for further operative procedure could be obtained. Under a system of rest and diet his general condition improved, and, giving him strict orders to show himself every week, I dismissed him on the 28th of January, 1881.

The boy Dennis, fairly comfortable and gaining weight during the last six weeks of his stay in the hospital, began to suffer as soon as movement and the less careful diet procurable by his parents recommenced. A free discharge from the lumbar sinus set in, he lost flesh, and became pale and anæmic.

March 29.—He was re-admitted into hospital. The pus deposit in the glass varies from one-eighth to one tenth, otherwise the secretion was normal. He suffered considerably, referring the pain to the front of the abdomen on a level with the umbilicus at the edge of the left rectus. The temperature chart was very uneven, up and down—the hectic temperature.

April 10.—I had sounded the sinus twice unavailingly; at this date a more fortunate direction of the probe among numerous passages enabled me to detect a stone.

May 5.—Leave for operative measures being at length obtained, I determined to remove the stone from the kidney or the gland itself, according to circumstances. Two conditions would, I foresaw, render either procedure difficult—viz., the thick semi-cartilaginous scar-tissue about the site of operation; and the remarkable proximity of the twelfth rib to the crest of the ilium—indeed, there was in the erect posture hardly more interval here than between any two ribs. In order to eliminate this latter difficulty as much as possible, I placed the boy, when etherized, across a firm, thick bag of sand, bending the lumbar spine forward and to the right.

I made an incision (not including the sinus mouth, which lay too near the spine) obliquely downward, and a little outward from the lower border of the eleventh rib to the ilium, and, drawing the skin apart, endeavoured to recognize the edge of the erector spinæ, but the parts were all converted into tough amorphous scar tissue, and the usual guide to the kidney was lost. The sinus appeared to pass through the thickness of the muscle. I enlarged the orifice a little, and, pressing my finger along the passage, felt the stone within the kidney. Passing my knife along the finger as a director, I opened the parts to the same extent as the outer wound; and then, drawing it along the lower border of the last rib, I made an angular flap, which, being retracted, gave a certain, though still restricted, room. I now passed in a pair of forceps, and incising the gland a little to facilitate its extraction, endeavoured to remove the stone; but it broke down in part, and the rest was evidently too deeply embedded; it could not be safely removed. Up to this time no appreciable amount of bleeding had taken place, but now blood began to well up pretty freely from the depths of the wound. I dreaded any continuance of this in so anæmic a patient. Therefore, placing a small piece of sponge on the wounded part of the kidney, I requested Mr. Morgan to make pressure on it with one finger. Deeming, under these pressing circumstances, boldness to be the best discretion, I very quickly (not in the slow and gentle manner so strongly inculcated by G. Simon) partly enucleated, partly broke up the gland. As soon as I thought enough had been done for my next purpose, I passed over in front of the kidney a stout piece of carbolized silk, drew

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it as far as possible towards the aorta, and tied it in a firm knot. Now, when Mr. Morgan removed the pressure, no bleeding followed, and I could complete my work more leisurely; this was only to carefully and slowly peel away all the inner part of the gland, leaving here no part attached to its surroundings, until the pedicle could be clearly and distinctly felt. In doing this, I was obliged to leave a small portion at the upper part of the kidney still attached; it was impossible to separate it, without incurring risks that seemed to me unjustifiable. I therefore broke this portion away. Another piece of silk was then passed over the separated gland, drawn well in towards the aorta, and tied round artery, vein, and ureter, *en masse.*

The next point, the extraction of the kidney through the narrow space between the twelfth rib and the ilium was evidently impossible. I passed into the depth a pair of strong scissors, cut the gland in two towards the ligature, and, having verified bloodlessness, severed each half from the pedicle, and remove it separately. One vessel in the depth, probably aberrant from the renal beyond the ligature, required tying. A sponge wrung out of carbolic solution was placed in the large hollow. The whole operation was performed antiseptically, and the dressings were carried out on the same principle.

On the night of the operation, the temperature was 101·8°; this was only one-fifth of a degree higher than it had been two nights before. Some fragments of stone came away on the dressing next day, and two days after, a remarkable calculus, was discharged; this was quite circular, the size of a sixpence, and thicker in the middle than at the edge. I gave this in charge to the house surgeon, who, in spite of my strict injunctions, lost it. The parts of stone actually collected weighed 134 grains, but so much was washed away at the time of operation and carried off in sponges that I should estimate its whole weight at double that amount.

The morning after the operation the urine contained a quantity of pink (blood-stained) albumen (one-sixth), quantity 32 ounces, sp. gr. 1013.

Further there is little to relate. The diet was regulated; acid, afterwards with quinine, was given, the albumen decreased, not steadily, but satisfactorily; temperature within a week became normal.

June 20.—Forty-sixth day. The wound had all but filled up, the ligatures remained, and kept up a certain suppuration. Also hanging out of the wound was a semi-decomposed fibrous thread, evidently the ureter which seemed to be daily more extruded. On the above date I found that the ligature only included this partially sloughed thread. By carefully drawing on it, I extracted it; it did not come away at the point tied, but about an inch beyond, altogether four and a half inches.

The boy is practically well: the urine contains now no trace of albumen. His temperature is normal and regular; he is getting fat, and is as ruddy as long confinement to bed in a hospital ward will allow. He has a sinus discharging, but very little comes from it.

DISCUSSION.

Dr. F. A. Rosenberger, Wurzburg: In connection with the foregoing communication I wish shortly to refer to a similar case, in which, although I am sure that the mischief arose from renal calculus, I am not certain whether the very considerable swelling of the abdomen depended upon a pyonephrosis, or upon an abscess communicating with the ureter or the pelvis of the kidney. The lumbar

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incision for the removal of the kidney was therefore only made tentatively. A fellow practitioner, thirty-seven years of age, and of good constitution, was attacked with severe colic in the left renal region, accompanied by fever, and the formation of a swelling in the abdomen., which became at last of considerable size. There was no lumbar swelling; the urine at times contained pus. In the previous year, a preliminary puncture, followed by an incision behind the anterior superior spine of the ilium, led to the evacuation of several pints of very offensive matter. A drainage tube was inserted. Healing, however, did not take place; but a constantly increasing discharge of pus, mixed with fragments of stone, went on. Loss of appetite followed, accompanied by emaciation, hectic, sweatings, a dry cough, a right pleurisy, and consolidation at the apex of the right lung. The patient, at last, when he was reduced to a skeleton, determined to allow a lumbar incision to be made, in order to drain the sac, with the view of at last having the kidney extirpated. An incision was made, under strict antiseptic precaution, from the last rib to the crest of the ilium, but, instead of finding the kidney, a fluctuating surface was come upon, occupying all the deeper parts of the wound. As it was clear that the case was one of pyonephrosis, a long incision was made into the mass, and about a pint of matter similar to that evacuated at the first operation was let out. The cavity was washed out with a solution of carbolic acid, and a drainage-tube was passed from the old sinus to the operation wound. All the symptoms from this time diminished, and eventually, including the pleuritic effusion and the dulness at the apex of the lung, disappeared. A small opening at this portion of the drainage-tube alone remained; the rest of the wound healing by first intention. At the present time a small drainage-tube remains in the sinus; but the patient has regained his old healthy appearance, and is able to apply himself to his practice as before.

Dr. Langenbuch, Berlin: I have made my incisions on the outer border of the rectus abdominis in both my successful cases of nephrectomy, by the abdominal incision. My reason for doing so, in the case of the right kidney, is the fact that the inner layer of the mesocolon covers the vessels passing to the ascending colon, whereas the outer layer is almost free from vessels. I recommend this position for the first incision, in order to enable the operator to open the fat round the kidney through this outer layer of the mesocolon, and so avoid the risk of hæmorrhage.

Dr. A. Martin, Berlin: I have performed eight nephrectomies, one for a malignant tumour (recovered, and free from recurrence for two years and a half now): seven for floating kidneys (four cured, three deaths). These two are perhaps the most frequent indications for this operation. Floating kidney often does not kill, but in some cases gives rise to unbearable suffering. As to the diagnosis, the urine is generally unchanged, percussion seems to be but of little value; the best way to find out the nature of a floating tumour on these parts is the method of Freund—*i.e.*, to place one hand below the costal margin behind, and press up, whilst the other hand compresses the tumour from the front. I have always preferred the median anterior incision—the regular laparotomy incision, and have gone through the peritoneal cavity to take out the tumour, incising the posterior layer, and peritoneum—always having previously ascertained for certain that the other kidney is in its natural position and healthy. The urine is diminished immediately after operation, by one-half; but in a few days the one healthy kidney so

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increases in its functional power, that at the end of the first week after the operation this one organ secretes the normal quantity of urine. Recovery was complete in the five cases which survived the operation. I believe that in cases in which much general disturbance is caused by one kidney, it is right to remove it, as soon as all other means have been tried and have failed to give relief. Sufficient evidence is now before us to prove that a man can live in perfect health with but one kidney.

Recent Advances in the Methods of Extracting Stone from the Bladder.

Sir Henry Thompson, London.

It will be taken for granted at the outset, that the subject defined in the foregoing title is intended to include any improvement in operating either by Lithotomy or by Lithotrity.

And, first, I think it may be affirmed that, of the two methods named, Lithotomy does not afford so much scope for the consideration of improvements as the operation by crushing. Dominated as lithotomy must be, for the most part, by anatomical conditions which do not change, and perfected as it has been by the experience of centuries, the slight modifications of procedure occasionally proposed are often due rather to the idiosyncrasies of different operators than to the discovery of any new or important fact. For the most part, the plan of incision known as the lateral method (*la taille latérale*) is adopted throughout Europe, America, and the East, with slight variations in different localities; an important exception being Paris, where median operations, in the form of medio-bilateral or pre-rectal, are mostly preferred. There is one modification, however, which requires to be noticed, relating rather to the mode of dealing with the stone itself, than to the method of making the incisions. Doubtless the success which lithotrity achieved has led to the proposal, which in varied forms and in several quarters has been made—viz., to combine a limited degree of incision with the crushing operation; a plan which was more systematically pursued by the late Dr. Dolbeau, of Paris, than by any other surgeon. Dolbeau's method, which he termed "perineal lithotrity," consisted in making a median incision in front of the anus, limited to the urethra; in forcibly dilating the neck of the bladder by special instruments; then crushing the stone by forceps introduced through the wound, and abstracting the débris by that route. It is worthy of note that Dolbeau recommended this procedure because "he knew by experience how great is the relief which follows extraction of all the calculus at a single sitting."* And his method is one which, somewhat modified perhaps, and in exceptional circumstances, as when the urethra is unduly narrow, may offer some advantages. But serious risk may be incurred by much manipulation for the purpose of removing débris in an empty bladder, as most lithotomists have observed, when a large stone has chanced to break in the process of extraction. For it is a fact of practical importance, hitherto not sufficiently noted, but which I desire to affirm in the following formula—viz.,

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* "Pour ma part, je recommanderais cette pratique si je ne savais par expérience combien est grand le soulagement qui succède à l'extraction d'une pierre faite séance tenante."—*De la Lithotritie périnéale*, p. 42. Paris: 1872.

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That the act of pushing forceps upwards through a perineal opening into an empty bladder, in order to seize fragments, is more likely to injure the bladder than the act of seizing them with a lithotrite introduced downwards by the urethra into an unopened bladder. Hence it has been proposed to break a large stone into fragments by means of a powerful lithotrite before performing lithotomy, the incisions required for which are then less extended, and therefore less dangerous, than they would be if the stone is to be removed entire. I can easily perceive that such a proceeding might sometimes be adopted with advantage, and that it is worthy our consideration. Of late, when removing an unusually large calculus by the lateral operation—say of 80 grammes (2½ oz.) or upwards—I have occasionally crushed it *in situ* by powerful forceps, rather than make a wound sufficiently large to allow me to extract it unbroken;* and I am not indisposed, when opportunity offers, first, to crush a large and hard stone with a powerful lithotrite, and then remove the fragments by forceps through a perineal opening, rather than by an evacuating sound in the urethra, especially if this canal be of small or even only average size.

There is, however, at least one exceptional condition, in which the operation of lithotrity and a median perineal incision of the urethra only, may be combined with manifest advantage, and to which I desire to call your attention. Three years ago I had a case of calculus in a patient aged sixty-seven, whose prostate was of enormous size, and who for some years had removed all his urine by catheter. This he was compelled to do every hour and a half, day and night, owing to the irritation present. In this condition I first crushed and removed most of the calculus by ordinary lithotrity; I then at once placed him in the position for lithotomy, made a small median opening into the membranous urethra, dilating it with my finger, and by means of small lithotomy forceps, extracted some remaining fragments, so as to empty the bladder completely. After this I introduced a short but large gum catheter into the bladder by the perineal wound, and fixed it there; this gave him perfect relief, for he was unable, in common with most patients suffering from hypertrophied and inflamed prostate, to tolerate a catheter tied in by the urethra; but by the means described he had complete repose, and experienced no unfavourable urinary symptom whatever. My friend, Mr. J. Morgan, of Chapel Street, London, and Mr. Brewer, of Huddersfield, were present and assisted me at the operation. I have no hesitation in regarding this combination of the two proceedings, in the circumstances described, as a valuable addition to our resources in some cases, not very uncommon, where the prostate is large, the bladder contracted, and ability to pass urine by the natural efforts is lost.

Before I quit the subject of lithotomy, let me briefly call attention to the value of an instrument, devised by my friend Mr. Buckston Browne, for arresting hæmorrhage. He has appended to the ordinary lithotomy tube an india-rubber bag, which, introduced in the flaccid state, and subsequently inflated with air, closes at once any source of hæmorrhage distant from the surface, the urine flowing meantime freely through the tube. I know no agent so certain and secure in its action, and as I think no man should perform lithotomy without one of these instruments at his side, it is named here as an important adjunct to our resources.

The history of lithotrity, which extends now to little more than sixty years,

* A new model for such forceps, designed by the author, was shown.

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is, on the other hand, a history of continuous change. The most important epoch in its progress, however, dates from the advent of anæsthesia. Up to this time the application of lithotrites and evacuators was necessarily made subordinate to the patient's powers of endurance, the performance being limited by the short period of time at the disposal of the operator, and by the size of the urethra, which could not be much distended without producing acute pain. Indeed lithotrity in its most modern form would not be possible without anæsthesia. But after the introduction of chloroform the surgeon ventured to crush more largely and remove débris more freely than before. Fergusson systematically removed the fragments chiefly by repeated introductions of the lithotrite, and without evacuating catheters; and others who disapproved of that proceeding used the aspirator designed by Clover, and tubes of the natural calibre of the urethra. I may be allowed, perhaps, to say that the first time that instrument was ever used was by myself, Mr. Clover having placed his first instrument in my hands for the purpose. The consequence was, that a calculus of small size was now removed at one sitting instead of at two or three sittings, as before the employment of anæsthesia.

But during the last twelve or fifteen years our American brethren have adopted, chiefly in dilating the strictured urethra, particularly Dr. Otis, of New York, larger instruments than have before been usually employed for the purpose, and they have shown that in a large proportion of cases, although certainly not in all, a bougie of very considerable size—say with a diameter of eight to ten millimetres—may be introduced through the urethra into the bladder, and often without injuring the parts. It has been more fully demonstrated than at any previous time, perhaps, that the healthy urethra is, in most instances, very distensible. And this fact seems first to have suggested the possibility of employing larger evacuating tubes and a more powerful aspirator than had hitherto been used. It occurred to Professor Bigelow, of Harvard University, that by adopting larger instruments he might remove a hard calculus of considerable size at a single sitting, and so in this respect rival lithotomy in its one admitted advantage of emptying the bladder completely at once. He accomplished this design more or less successfully in a few cases, and soon came to the conclusion that the bladder was left in a condition of greater safety when no fragment remained to irritate it, although the operation had been unusually prolonged and severe, than when, after a much less irritating employment of instruments, portions of the calculus were left still unremoved. The same conviction which led Dolbeau to perform long and severe manipulation through the perineal wound and distended neck of the bladder, in order to empty it at a single sitting, has led Bigelow to aim at achieving the same result by lithotrity instruments through a distended urethra.

The procedure of Bigelow consists in a bolder and more extended application of the two agencies—viz., crushing the stone and washing out the débris—which have long been adopted and recognized as constituting the operation of lithotrity. For this purpose he adopts a more powerful lithotrite, a larger evacuating catheter, and a stronger india-rubber bottle; and he uses them for a longer time than has been customary with previous operators. He insists that it is necessary to crush more and to evacuate more than heretofore; but the essence of the change proposed, and the value of his idea, lies in the alleged greater safety to the patient attained by emptying the bladder in all cases completely at a single sitting. To him is due the improvement, whatever it may be, which the realization of this idea achieves; and I shall be glad if to any extent, however humble, I may contri-

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bute towards the elucidation of this point, which is still to some extent perhaps a question *sub judice*.

Rather more than two years ago I adopted, after a few trials, the one-sitting method as a rule, not binding myself to its employment for every case, but reserving, according to my judgment of the varied circumstances which present themselves, the lateral operation of lithotomy for exceptional cases; and in one case only of lithotrity employing four sittings for a stone of very large size, where the patient was not deemed capable of enduring prolonged etherisation. The result is somewhat more favourable than any I have before attained, so far as the numbers thus treated enable me to judge.

My entire experience during this period, regarding the adult male only, amounts to 103 cases, their mean age being just 60 years: their histories form an Appendix, containing all details, and the names of the medical men who have seen the cases with me.

Of these 103 cases, I have operated on 91 by lithotrity at one sitting; 88 have recovered, and only 3 have died.

One case of lithotrity at 4 sittings, referred to, made a good recovery.

Ten cases were operated on by lateral lithotomy, of which 6 recovered and 4 died.

One case of median lithotomy, which recovered.

Total, 103 cases of lithotomy and lithotrity in adults; recoveries, 96; deaths, 7.

My assistant, Mr. G. Buckston Browne, has in my absence during the same period operated on 13 cases of lithotrity at one sitting, 12 of whom recovered and 1 died. All these cases are exhibited in the two trays in the cabinet before you, which contains nearly 600 other calculi, crushed and removed, chiefly by the aid of the original evacuator of Clover; besides more than 100 cases of calculi, which I have removed by lithotomy. All these are operations on the adult, and the mean age is upwards of 60 years.

The time at my disposal does not permit me to discuss many interesting points connected with the foregoing cases, and furnished in the Appendix. I will, therefore, only offer a few remarks, grounded on my experience, in the form of brief practical deductions therefrom.

And, first, I will observe that, in order to attain the object proposed by performing lithotrity at a single sitting, in a very large majority of cases no modification of pre-existing instruments is necessary. Of course each operator may, and often does, prefer some little difference in form or action, devised by himself, to those adopted by another. But, inasmuch as the great bulk of calculi met with are under 20 grammes (5 drachms) in weight, instruments larger than those hitherto used are, as a rule, wholly unnecessary. I can scarcely insist on a more important axiom regarding lithotrity than this—viz., that the lithotrite and evacuating catheter should in all cases be of the smallest size consistently with power to effect the removal of the calculus at one sitting. When the stone is larger than those hitherto usually removed by lithotrity, then, but only then, should the instruments be larger. For, although it is incontestable that larger instruments than those hitherto used may be applied in urinary passages of average size, by somewhat unnaturally distending them; yet it is no less certain that the employment of large instruments is attended by risks which are absent with small instruments—a fact which to my knowledge has been too often overlooked. I do not hesitate to assert that the important practical difference between the instrumeant employed by most experienced lithotritists up to three years ago, and those

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which have been recently introduced by Professor Bigelow, is this—that the former are limited in size by the natural calibre of the urethra, while the latter frequently require the urethra to be distended to a size beyond that which is natural. I have known large lithotrites and evacuators, such as I have never found necessary in any case, to be employed for the purpose of applying Bigelow's procedure to small calculi, with disastrous result; meaning by the term small calculi such as have hitherto been removed by small instruments with almost uniform success; the unnecessarily large instruments having occasioned injury to the urethra, followed by severe rigors and a fatal result. I state a simple but significant fact, familiar to experienced surgeons, that many a urethra will admit a bougie of No. 12 or 13 English (22–24 French) without any unpleasant consequences; while if No. 16 or 17 English (28–30 French) be passed, severe constitutional disturbance will certainly follow. Much discredit will speedily attach to lithotrity in this newest phase thereof, if we do not adhere to that most important rule of practice—viz., always to remove the stone by the smallest possible amount of mechanical action. And I assert that this can only be done by employing instruments which are conformable in size to that of the stone itself, and never adopting unduly heavy and powerful mechanism unless the exceptional size of the calculus demands it. The great value of lithotrity is discredited by indifference to a rule so obviously desirable.

And allow me to remark here, that it is no less a flagrant error to ignore so completely the resources of our art, and to disregard experience in the application of them, as to adopt any form of lithotomy when the stone is so small that it may be easily crushed in five minutes with a small lithotrite. Yet I have known lithotomy, not only lateral, but supra-pubic, gravely adopted here in such conditions, and fatal results gratuitously incurred—to me circumstances of the deepest regret.

And this leads me to observe, in the second place, after very grave consideration of the subject, that there is no surer means of attaining increased success from operative proceedings at the present time than by teaching the importance of diagnosis, so as to ensure a suitable application of means to ends in removing the stone. I am certain that no one principle in my own experience has, so much as that, conduced to the saving of life among calculous patients; and also to the avoiding of subsequent chronic disease among those who recover. When the importance of adopting the method and the instruments best fitted for each case of stone—a result which can only be effected by first ascertaining the size and nature of the body with which we have to deal—is fully appreciated, greater success will be attained than has ever previously been achieved, or than can be attained by adopting any single method of operation, however complete it may appear to be.

Thirdly, a great improvement in the results of calculus operations will be effected when the immense importance of ascertaining the presence of the stone as early as possible in the history of each case is understood. It cannot, in our teaching, be too strongly asserted that in almost all instances when there is a calculus in the bladder, unmistakable signs of its presence are manifest to the intelligent observer, and that its existence should be verified without delay—a result always possible at an early stage. If this were done, every calculus might be removed by lithotrity at a single sitting, without recourse to any extraordinary means. This doctrine I have taught unceasingly on all occasions for many years, having long been profoundly impressed with its importance; and this

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is still more apparent as the method of operating becomes more perfect and efficient.

To conclude: I venture to submit that the progress which has been made in the art of extracting the stone from the male bladder during the last few years may be summed up under the following heads:—

1. In operating by lithotrity, there appears to be conclusive evidence in favour of the practice of emptying the bladder of a stone at a single sitting when practicable, even although the manipulation and the instruments demand a greater expenditure of time and more disturbance than have been hitherto deemed admissible; provided only that the instruments employed shall never be larger than the nature of the stone demands.

2. In operating by lithotrity for certain rare cases, important results may be attained by combining with it an incision into the urethra from the perineum, for the purpose of withdrawing urine, or even some fragments of the stone.

3. In perineal lithotomy for a stone which is unusually hard and large, it is often advantageous to use modern approved methods of breaking up the stone before removing it, rather than to withdraw it entire, so that the operator may avoid unnecessarily extended incisions, or rupture of the neck of the bladder.

4. In the exercise of careful diagnosis; first, as to the existence and then as to the size and nature of the calculus in every case, with the view of selecting the operation best suited for it, and always adopting that which shall inflict the least injury, or even disturbance, in the organs involved.

APPENDIX.

Containing details of all the Cases referred to in the ninety-one Cases of Lithotrity at "One Sitting" in the Adult Male.

CASE 1.—A gentleman, aged 54. December 16, 1878. Oxalate and phosphate, weighing 97 grains; in 9 minutes. Sent to me by Mr. Spencer Wells, who was present at the operation. Occasionally forms phosphates, and washes out the bladder to remove them.

CASE 2.—A gentleman, aged 66. December 16, 1878. A uric acid calculus, weight 84 grains; removed at one sitting in 7 minutes. He had some fever and impaired health for several weeks, but ultimately recovered; had no more symptoms and is now in good health. Seen with Dr. Taylor, of Camberwell.

CASE 3.—A gentleman, aged 43. December 21, 1878. Oxalate of lime, small; in one sitting; 42 grains. No fever or other untoward symptoms after; quite well up to present time. Mr. Spencer Wells was present at the operation.

CASE 4.—A gentleman, aged 72. December 22, 1878. I had crushed a calculus five and a half years before, and he had been free from symptoms until about twelve months ago. I removed now, in one sitting, a phosphatic calculus, weight 154 grains, in 8 minutes; a slow but sound recovery. He draws off all his urine by catheter. Dr. Sutro was present.

CASE 5.—A gentleman, aged 69. January 6, 1879. Uric acid and phosphatic mixed, which had caused much suffering. Much relieved; he passes all his urine by catheter. Dr. A. K. Longhurst was present. A little removed at a second sitting a day or two after.

CASE 6.—A gentleman, aged 69. January 22, 1879. Uric acid, weighing 172 grains; in 11 minutes. Sent to me by Mr. Lathbury, of Finsbury. Successful. This patient had also diabetes.

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CASE 7.—A gentleman, aged 68. January 31, 1879. Uric acid, weighing 114 grains; in 9 minutes. A rapid recovery. Sent to me by Mr. J. H. Walters, of Farringdon.

CASE 8.—A gentleman, aged 79. February 12, 1879. Phosphatic, weighing 194 grains; in 12 minutes. I had cut him a year before, removing four uric acid calculi. He made a quick recovery, but passes, as before, all his urine by catheter, from enlarged prostate. Mr. Clover gave ether on both occasions.

CASE 9.—A gentleman, aged 65. February 13, 1879. Phosphatic, 116 grains, in 6 minutes. He passes most of his urine by catheter. Sent by Mr. Holberton, of Hampton.

CASE 10.—A gentleman, aged 69. March 15, 1879. Uric acid, 200 grains; in 10 minutes. A few fragments were removed at two explorations soon after. A good recovery. Seen with Dr. Bantock, who was present at the operation.

CASE 11.—A gentleman, aged 56. March 15, 1879. Uric acid. Dr. Bantock, Dr. Muir, and others present. A good recovery.

CASE 12.—A youth, aged 16. March 28, 1879. Oxalates and urates mixed; rather small. Mr. Cadge and Mr. R. Harrison present.

CASE 13.—A gentleman, aged 82. March 23, 1879. Phosphatic, about 60 grains. Dr. Smith, of Great Hadlam, present.

CASE 14.—A gentleman, aged 47. March 28, 1879. Pure triple phosphate, weighing 94 grains: in 7 minutes. Sent to me by Dr. Mitchell of New Cross, who was present at the operation; as were also Mr. Cadge and Mr. Reginald Harrison.

CASE 15.—A gentleman, aged 63. June 2, 1879. Phosphatic, weighing 68 grains; in 6 minutes. Mr. Bailey gave ether; Dr. Keyes, of New York, was present. He has to pass a catheter three times a day, not emptying his bladder naturally.

CASE 16.—A gentleman aged 66. June 2, 1879. Uric acid, weighing 175 grains; in 10 minutes. Recovery complete. Seen with Dr. G. O. Rees, who was present at the operation.

CASE 17.—A gentleman, aged 70. June 12, 1879. Uric acid, weighing 98 grains; in 10 minutes. Recovery complete. Sent to me by Dr. Ryott, of Newbury, who was present at the operation.

CASE 18.—A gentleman, aged 70. June 25, 1879. Mixed oxalates and urates, weighing 214 grains; in 12 minutes. Recovery complete. Seen with Dr. George Johnson, who was present at the operation.

CASE 19.—A gentleman, aged 60. June 29, 1879. Oxalate of lime, weighing 165 grains; in 9 minutes. Recovery complete. Seen with Mr. Morgan, of Sussex Place, and his son, who gave ether.

CASE 20.—A medical man, aged 41. July 16, 1879. Phosphatic, weighing 208 grains; in 15 minutes. I removed 10 grains subsequently. He had orchitis and fever, and made a slow recovery.

CASE 21.—A gentleman, aged 78. July 19, 1879. Uric acid, weighing 329 grains; in 25 minutes. At his age, Mr. Clover preferred not to give ether longer, and I postponed the proceeding three days, removing then 192 grains more in 13 minutes, making a total of 521 grains (or nearly 1 ounce and 1 drachm) of very hard uric acid. Recovery complete. Sent to me by Mr. Quain, of Cavendish Square, and is well now (1880).

CASE 22.—A gentleman, aged 58. July 31, 1879. Uric acid, weighing 145 grains; in 11 minutes. Recovery complete. Seen with Dr. George Johnson, who was present at the operation.

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CASE 23.—A French gentleman, aged 67. July 31, 1879. Uric acid and some phosphate, weighing 40 grains. Dr. George Johnson and others present. A rather tedious recovery, never emptying his bladder except by catheter.

CASE 24.—A gentleman, aged 56. August 7, 1879. Uric acid, weighing 106 grains; in 10 minutes. Mr. M. B. Hill and Mr. John Morgan present. Some irritation of the bladder continued, and was troublesome for a few weeks, and then subsided.

CASE 25.—A gentleman, aged 56. August 13, 1879. Uric acid. Has some narrowing of the urethra at three and a half inches from the orifice, so that a smaller evacuating catheter than usual had to be attached to the aspirator. Weighed 123 grains; in 12 minutes. Sent to me by Dr. Kidd; Mr. John Morgan and Dr. Lebec, of Paris, present. A good recovery.

CASE 26.—A gentleman, aged 54. August 19, 1879. Uric acid, weighing 45 grains; in 6 minutes. Mr. John Morgan present. A good recovery.

CASE 27.—A gentleman, aged 70. August 16, 1879. Phosphatic, weighing 42 grains; in 5 minutes. A good recovery.

CASE 28.—A gentleman, aged 65. December 5, 1879. Uric acid and oxalate, weighing 40 grains; in 5 minutes. His attendant, Dr. Marsden, of Grosvenor Street, was present. A good deal of irritation followed: and although no phosphatic deposits or cystitis appeared, he passes water frequently, although without pain or blood. Otherwise he is quite well.

CASE 29.—A gentleman, aged 55. December 3, 1879. Uric acid. I removed 158 grains at a sitting of 10 minutes, leaving a little more in the bladder, which I removed (29 grains) a day or two after. Mr. William Rose, of King's College Hospital, was present. An excellent recovery.

CASE 30.—A gentleman, aged 62. January 1, 1880. Uric acid, weighing 98 grains; in 8 minutes. Sent to me by Dr. Moore, of Lancaster. A good recovery.

CASE 31.—A gentleman, aged 57. January 22, 1880. Uric acid, weighing 65 grains; in 5 minutes. Sent to me by Mr. J. E. Williams, of Whitstable. A somewhat tedious recovery, but he gradually attained an improved condition.

CASE 32.—A gentleman, aged 70. February 28, 1880. Uric acid, weighing 308 grains, in 8 minutes; finding 15 grains more three days after. Mr. Henry Smith, of King's College Hospital, saw this case with me. A rapid and complete recovery.

CASE 33.—A gentleman, aged 66. March 2, 1880. Uric acid, weighing 78 grains; in 6 minutes. Dr. George Johnson was present at the operation. A rapid and complete recovery.

CASE 34.—A gentleman, aged 65. March 20, 1880. Uric acid, weighing 51 grains: in 4 minutes. Sent to me by Dr. Handfield Jones. He had orchitis, and made a rather slow but complete recovery.

CASE 35.—A gentleman aged 73. April 26. Uric acid, weighing 96 grains; in 9 minutes. Mr. Erichsen and Mr. Furner, of Brighton, were present. A good recovery.

CASE 36.—A gentleman, aged 71. May 24, 1880. Uric acid with phosphates, 76 grains; in 7 minutes. Brought by Dr. Charles Mott, of Walton, who was present at the operation. He had a large prostate; did not empty the bladder, and was taught to use a catheter twice a day. An excellent recovery.

CASE 37.—A gentleman, aged 61. May 19, 1880. Uric acid, weighing 78 grains. Mr. Furner, of Brighton, present. A somewhat slow but good recovery.

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CASE 38.—A gentleman, aged 70. May 29, 1880. Phosphatic, weighing 184 grains. He was seen by Mr. Lund, of Manchester. He came to me greatly broken in health, with severe symptoms, which I at first regarded as chiefly renal, and did not suspect to be complicated with calculus. Finding one subsequently, I thought it right to remove it at one sitting, and this much relieved him ; but he slowly sank about five weeks afterwards with advanced pyelitis, dilated ureter, and purulent deposits in the kidney.

CASE 39.—A gentleman, aged 67, from Hamburg. June 3, 1880. Uric acid and oxalate, weighing 142 grains. Mr. Lund, of Manchester, was present. He made a good recovery. He had some enlargement of the prostate; did not quite empty his bladder, and learned to do so with a catheter every night.

CASE 40.—A gentleman, aged 50. June 21, 1880. Chiefly phosphatic, with little uric acid, weighing 105 grains. Dr. Harker, of Lancaster, and Dr. Carpenter, of New York, were present. He made a good recovery.

CASE 41.—A gentleman, aged 70. June 21, 1880. Weak physically and mentally, with a very large prostate, so that the calculus could only be seized with reversed blades. Uric acid, weighing 80 grains. Brought to me by Dr. Harker, of Lancaster. There was at first no ground for anxiety; but in a few days he had fever and an attack of bronchitis, of which he died on July 5.

CASE 42.—A gentleman, aged 68. June 26, 1880. Uric acid calculus, weighing 76 grains. Sent to me by Dr. James McCulloch, of Dumfries. Made a rather slow recovery, from persisting chronic cystitis.

CASE 43.—A gentleman, aged 72. July 3, 1880. A phosphatic calculus, weighing 78 grains. Dr. Bainbridge, of London, who brought him to me, and Dr. Weir, of New York, were present. He had long passed all his urine by catheter, from enlarged prostate, and of late with distressing frequency. He is now (four weeks afterwards) enjoying freedom from all pain. Clear urine, and retains it four hours.

CASE 44.—A gentleman, aged 77. July 10, 1880. Uric acid, weighing 265 grains; in 18 minutes. Professor Humphry, of Cambridge, was present. He made an excellent recovery. He called on me, July 8, 1881, absolutely free from symptoms, and in perfect health.

CASE 45.—A gentleman, aged 46. July 10, 1880. Uric acid, weighing 96 grains. Sent to me by Dr. Wynn Thomas, of Birmingham. Professor Humphry, of Cambridge, was present. He has had a good deal of irritation, but it is gradually subsiding, and has, indeed, almost disappeared.

CASE 46.—A gentleman, aged 79, retired medical man. July 14, 1880. Has passed no urine, except by catheter, for three years, from enlarged prostate. Phosphatic, weighing 128 grains. Mr. Eddowes, of Shrewsbury was present. A rapid recovery.

CASE 47.—A gentleman, aged 29. August 5, 1880. Small, oxalate of lime. Brought to me by Mr. Furber, of Kensington, who, with Dr. Joseph Warren, of Boston, and Mr. F. S. Edwards, of Bartholomew's School, was present. He made a rapid recovery.

CASE 48.—A gentleman, aged 55. August, 9, 1880. Uric acid. Brought to me by Mr. Harris, of Mildenhall, who, with Dr. J. Warren, was present at the operation. Good recovery.

CASE 49.—A gentleman, aged 60. August 16, 1880. Uric acid, small. Brought to me by Dr. Way, of Eaton Square, who was present at the operation. A good recovery.

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CASE 50.—A gentleman, aged 48. August 17, 1880. A small uric acid calculus. Mr. E. Milner, of the Lock Hospital, present. An excellent recovery.

CASE 51.—A gentleman, aged 77. August 14, 1880. Uric acid, weighing 97 grains. Brought to me by Mr. Tegart, of Jermyn Street, who was present at the operation. An excellent recovery.

CASE 52.—A gentleman, aged 73. August 17, 1880. Uric acid, weight 318 grains; Removed in 15 minutes, His medical attendants, Dr. Clothier, of Highgate, and Mr. Milner present. An excellent recovery.

CASE 53.—A gentleman, aged 52. August 23, 1880. Uric acid, small. Mr J. Morgan, and Dr. Warren, of Boston, present. Good recovery.

CASE 54.—A gentleman, aged 54. August 17, 1880. Mixed urates and phosphates, weighing 136 grains. Brought to me by Dr. Abud, who was present. Made a good recovery.

CASE 55.—A gentleman, aged 64. August 27, 1880. Uric acid, weighing 130 grains. Sent by Mr. J. C. Lindop, of Newport, Salop. An excellent recovery.

CASE 56.—A gentleman, aged 61. August 30, 1880. Mixed urate, oxalate, and phosphate, weighing 175 grains. Dr. Clothier, of Highgate, present. Good recovery.

CASE 57.—A gentleman, aged 62. September 13, 1880. Uric acid. Dr. Macloughlin present. Excellent recovery.

CASE 58.—A gentleman, aged 54. September, 28, 1880. Uric acid. Mr. Swinford Edwards present. An excellent recovery.

CASE 59.—A gentleman, aged 70. August 14, 1880. Phosphatic. Mr. John Morgan present. Recovery rapid and complete.

CASE 60.—A gentleman, aged 63. September 21, 1880. Phosphatic calculus, small, lying behind a large prostate. For eight years he had passed all his urine by catheter. He made a good recovery.

CASE 61.—A gentleman, aged 63. September 27, 1880. Multiple uric acid; weight 108 grains. Brought to me by Dr. Macnaughten Jones, of Cork, who was present at the operation. Good recovery.

CASE 62.—A gentleman, aged 60. October 15, 1880. Uric acid. His medical attendant, Dr. Birt, of Stourbridge, present. Very obstinate cystitis, associated with phosphatic deposit, appeared soon after; a very easy and short operation by one sitting. This continued some months, only gradually diminishing. He was seen with Dr. Andrew Clark.

CASE 63.—A gentleman, aged 48. November, 6, 1880. Uric acid, small. Very obstinate cystitis followed, just as in the preceding case, without known cause; but made a perfect recovery in 2 or 3 months. Sent to me by Dr. James Sawyer, of Birmingham.

CASE 64.—A gentleman, a surgeon, aged 71. October 29, 1880. Uric acid Mr. Charles Moss gave ether. Returned to his practice in a few days, never having had a bad symptom.

CASE 65.—A gentleman, aged 36. November, 6, 1880. Small uric acid. Mr. Ceely, of Aylesbury, present. Recovery rapid and complete.

CASE 66.—A gentleman, aged 70. November 12, 1880. Large hard phosphatic calculus; weighed 360 grains. Seen with Mr. Curgenven, of Bayswater. No bad symptoms. Three days afterwards, removed a few grains of calculous material. This was followed by pain in right ureter and kidney, rigors, and suppression for about twenty-four hours. He rallied, but gradually sank on the 6th day after the second sitting.

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CASE 67.—A gentleman, aged 62. November 10, 1880. Mixed oxalate and phosphate, weighing 60 grains. Sent to me by Dr. Kidd. An excellent, rapid recovery.

CASE 68.—A gentleman, aged 73. February 3, 1881. Uric acid; weight 104 grains. I operated at Ghent, Belgium, with Dr. Bodaert. Some months later, Dr. Bodaert wrote to me that the patient was driving daily, and that the operation was *un succès magnifique.*

CASE 69.—A gentleman, aged 60. February 8, 1881. Phosphatic calculus, weighing 96 grains. Dr. Chepmell and Dr. Barton Smith present. The patient had long passed all his urine by catheter, owing to atony of the bladder. He was now suffering very severely, losing much blood, and greatly reduced: but he rapidly regained his health, lost all symptoms of stone, and is now well and active.

CASE 70.—A gentleman, aged 48. February 25, 1881. Phosphatic calculus, weighing 280 grains. Sent me by Dr. Andrew Clark. Dr. Philpot, of Dulwich, was present at the operation. The patient had a narrow stricture, for which he first came to me; this was dilated and the stone found. Rapid recovery.

CASE 71.—A gentleman, aged 75. March 16, 1881. Uric acid, weighing 46 grains. Sent by Dr. Kidd. Mr. Franklin, of Putney, present at operation. Recovery complete.

CASE 72.—A gentleman, aged 67. March 18, 1881. Uric acid, weighing 106 grains. Sent by Dr. Hayle, of Rochdale. This patient was the subject of diabetes. After the operation there was fever and much cystitis. The urine remained bloody from 4 to 5 weeks. After this he lost power to empty the bladder, and learned to use a catheter. Urine deposited phosphates. Chronic cystitis remained for some time, but gradually subsided, and he now drives about without pain, and his health has greatly improved; but he is still diabetic.

CASE 73.—A gentleman, aged 55. March 18, 1881. Uric acid, weighing 175 grains. Mr. Kent, of the Middlesex Hospital present. Some cystitis followed the operation, which was troublesome for a few weeks. Ultimately he made a complete recovery.

CASE 74.—A gentleman, aged 71. April 14, 1881. Phosphatic, weighing 80 grains. Seen with Mr. Thomas Bond, of Westminster. Successful.

CASE 75.—A gentleman, aged 64. March 28, 1881. Uric acid, weighing 54 grains. Dr. A. Eccles, of Bayswater, present. Recovery was rapid and complete, and he shortly resumed his practice as a surgeon in Plymouth.

CASE 76.—A gentleman, aged 65. April 30, 1881. Mixed urates and oxalates, weighing 104 grains. Sent by Dr. Best, of Birmingham, who was present at the operation. Made a good recovery. He had enlarged prostate, and passes a catheter four times daily.

CASE 77.—A gentleman, aged 70. May 25, 1881. Uric acid, weighing 105 grains. Sent me by Mr. Patten, of Ealing, who was present at the operation. An excellent recovery.

CASE 78.—A gentleman, aged 68. May 25, 1881. Uric acid, weighing 55 grains. Mr. Patten, of Ealing, present. Good recovery.

CASE 79.—A gentleman, aged 65, June 7, 1881. Uric acid, weighing 124 grains. Sent to me by Mr. Bym, of Westbourne Place, Eaton Square, who was present at the operation. He is recovering slowly.

CASE 80.—A gentleman, aged 66. June 3, 1881. Mixed urates and phosphates, weighing 125 grains. Seen with Mr. Blackstone, of Regent's Park. Recovery excellent and rapid.

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CASE 81.—A gentleman, aged 71. June 11, 1881. Phosphatic, weighing 112 grains. Mr. Charles Moss gave ether. Recovery rapid and complete.

CASE 82.—A gentleman, aged 67. June 11, 1881. Uric acid, weighing 62 grains. Sent by Dr. Orr, of Fleetwood, Lancashire; Dr. Curtis, of Boston, present. Recovery rapid.

CASE 83.—A gentleman, aged 30. June 11, 1881. Phosphatic, weighing 180 grains. He came to me from South Africa. Dr. Curtis, of Boston, present. Recovery rapid and complete.

CASE 84.—A gentleman, aged 51. June 14, 1881. Uric acid weighing, 80 grains. Dr. Kidd sent him to me. Dr. Imschoot, of Ghent, present. Good recovery.

CASE 85.—A gentleman, aged 68. June 27, 1881, Oxalate of lime, weighing 30 grains. Sent to me by Mr. Evershed, of Arundel. Dr. Imschoot, of Ghent, present. A good recovery.

CASE 86.—A gentleman, aged 59. June 27, 1881. Uric acid, 112 grains. Sent to me by Dr. Clay, of Plymouth. Dr. Belfield, of Chicago, present. Some fever followed the operation, but he made a good recovery.

CASE 87.—A gentleman, aged 72. June 13, 1881. Phosphatic, weighing 46 grains. Mr. Charles Moss gave ether. Good recovery.

CASE 88.—A gentleman, aged 24. July 7, 1881. Phosphatic calculus, weighing 246 grains. Seen in consultation with Dr. W. M. Ord, who was present at the operation.

CASE 89.—A gentleman, aged 74. July 12, 1881. Phosphatic calculus, weighing 95 grains. Seen with Dr. E. R. Butler, of Kensington, who was present at the operation.

CASE 90.—A gentleman, aged 60. July 20, 1881. One sitting, removing 320 grains of uric acid. Seen with Mr. F. E. Webb, of Maida Vale.

CASE 91.—A gentleman, aged 55. July 23, 1881. One sitting, removing 95 grains of uric acid. Sent to me by Dr. P. Mimms, of Thetford, Norfolk.

Eleven Cases in which Lithotomy was performed during the recent period, in which the "One Sitting" Lithotrity was adopted as the rule.

CASE 1.—A gentleman, aged 74. May 9, 1879. With Mr. Goolden at Maidenhead. Death.

CASE 2.—An Irish gentleman, aged 62. February 11, 1880. With Dr. G. Johnson, of London. Successful.

CASE 3.—A gentleman from New Zealand, aged 33. May 21, 1880. Seen with Mr. Christopher Heath and others. Successful.

CASE 4.—An Irish gentleman, aged 52. June 1, 1880. Sent to me by Dr. Thompson, of Belfast. Successful.

CASE 5.—A gentleman, aged 28. June 10, 1880. Sent to me by Mr. Curgenven, of Derby. Successful.

CASE 6.—A gentleman, aged 62. July 13, 1880. Brought to me by Dr. Bright, of Forest Hill. Successful.

CASE 7.—A gentleman, aged 69. August 18, 1880. Uric acid, large. Seen with Dr. Hullet Browne and Dr. Davies. Owing to the enormous size of the prostate, lithotrity was not deemed advisable. Death.

CASE 8.—A gentleman, aged 68. September 28, 1880. Urates and oxalates. Mr. Swinford Edwards present. The stone was large, and so irregular in outline that it could not be extracted, and had to be broken up with screw forceps in the

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bladder. All went well for ten days, when secondary hæmorrhage occurred. For a time he seemed likely to recover, but he gradually sank, and died 35 days after operation.

CASE 9.—A gentleman, aged 54. October 23, 1880. Urates and phosphates, large; health very imperfect. Seen with Dr. Turnour, of Denbigh. Died 31 days after operation, of diseased kidney.

CASE 10.—A gentleman, aged 29. November 6, 1880. A tumour and phosphatic calculus. Median operation; excellent recovery. He had been crushed a year before, but stone soon returned. On sounding I thought I could detect an impacted calculus; determined on cutting him, and asked some friends to be present, among them Mr. Ceely, of Aylesbury, Dr. Seegen, of Vienna. It proved to be a pedunculated tumour coated with phosphatic matter. I twisted it out entire by means of small lithotomy forceps. He made a rapid recovery and has been well ever since. The tumour is in my possession; it is larger than a walnut.

CASE 11.—A gentleman, aged 67. February 26, 1881. Oxalate of lime. Sent to me by Dr. Kidd. Went home quite well in 28 days.

Thirteen Cases of Lithotrity at One Sitting operated on during the same period by my assistant, Mr. G. Buckston Browne.

CASE 1.—A gentleman, aged 68. September 19, 1878. Phosphatic calculus, weight 140 grains. Mr. Clover gave ether. Went home well in 10 days.

CASE 2.—A gentleman, aged 63. October 1, 1878. Phosphatic calculus. weight 120 grains. Mr. Clover gave ether. Beside the stone there was malignant disease of the bladder, and although relieved by the operation, he died in January following.

CASE 3.—A gentleman, aged 81. October 23, 1878. Phosphatic calculus, weight 130 grains. Seen with Dr. Smith, of Little Hadam, Herts. Made a capital recovery, and went home well on November 6.

CASE 4.—A gentleman, aged 65. September 12, 1879. Phosphatic, weight 40 grains. Soon went home well.

CASE 5.—A gentleman, aged 69. September 20, 1879. Multiple uric acid calculi, weight 120 grains. Seen with Dr. Horace Dobell. Made a good recovery.

CASE 6.—A gentleman, aged 63. October 8, 1879. Phosphatic calculus, weight 160 grains. Seen with Dr. Palfrey. Went home well in 12 days.

CASE 7.—A gentleman, aged 63. October 10, 1879. Mixed uric and phosphatic calculus, weight 154 grains. Seen with Dr. R. T. Smith, of Haverstock Hill. Success perfect.

CASE 8.—A gentleman, aged 72. December 17, 1880. Phosphatic calculus, 90 grains. Mr. Charles Moss gave ether. Excellent recovery.

CASE 9.—A gentleman. aged 76. December 30, 1880. Phosphatic calculus, 120 grains. Mr. Charles Moss gave ether. The patient had for seven years drawn all his urine by catheter. He was much relieved by the operation, and went home well.

CASE 10.—A gentleman, aged 62. January 8, 1881. Uric acid calculus, weight 40 grains. Mr. Charles Moss gave ether. Success perfect.

CASE 11.—A gentleman, aged 55. February 5, 1881. Uric acid calculus very hard, weight 312 grains. Mr. Moss gave ether. All went well for two days, when he became feverish, and he died in a week with symptoms of uræmic poison. Throughout the urine was clear, and free from blood and mucus. He had for

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nine years passed quantities of uric acid calculi, and had several times been to Vichy.

CASE 12.—A gentleman, aged 74. May 24, 1881. Large uric acid calculus, weight 360 grains. Dr. Arcedeckne Duncan, of London, brought him to me, and assisted me at the operation. Complete success.

CASE 13.—A gentleman, aged 70. June 25, 1881. Phosphatic calculus, very large, weight 601 grains. Mr. Charles Moss gave ether. Owing to the great size of the stone the case was a difficult one, and the operation lasted 40 minutes. Excellent recovery.

Modern Lithotrity.

HENRY J. BIGELOW, Boston, Mass.

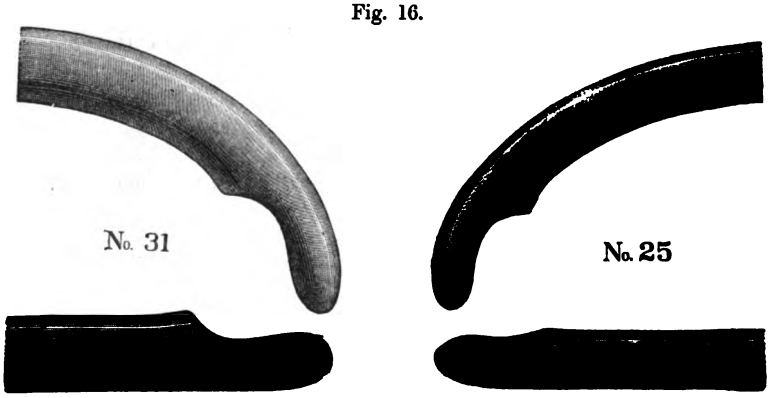
My object in the present communication is to show in what the modern operation of lithotrity consists, and to explain the instruments which have made its performance possible; for the removal of a vesical calculus through the urethra is now mainly a question of apparatus of which certain essential details are new. And as it is by no means necessary that the different parts of the apparatus should be put together exactly in any particular manner, it will be perhaps better to illustrate the principles of its construction than to insist on any special form of it. It will also be unnecessary for me to dwell on those parts of the subject which belong as well to the old lithotrity as to the new.

Lithotrity before 1878.

From the days of Civiale to the year 1878 there was little change in the operation. The duration of a sitting was as brief as the skill of the surgeon, stimulated by his fear of producing cystitis, could make it. Three minutes or less was the limit inculcated by standard books and the teaching of specialists, and the use of anæsthesia was exceptional. At present anæsthetics are the rule. The instruments have been already modified in an important manner, while sittings often last half an hour, and have been successfully extended to three hours and more.

Had Clover (whose catheter had a calibre only 21 of the French standard—about 12 English) or Mercier employed larger catheters (between 25 and 31 French —15 to 20 English), they might have evacuated the bladder completely. They

Fig. 16.



Large and small evacuating catheters, straight and curved (Nos. 25 and 31, French).

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would have found how little affected it was by a long operation, if no fragments were left behind, and that polished instruments were not injurious to it, while sharp fragments were. They would have discovered a tolerance on the part of the bladder wholly at variance with the traditions of half a century. Upon this tolerance modern lithotrity is based.

Lithotrity since 1878. In what its new feature consists.

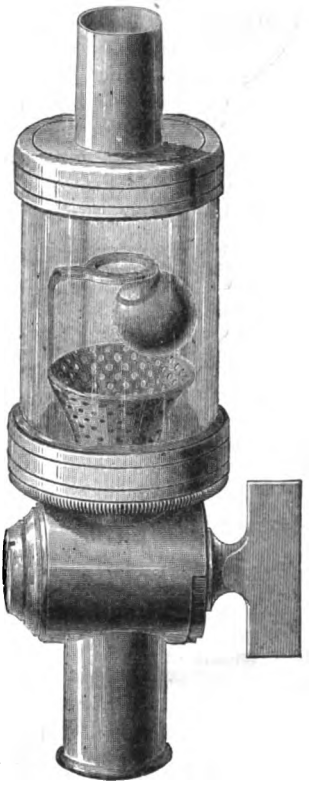
The new and essential instrument of the operation is the large catheter (25 to 31), whether straight or curved (Fig. 16). This is indispensable. It has an orifice at the extreme end, one side of which is prolonged so as to make its introduction easy. It has been adopted with little or no change, so far as I know, everywhere. Although my first apparatus was provided with efficient means of suction and a detached trap, neither of which was used before, its distinctive feature was the large catheter. The small size of the previous evacuating catheter delayed surgical progress for half a century. All the fragments could not pass through it, and it was impossible for surgeons to ascertain how the bladder would behave when once completely emptied of all fragments.

The new and essential part of the evacuating apparatus.

Assuming all this to be admitted, let us examine the rest of the apparatus, and see how the operation is modified by it. As even a single minute fragment left in the bladder may be the nucleus of a future stone, it is important to get rid of it with certainty and once for all. Now, although a fragment remaining in the catheter, after the bulb has ceased to expand, goes back to the bladder, yet if it has once passed beyond the catheter, its return to the bladder ought to be made impossible—the surgeon should be able to secure it. Yet no evacuator hitherto devised, whether with a long or a short connection between the bulb and the catheter, accomplishes this important end. They all inject fragments from the apparatus back into the bladder. I find by experiment that a few added inches of elastic tube make little difference. It is from the bulb or bottle that fragments are chiefly returned, before they settle into the receiver, and not from the tubes.

How to prevent the return of a fragment once evacuated.

Fig. 17.



Catheter-valve or strainer. A rubber ball, acting as a valve, has a seat of perforated metal, which strains the water. It prevents the fragments which pass it from returning.

To prevent this I have fixed a simple contrivance to the head of the catheter, which is absolutely effectual in securing every fragment as it comes through it (Fig. 17). It consists of a small light glass cylinder, containing a ball-valve of rubber acting noiselessly, the valve seat of which is perforated so as to strain the return current, and to keep back any fragment that has once passed it. Such a catheter-valve or strainer, to which I alluded

The new catheter-valve and strainer.

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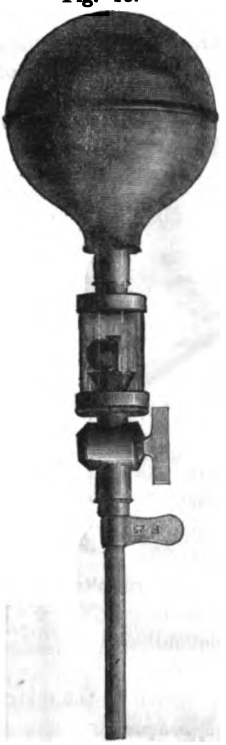
in my first paper (1878), is, I think, nearly as important as the large evacuating catheter itself, of which it may indeed be considered as a part.

The rest of the apparatus admits of endless variations, which are less important.

The evacuating sound.

If to the essential large catheter and trap we merely attach a strong elastic bulb, we have an excellent and simple instrument for a short washing, or for sounding for a last fragment. I have called this an evacuating sound (Fig. 18). No fragment drawn by it from the bladder can get back. This apparatus resembles Clover's, which was the first of the straight variety. It differs from it in having a large catheter, a valve or strainer, and a stiff bulb, all of which are either necessary or important for success.

Fig. 18.



The evacuating sound. It consists of a small evacuating catheter, a catheter valve, and a rubber bulb.

The hose.

One of the early additions to my own first instrument with which, in its original form, Thomas Smith successfully evacuated nearly two thousand grains of débris, was a hose. This contributes greatly to the convenience of the operation. When, for example, at each aspiration of the bulb, the wall of the bladder is drawn with a painful jerk, or series of jerks, into the orifice of the catheter evacuation is obstructed. More water is then needed to distend the walls. This is conveniently supplied by a small hose attached to the top of the bulb (Figs. 19, 21, 22). Should the patient strain or vomit, and the bladder be forcibly distended in consequence, we have here a means whereby the superfluous water can be quickly removed from it. When air rises to the top of the apparatus from the empty catheter or a leaky joint, it can escape by this hose, and be replaced by water. Valves in the hose itself, or in the vessel into which it delivers, allow the bloody water to be changed for fresh. By its means the amount of water can be regulated, and the operation becomes a dry one, while the apparatus is as simple as a common enema syringe.

A straight evacuator with a hose, and a receiver next the catheter.

I have made a convenient straight evacuator out of the evacuating sound by attaching to it a light receiver, like that of a breast-pump, close to the catheter, adding also a flat strainer* (Fig. 25) at the entrance of the bulb, to keep the fragments out of it, and a hose to manage the air and water (Fig. 19).

The same arrangement is here also shown (Fig. 20) in metal instead of glass.

A straight evacuator with a receiver below the bulb.

In Fig. 21 the glass receiver has been transferred from the neighbourhood of the catheter to the position it occupied in my first evacuator, below the bulb, to which the fragments are now, of course, admitted, by removing the flat strainer.

All these are varieties of the straight apparatus. This is much

* The strainer, new in the evacuator, is susceptible of a variety of applications and of form. A long narrow slit in the tube answers as well as the perforations.

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the best, in the distribution of its parts, of the evacuators wholly supported by the hand. The centre of the bulb is in line with the catheter, and were it not so heavy when full of water, it would be a most convenient handle for the management of that instrument.*

Fig. 19.

A straight evacuator, made from the evacuating sound by adding, below the valve, a glass receiver to hold the fragments, and, above the bulb, a tap for the hose. Between the valve and the bulb, inside, is a flat strainer (not seen) to keep the fragments out of the bulb.

In short, if the operator prefers to carry the bulb, unsupported, in his hand, straight instruments, such as have been described, are the best.

A complete evacuator.

But I prefer a support or stand of some sort. If a flexible tube or joint is interposed between the bulb and the catheter, the former can be supported upon the patient, or on a stand at a height corresponding with the inclination which the prostate gland gives the catheter, while the latter has a free and independent motion. Such is the instrument I use for any considerable evacuation (Fig. 22).†

It is no great matter where the catheter enters the wall of the bulb, but it

* By a straight instrument is here meant one in which the catheter and bulb are fixed in line rigidly, so that one cannot move without the other. If the bulb, still retaining a metal connection, were bent down out of this line, it could be supported on the patient or the table. But when it is bent up and attached inflexibly, it seems to me more difficult to manage. It is then neither so convenient for the operator to support its weight, nor is it easy to keep the catheter from oscillating. The rigid short connection seems to have been adopted in the hope, by shortening their route, of preventing a return of fragments. It fails to do this, because most of the fragments come not from the tubes, but from the bulb.

† The mobility of the bulb in a stand can be provided for in a variety of ways—by making a ball-and-socket joint of t e spherical bulb and a ring (Fig. 22), or of its glass receptacle (*Transactions of the Clinical S ciety*, London, 1879; and *Boston Medical and Surgical Journal*, January 1, 1880). The bulb can also be hung from a crane.

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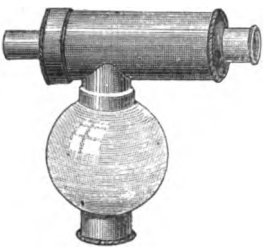
should finally deliver inside at some point lower than the top, so that any air can rise out of the way. Mr. Berkeley Hill was quite right also in saying that the catheter should not deliver into a narrow part of the apparatus where fragments are crowded together, because then they go back to the bladder. It is the worst place, although if a valve or strainer is used the defect is less important.

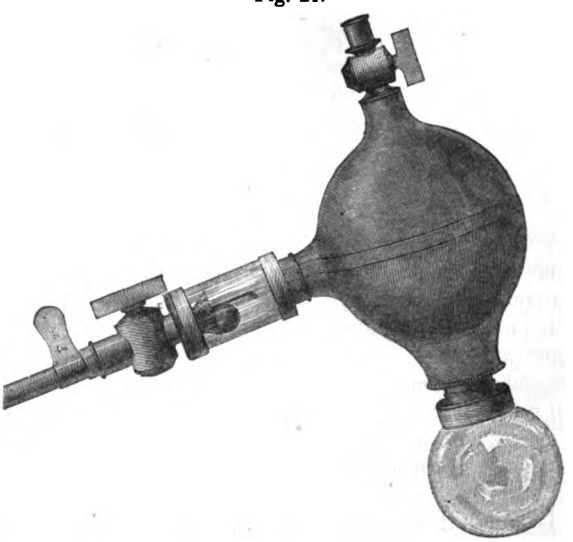
Fig. 20.



The valve and trap of Fig. 19, made of metal.

But, let me repeat it, a proper catheter to deliver the fragments, with an effectual contrivance to prevent their return, are the essential parts of the new apparatus; the rest is a matter of convenience or perfection, and every operator can choose the arrangement which suits him best. When the instrument is imperfect, the fragments will go backwards and forwards; but they always leave a certain number outside. The evacuation will be accomplished in the end by every evacuator that has the large catheter, which is the key to modern lithotrity.

Fig. 21.



The same as Fig. 19, except that the receiver is here transferred to the bulb, and the flat strainer has been removed to admit the fragments to it.

What has been here said about the evacuator, an instrument which has been of late years so modified as to make it a chief factor in the operation of lithotrity, is intended rather to illustrate the principles which should guide the surgeon in its construction, than to recommend any particular one. For, although the instruments I showed at the Congress, and which I have described here and use, accomplish the work of drawing out the fragments and preventing their return more perfectly than any previous apparatus for the purpose, this is not the only consideration which always in practice determines the surgeon in the choice of such an instrument.

I now recommend an evacuator on the ground of simplicity in construction

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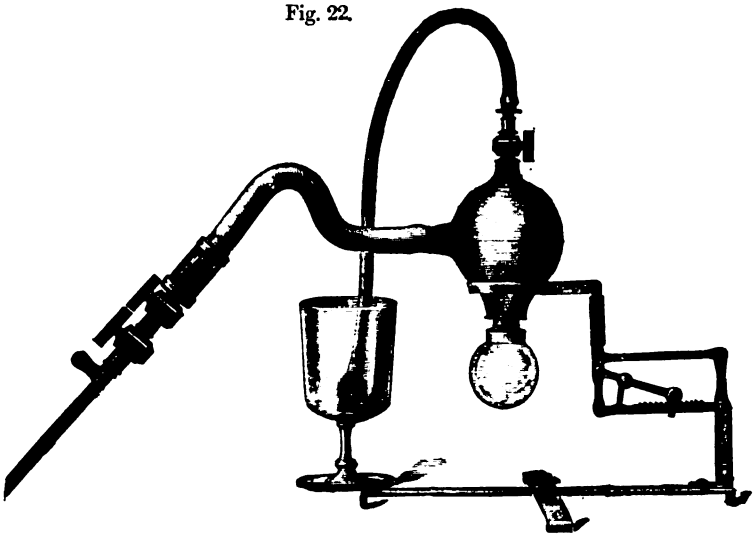
and economy in price.* It works quite well enough, with or without a stand. It is, in fact, the instrument already described, with a strainer substituted for the valve. Several instruments shown by me were provided with strainers, but this one is arranged with reference to the fact that the return of fragments from the apparatus to the bladder is much greater

A simplified evacuator.

Fig. 22.



A complete evacuator, used by the writer, being the same as that represented in Fig. 20, with the addition of a stand and an elastic tube. Curves in the elastic tube make it less liable to flatten when bent. The hose is attached when it is needed.

from the bulb than from the tubes to which it has been erroneously attributed. The strainer is here a perforated cylinder added to the catheter, passing inside the bulb, and extending nearly across it. Through this the fragments are drawn into the bulb-cavity. They swirl there for a few seconds, but when the water goes back to the bladder, it strains them off, and they fall into a receiver. The lengthened tube adds little to the amount of the returned débris, and, if obstructed, is easily cleared by a rod (Figs. 23, 26).

Tubular strainer.

The apparatus here described has, with or without stand, the following advantages :—

1. As a bulb is spherical, whether a receiver is attached to it or not, the hand of the operator, grasping its centre, is in line with the catheter, as if holding the handle of a long screw-driver, which is better than if the hand were above it, out of line. This is an advantage common to all straight evacuators.

2. The instrument is a short one. A long inflexible apparatus is liable to irregular movements which produce pain. Otherwise, the tubular strainer might be placed outside the bulb (in the glass trap, for example, described by Mr. Berkeley Hill—Figs. 24, 25). It is here fixed inside the bulb (Figs. 23, 24, 26), which so shortens the apparatus, that we have room for two connecting cocks, one for the bulb and one for the catheter. This makes the operation much drier than where there is no cock for the catheter.

* The aspirators here described and also the various lithotrites are made in this country by J. Weiss & Son, London.

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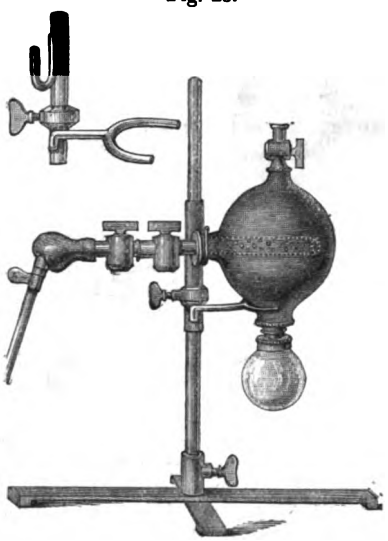
3. There is a hose for air and water above the bulb, and a receiver for fragments below it.

Without a stand, this instrument belongs to the straight evacuators with a rigid connection between the catheter and bulb. The operator then, of course, supports the weight of the bulb, which I find troublesome if the operation is more serious than that of washing out the bladder or securing a last fragment. We can easily have a stand here, steady enough to sustain the weight of the arm of the operator, and prevent the oscillation of the catheter. Three inches of elastic tube, or an elastic ball about an inch in diameter, can be interposed between the bulb and catheter to act as a universal joint.* It is then easy to rest the bulb in one hand on the patient's thigh, while the catheter is manipulated with the other; or upon a stand. The one here figured is of simple construction, and can be adjusted at any convenient height with reference to the catheter (see Fig. 23).†

Stand.

Universal joint.

Fig. 23.



The "Simplified Evacuator" and Stand. The catheter is prolonged, by a long tubular strainer, into the bulb. This makes a catheter-valve, though still advantageous, less necessary. An elastic ball or universal joint, with a small tubular strainer, is also substituted at the head of the catheter, for the elastic tube, and makes the instrument shorter. The stand is here a retort stand. The bulb hangs firmly in a fork (separately figured), and can be variously inclined.

I have condensed the arrangement here described from the materials referred to in the preceding part of this paper, having been lately persuaded that a compact and inexpensive apparatus, performing as perfectly as this one, is still a desideratum. To appropriately designate it, I have called it "The Simplified Evacuator." A stand and elastic joint can be had with it.

Within three years renewed attention has been given to the lithotrite, which was supposed to be beyond improvement. A calculus may indeed be crushed by any lithotrite, if strong enough. But, since decisive crushing has become more desirable, and the limit to the size of the stone has been extended, different lithotrites are more and more used for different stones. The old lithotrite, the combined work of Retoré, Costello, Charrière, and Weiss, underwent little change in half a century. A most convenient addition to it is the long handle described by Thompson.‡

Lithotrite.

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* A small metal drum with elastic ends makes a good joint. The metal sides cannot fall against the catheter and obstruct it.

† If the patient lies on a soft bed, the bulb must be held in the hand, unless the stand be steadied, like a fracture box, by something under it. But I usually do this surgical operation like others, on a table of some sort.

‡ "Practical Lithotomy and Lithotrity," p. 157. London, 1863.

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It gives a better hold for the left hand. But the inconvenient button in the lock of Weiss, which it encloses, has never been changed. The thumb and the eye of the operator must search for it when the instrument is turned. By changing the lock a little I have been able to replace this button by a more convenient ring, which the fingers cannot miss (Fig. 27). To reach this ring-lock with the fingers, after the stone is seized, requires neither practice nor attention, while one or both are needed with the Weiss's button.

Fig. 24.

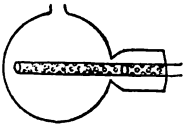


Fig. 25.

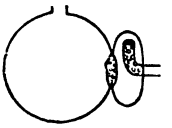
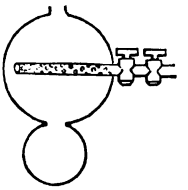


Fig. 26.



Diagrams intended to show how much a straight instrument is shortened by flattening the glass receiver (Fig. 25) or by removing it from the catheter (Figs. 19, 20, 24, 25) to the bulb (Figs. 21, 22, 23, 26). The space may be occupied by stopcocks. The body of the bulb is spherical, and lies in the axis of the catheter, with a hose and a receiver attached to it.

The easiest movement of the right forearm or hand is rotation from left to right—supination. I have adapted the locking of my lithotrite to this motion.* An instrument which can be thus locked and unlocked has this superiority, that not even the fingers of either hand relax their grasp until the stone has been screwed down between the blades. No other lithotrite gives the surgeon this advantage. This lock is attached to the usual American instrument, and I have lately further improved it (Figs. 28 and 29).

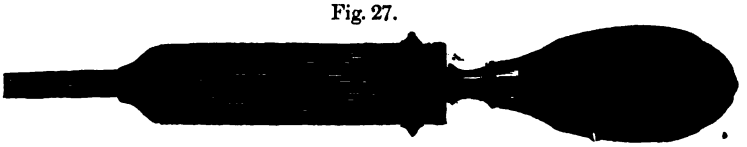
What instrument is most convenient.

We may laboriously educate the movements of the hand to the piano, the violin, or some especial lithotrite, but we cannot alter them. That instrument is the most convenient, which, in accomplishing its object, is best adjusted to the easiest movements of the hand Workmen may become, indeed, proficient in the use of any form of instrument; but the skill of the Japanese wood or metal worker, which is unrivalled, is not evidence of the convenience of his tools, which are rude.

Certain former improvements in the lithotrite.

In the *Lancet*, of November 2, 1887, p. 616, I pointed out the advantages of certain lesser modifications of the usually accepted lithotrite, partly new and partly not. Among these were, besides the increased calibre of the jaws, a large opening at the heel of the female blade into which the flanges of the male blade projected,

Fig. 27.



A lithotrite in which a ring is substituted for Weiss's button to operate the lock.

* This was accomplished by making the usual guides, which prevent the screw from rattling against the sides of the lock, also turn and close it, by the action of the right hand upon the male blade. I have called it the male blade lock.

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so as to drive out the débris and prevent impaction where it is likely to be great; jaws as much at right angles with the stem as can be safely introduced, whereby their efficacy is increased; a thicker toe to the female blade, elongated to prevent the bladder from being seized by the closing blades, and well rounded off to facilitate its introduction; a ball or larger wheel or handle instead of the

Fig. 28.

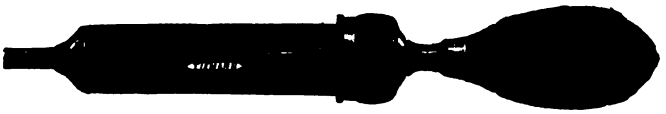

Fig. 29.

Author's lithotrite, having a male blade or right-hand lock, which is closed by a quarter revolution of the lock cover held between the thumb and fore-finger of the operator's right hand. It also has an elongated vulcanite handle shaped to fit the hand.

classical narrow wheel of Civiale, which had been retained for half a century although it excoriates the hand. (See Figs. 28, 29, 30, 31.) The adoption of all these changes in the most common American lithotrite, and their recent valuable endorsement, in his later instrument, by so skilful a lithotritist as Sir Henry Thompson, show their value.

Size of instruments.

Before choosing the instruments for an operation, I measure both the urethra and the stone. But the limit to the size of instruments is not merely mechanical. It is also related to the fact that the membranous urethra is peculiarly intolerant of injury.

Size of the catheter.

The healthy urethra, according to Otis, has a calibre about 32 French, the meatus being about 24. I generally use a catheter about 29 French (18 English), but also smaller sizes, which are often more comfortable to the unetherized patient, and, in a diseased subject, safer. With 25 or 26 you can produce the click of a last fragment or a small calculus, and easily deliver it when crushed. In fact, much of the débris of a stone is in the form of dust, and what is not can be further comminuted.

But when the stone is not a small one, and the urethra is of normal capacity, I use the size 29 or 30, or even 31—as large as the urethra will comfortably admit, not larger. The advantage in doing so is that the smaller fragments need not be crushed again.

Size of the lithotrite.

Strong lithotrites are now in general use for large and hard stones. I like a large lithotrite on account of its powerful, rapid, and efficient action, but it by no means follows that it is essential to use a large instrument to break a small stone. Any small lithotrite answers for small stones. The average stone of these days is small. I always crush a large fragment with a small instrument. A large stone once broken can obviously be treated by an instrument of less size; but once in the bladder, I think the surgeon will generally continue to use the instrument with which he has successfully begun the crushing.

A calculus out of the bladder would be crushed best by a colossal instrument

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at a single grasp. It is the necessity of safely introducing and withdrawing the instrument that determines the size of its blades. The length has most to do with their introduction, the calibre at the heel with their withdrawal through the prostatic and membranous urethra.

Length of the blades.

While long blades slide easily from the meatus as far as the triangular ligament, the delicacy and skill of a practised operator are needed at this part of the passage. On the other hand, once safely in a bladder of fair size, a long blade has a positive advantage in seizing and crushing the stone. The instrument can be readily turned down either way upon the oval floor, especially if the handle be inclined a little to the side towards which the blades are turned. Behind the perpendicular wall of a high prostate, short and blunted blades, which can be inverted readily, are the most convenient.

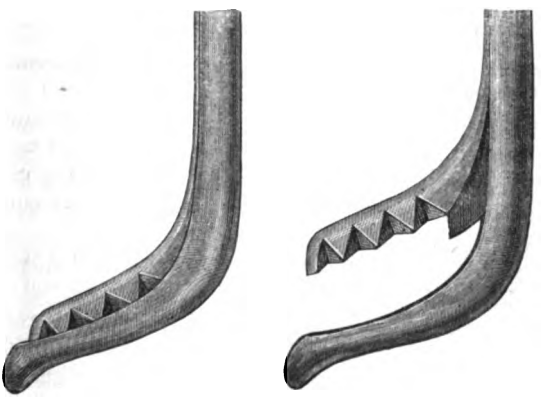
Calibre of the blades.

The calibre of the blades is a question by itself. Blades may safely be large at the heel, which is the largest part, if they do not become clogged with débris, and, by their increased size, endanger the neck of the bladder while being withdrawn. If they cannot be cleared of débris they should be small. Fergusson's method of removing débris with the lithotrite is not now generally in use. No lithotrite that can be cleared is drawn out loaded. It is almost universally agreed that fragments can be removed better through a catheter than in a lithotrite. This fact has led to renewed search for a lithotrite that can be completely emptied and closed before withdrawal, without danger of impaction. It can then be made stronger and larger—at least as large as a smaller lithotrite when it is full of débris, or of the evacuating catheter (26 to 29 French) that is used at the same time—and, as it does not need to be repeatedly withdrawn to be emptied, crushing can be continued as long as it is desirable.

Impaction.

The dust of certain stones mixed with mucus clogs the lithotrite, especially at the heel, with a very hard composition. To prevent this I have recommended a high floor at this part, or, what is the same thing,

Fig. 30. Fig. 31.



Blades of the author's lithotrite. The notches, as here represented, are intended to be deep and steep enough to insure the blades against the usual impaction in front. At the heel, where clogging is always worst, the débris, divided by the spur of the male blade, falls off on each side from the high floor of the female blade. The female blade has a long blunted toe, curved forward. The size of the heel is 26 French.

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low sides, in the female blades; and in the male blade, lateral notches steep enough to push off the débris laterally (Figs. 30 and 31). This plan has been adopted in the more common American instrument. I think also well of the fenestrated lithotrite in practised hands. An instrument of this kind will comminute the stone, especially a hard one, sufficiently to pass the catheters. When lithotritists, three years ago, were warning each other against the danger of fatal cystitis, which had been attested, as they supposed, by the experience of half a century, my first precaution while making experiments, was to protect the floor of the bladder by a solid female blade. A projecting splinter of stone, firmly set in the fenestra of an open female blade, while being crowded through it, is obviously a dangerous weapon. The blades should always be lifted off the floor as soon as the fragment is secured, before crushing it. But it is a great relief to the hand of the operator if the blades shut, at the last moment, quietly together. The old fenestrated instrument can be made to crush as well as cut the stone by widening the female blade and sharpening its rim inside so as to retain it (Fig. 32). This also protects the bladder from any scissors-like action

Fenestrated lithotrite.

Fig. 32. Fig. 33.

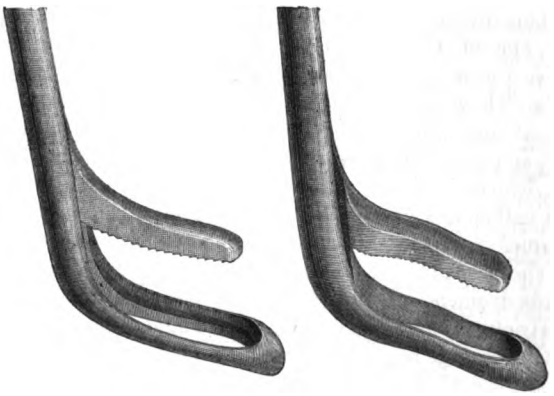

Fig. 32.—Fenestrated instrument, with a wide floor and high rim to hold the stone while being crushed. The toe is long and curved.

Fig. 33.—Fenestrated instrument, with blades curved laterally to widen the grasp upon the calculus. The toe is also curved.

of the edges. The sole of the male blade is grooved lengthwise, and serrated. In order to widen the hold upon the stone in another way, and occupy less room in the urethra, I have used a zigzag fenestrated blade, which has proved quite efficient (Fig. 33). I have employed a fenestrated instrument with great success upon a hard stone, which it crushes and does not so much slice as it does a soft stone. Dr. Keyes first advocated the use of the fenestrated instrument to avoid impaction in the long sitting.

The toe of the female blade should be not only long and rounded, so that it shall not injure the upper wall of the prostatic urethra as it moves along, but I have also curved it forward. Its added length, besides protecting the bladder, then makes the passage easier (Figs. 30, 31, 32, 33, and 34).

Toe of the female blade.

I may add a word about lithotrites. Mine are of several sizes, and of two sorts—the solid and the fenestrated. The latter, of course, cannot clog, and had been sufficiently described. Of the former it may be further said that it does not

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clog, the notches in the male blade being steep enough to deliver the detritus at the side. It is absolutely essential that the notches be deep and well slanted. This implies a thick male blade, to which I have no objection. The crushing is chiefly done at the front of the blades, where, so far as I know, every other

Fig. 34.

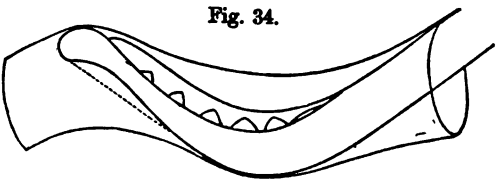

Diagram intended to show how the curved and blunted toe protects the roof of the membranous urethra, and also that the blades in passing, occupy no more room than without it. The position of the instrument here represented is the proper one at this part of the urethra. The dotted line shows that the curve is the result of removing superfluous metal from the sole of a blunted blade.

lithotrite with solid blades clogs. The heel, which never crushes advantageously, has here a long narrow fenestra, cleansed by a projecting metal plate which passes through it. This divides the débris, one-half of which falls off at each side from the high floor, and effectually hinders clogging where it is always worst. These lithotrites have the male blade, lock, and long vulcanite handle figured in the previous pages. The blades represented in Figs. 30 and 31 are of the middle size. They measure at the largest part of the heel 26 French. The stem of the instrument is small, and moves freely in the urethra, but it communicates great power to the blades, which are strong, and adapted to general use.

Passage of instruments.

A straight catheter, which I think evacuates best, generally passes with great ease. Sometimes a curved catheter passes better, and now and then the urethra is capricious about receiving any instrument. In passing an instrument, after dividing the meatus if necessary, or divulsing a stricture, we reach at once the triangular ligament, where the chief difficulty of the passage lies. Here the instrument should be insinuated with a light hand, not by depressing the handle in a quarter circle as if it were a catheter, still less by force, but the aperture should be carefully sought for. Beginners are very apt to strike above. To avoid this mistake, after the instrument has been passed vertically down quite as far as it will go, it should be withdrawn a little before being pushed forward, and this manœuvre repeated till the point drops through the ligament. A student may quickly acquire great skill in passing an instrument in the living urethra by practice upon the dead subject, if he will twist a common tin sound into irregular angles and curves, and pass first one end and then the other through the opening. The ligamentous orifice is really the turning point in the successful passage of an instrument. Once safely beyond it, a catheter, if straight, can be rotated horizontally through the prostatic urethra. But a curved catheter, or a lithotrite, should be lowered only to an angle of about 45° with the horizon, if the patient is on his back, and then urged carefully forward like a boat in the water upon an even keel. The floor of the canal is depressed by the heel of the instrument as it slides along, and this relieves its roof from the pressure of the toe (Fig. 34).

Sounding for a stone or fragment.

I usually search for a stone with a common tin sound bent extemporaneously to suit the case, and measure it with a lithotrite, which also makes an excellent sound. The search for a last fragment or a minute calculus is best done with a small evacuating catheter, by which

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the fragments can escape, or, if too large to pass, are arrested with an audible click. A trap and bulb are attached to it. (See Fig. 18.) In these days fragments are made to seek the sound rather than the sound the fragments, as has been the case heretofore. I think that the old method of sounding for a small calculus is generally abandoned, unless the calculous material is adherent or otherwise immovable.

Evacuation.

Evacuation is a simple thing, and is usually successful, even with an imperfect evacuator. To secure the water I tie an elastic band around the penis after the catheter is in place, and judge of the distension of the bladder by ascertaining, from time to time, the tension of the urethra behind it. The pumping is an easy continuous movement. There is no need of pausing after the stroke, which may be as rhythmical as the swing of a pendulum. If any pause is made, it is after compressing the bulb, to let the débris settle, when there are few left. Every fragment, sooner or later, finds the catheter, most of them at once, and with surprising readiness. The most advantageous spot, higher at first, lower when fragments become fewer, is soon discovered by trial. A quarter of an inch higher or lower sometimes makes an unexpected difference. When the fragments cease to fall into the receiver, the catheter may be immediately withdrawn. But I often add to the evacuation a thorough sounding in pursuit of a possible last fragment, which perhaps does not exist, and thereby make the operation much longer. This search can be deferred if the condition of the patient makes it desirable, although I do not think it more important to abbreviate this surgical operation than any other. Without this prolonged attempt at complete evacuation, verified by a final careful sounding, evacuation is usually a short matter; and the number of grains evacuated in a unit of time, say in a minute, is large.

Obstruction.

Let me illustrate with a single case how the catheter may be obstructed. A patient was relieved, three years ago, by lithotomy, of about a gill and a half of stones of the uniform size of small marbles. A month ago I operated upon him by crushing. The operation occupied two and a half hours, and was tedious in the extreme. Neither the lithotrite nor the evacuator performed as usual. Afterwards it was clear that the crushing was slow, because I could never seize a piece larger than a marble; and evacuation was next to impossible, because the catheter was constantly obstructed by a similar marble. The patient did well. He was seventy-two years old. The quantity removed was nearly 900 grains.

Unfavourable cases.

Among diseases that obviously reduce the chances of a patient, surgical kidney, upon which Sir Henry Thompson has laid much stress, is perhaps the most common and most fatal complication of any operation for stone. Its symptoms have been of late so accurately defined by Professor Guyon and his disciple, Dr. Bazy, that perhaps the most important single question to be solved, if possible before operating, is whether the patient has surgical kidney or not.

Preparatory treatment.

I adopt no special preparatory treatment. If necessary, immediately before the operation, I enlarge the meatus or divulse a stricture. Rest after a journey, the relief of some obviously aggravated and temporary symptom, the antiseptic or other washing of the bladder when desirable—all this belongs to the common principles and common practice of surgery.

After the operation, opiates and warm fomentations are often comforting and sometimes essential. The bladder should be left empty, and, as far as possible,

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kept so. Where complete retention is probable, and it is always possible, the patient should be within reach of the surgeon. The bladder may be washed out with water, or mild antiseptic fluid, if the character of the urine suggests it. One of the most troublesome complications is a mass of mucus that creates tenesmus, and will not pass through the catheter.

Subsequent treatment

I have been cognizant of four fatal cases in which the deep urethra, which is less tolerant of injury than is the bladder, was lacerated. Such cases do not count against lithotrity, but show the need of habit as well as care in passing and withdrawing the instruments, especially the larger sizes. These, in the hands of the general surgeon, are less safe than the smaller ones. Within the bladder the entire wall can hardly be caught with impunity in the forceps-like extremities of the old lithotrite, though strips of mucous membrane have been torn from it without serious consequences.

Accidents.

I think I am justified in assuming it to have been demonstrated that, where lithotrity has hitherto been the accepted rule of practice, as in the case of a healthy patient or a small stone, the new method is better than the old one; and that the domain of lithotrity has been notably extended. I think we need no further statistics relating to small stones.

Statistics.

The question still remaining concerns lithotomy and its indications. It is whether modern lithotrity is not better than lithotomy, in that class of cases hitherto claimed by lithotomy, comprising larger stones and surgical kidneys, conditions which are distinctly unfavourable to any operation at all. It will take time to decide this point. The practised surgeon may even find it so difficult to rid himself of a merely traditional preference for lithotomy, that the verdict may be unnecessarily delayed. But even when lithotomy is successful, I ask myself if the patient would not have encountered a less risk with the new lithotrity? I do not know any new indications that enable us to decide peremptorily when lithotomy is better than lithotrity. The latter does not, it is true, so completely drain and rest the bladder. In the case of a large stone it is, perhaps, likely to be done less skilfully than the operation of lithotomy by the general surgeon. But there is no danger of secondary hæmorrhage, nor is there a grave wound in a diseased prostate, communicating, perhaps, with a putrid catarrh of the urinary tract. As yet no disproportionately unfavourable results have set a limit to lithotrity for large calculi and diseased patients. On the contrary, its use is becoming more common. Extraordinary stones have been successfully removed by crushing.

The question between lithotomy and lithotrity.

I myself add to 21 cases I have published of promiscuous stones, large and small, 24 cases with one death. Of these I report here only 10 cases,—comprising 6 calculi weighing over 200 grains and under 500, 1 over 500 and under 700, and 3 over 700 and under 1,000 grains. The single death was by septic infection, in a patient 66 years of age, having a phosphatic stone of 270 grains. There was no apparent injury of the canal or bladder sufficing to explain it. The only peculiarity of the case was an unusual vascularity of the urethra during the operation.

The manœuvres in a long sitting do not greatly differ from those in a short one. But they need more persistent care and patience, because the attention is rather apt to flag during the continued repetition of the same movements. The possession of a reflex skill, acquired by practice and acting without thought, as in swimming, bicycling, writing, or

What skill is needed in the new lithotrity.

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playing a musical instrument, is then of the greatest value. It insures to the patient, as no mere good intention can, the advantage of what Cadge has called "all the little knacks and tricks which go to make up successful lithotrity." I think, with the improvements here shown, the operation becomes safer for the general surgeon. I often hear of successful operations upon large stones by the new method, in the hands of surgeons who have scarcely done lithotrity before, but I am satisfied that patients are yet safer in the hands of a surgeon who makes lithotrity in some measure a special study.

Nouveaux Instruments pour pratiquer la taille hypogastrique avec le Thermocautère et indications opératoires.

Dr. Théophile Anger, Paris.

La première communication sur l'opération de la taille avec le thermocautère fut faite à la Société de Chirurgie dans la séance du 21 Janvier, 1874. Je présentai à mes collègues un individu auquel j'avais pratiqué, dans des conditions désespérées, les incisions du périnée avec le thermocautère. On m'objecta que n'ayant pas incisé le col de la vessie avec le thermocautère, je n'étais pas en droit d'intituler l'observation "taille avec thermocautère." Ce fut pour répondre à cette objection que je fis confectionner les instruments décrits dans la communication précédente faite au Congrès de Genève. Depuis lors j'ai pratiqué six fois, avec ces instruments, la taille périnéale et dans tous les cas je n'ai eu qu'à me louer de cette façon de procéder. L'urèthre, le col de la vessie et la prostate ont toujours été sectionnés sans hémorrhagie. Toutes les fois que la prostate n'était pas hypertrophiée, l'extraction des calculs s'est faite sans perte de sang. Mais dans les cas contraires, lorsque j'ai rencontré de grosses prostates, l'extraction des calculs a toujours été accompagnée d'une perte de sang notable. Je m'expliquerai plus loin sur les conditions qui favorisent ces pertes de sang pendant l'extraction des calculs, quelque soit d'ailleurs le procédé que l'on emploie. Qu'il me suffise pour le moment de dire que l'impossibilité d'extraire sans hémorrhagie les calculs des individus atteints d'hypertrophie notable de la prostate, me fit songer à réserver pour ces cas la voie hypogastrique; et quoique par cette voie les hémorrhagies soient peu à redouter, je résolus pour d'autres raisons de me servir du thermocautère.

Dans le but de faciliter l'opération, je fis construire les instruments représentés ci-dessous et qui consistent dans une sonde ayant à-peu-près la courbure des sondes d'argent de trousse. Dans cette sonde, creuse et ouverte à ses extrémités, glisse un mandrin dont l'extrémité est formée d'une suite de chaînons articulés. La sonde étant introduite dans la vessie, il suffit de pousser le mandrin pour faire saillir dans la cavité vésicale les chaînons, qui décrivent alors une courbure en sens inverse de la courbure de la sonde. Si l'on abaisse alors le pavillon de la sonde, ces chaînons viennent s'appliquer contre la paroi antérieure de la vessie et la font saillir à l'hypogastre dans une étendue de cinq à six centimètres. Il est alors facile de sentir le cathéter avec les doigts promenés à la surface de la région suspubienne. On s'arme alors du thermocautère et l'on incise, couche par couche, la paroi abdominale. Le soulèvement de la vessie par le cathéter devient d'autant plus apparent que l'on s'approche du but, qui est l'incision de cette paroi. Lors qu'on a franchi l'aponévrose abdominale, et que l'on a refoulé en haut le cul-de-sac

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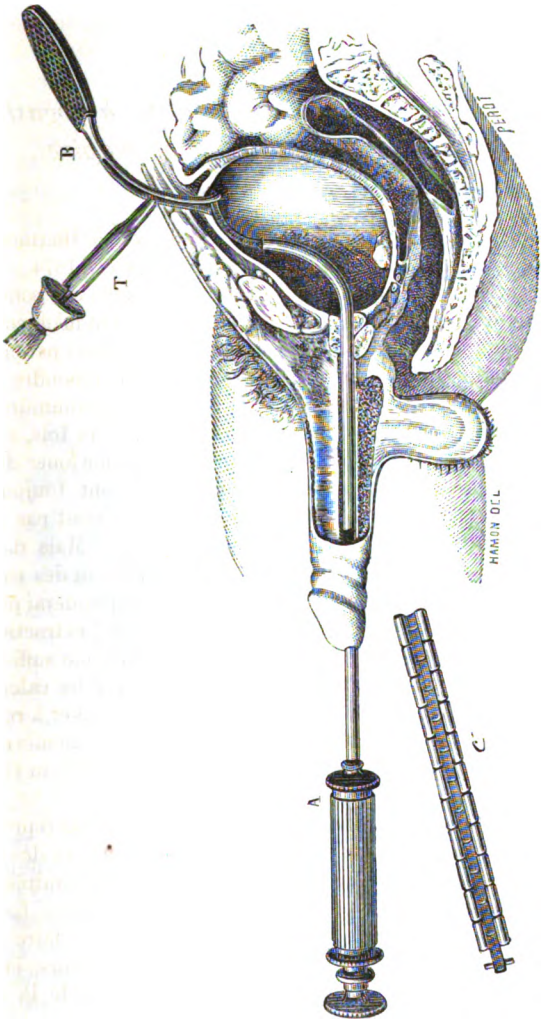
péritonéal, on place à cheval sur le cathéter intravésical un cathéter cannelé et bifurqué, dont la rainure correspond avec celle du cathéter intravésical. Rien de plus facile alors que faire parcourir au couteau rougi du thermocautère les rainures correspondantes des deux cathéters.

La vessie est ainsi ouverte d'un seul coup et avec une sécurité absolue. On saisit les bords de la section avec des pinces à pression pour les empêcher de se

Fig. 35.



A. Sonde et cathéter vésical destiné à soutenir et à faire saillir la paroi antérieure de la vessie. B. Cathéter fourchu et cannelé que l'on applique sur le précédent de façon que les rainures correspondantes permettent au thermocautère de sectionner d'un seul coup la paroi antérieure de la vessie. c Chaîne articulée vue par la face cannelée. T. Thermocautère que l'on fait glisser successivement dans les rainures correspondantes des instruments B et A.

rétracter dans le petit bassin, et l'on introduit le doigt pour s'assurer de la présence, du nombre et du volume des calculs. La figure ci-jointe représente mieux que ne pourrait le faire une description le principal temps de l'opération :—

Revenons maintenant sur quelques détails de l'opération. L'incision de la peau doit commencer immédiatement au-dessus du pubis et remonter vers l'ombilic, en suivant la ligne médiane dans une étendue de sept à neuf centimètres suivant

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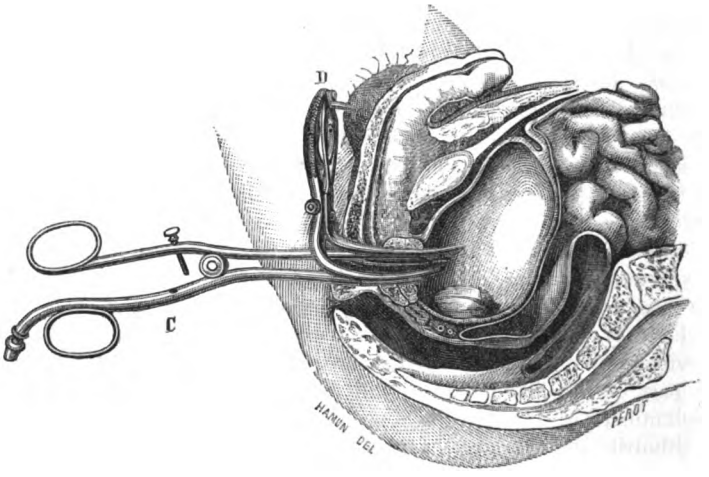
le volume présumé du calcul. Lorsque la section des différentes couches de la paroi abdominale est arrivée à la ligne blanche, je pense qu'il est utile de sectionner cette ligne blanche non seulement dans le sens vertical, mais encore de détacher du pubis à droite et à gauche de la ligne médiane, dans le sens transversal, les fibres aponévrotiques qui s'attachent au pubis. La section ainsi pratiquée prend la forme d'un T renversé ; et voici sa raison d'être. Si l'on se contente de l'incision verticale, la fente pratiquée à l'aponévrose abdominale prend la forme d'une boutonnière peu susceptible de s'entrouvir pour laisser passer, l'urine ; et comme au-dessous de cette boutonnière le tissu cellulo-graisseux est très-lâche, très-extensible, il offre à la stagnation et à l'infiltration urineuse une voie très-favorable ; et on sait que la stagnation et l'infiltration urineuses constituent l'accident primitif le plus à redouter après la taille suspubienne. Il est donc indiqué d'ouvrir à l'urine une voie facile et béante ; et, pour obtenir ce résultat, il suffit de supprimer la boutonnière aponévrotique en la débridant de chaque côté au niveau du pubis. Par cette petite incision en T dont je viens de parler, on supprime l'obstacle à l'issue au dehors de l'urine.

Fig. 36.



Le second temps de l'opération, c'est-à-dire l'incision de la vessie peut présenter quelques difficultés, au moins en théorie. Lorsque le calcul est volumineux, la vessie petite et revenue sur elle-même ; lorsqu'elle est irritable et ne supporte aucune injection, l'introduction et le placement du cathéter peuvent offrir des difficultés d'exécution qui ont été maintes fois signalées. La vessie contracturée sur le calcul peut résister à l'introduction et à la mise en place du conducteur intravésical. Dans ces conditions, le placement et même l'introduction du cathéter vésical étant impossibles, je pense que la taille hypogastrique pourrait être encore conduite à bonne fin si un aide introduisait un ou deux doigts dans le rectum et repoussait en haut et en avant le calcul qui servirait alors de point d'appui au thermocautère pour sectionner la paroi vésicale.

Le grand avantage du thermocautère dans la taille hypogastrique n'est plus, comme dans la taille périnéale, d'épargner le sang des individus épuisés et anémiés ; c'est surtout de produire une section nette, sèche, mettant mieux l'indi-

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vidu à l'abri de l'infiltration urineuse. Je n'ai pratiqué qu'une fois cette opération à l'hôpital Tenon sur un vieillard de 74 ans, très-épuisé, ayant une grosse prostate, un calcul volumineux, et d'une dureté telle qu'il s'échappait des mors du brise-pierre chaque fois que je voulais l'écraser. Après deux tentatives infructueuses de lithotritie, je résolus, vu l'épuisement du malade, de pratiquer la taille sus-pubienne. L'opération fut exécutée à l'hôpital Tenon. Après avoir sectionné la ligne blanche, je détachai l'aponévrose abdominale à droite et à gauche au niveau de ses insertions au pubis. Retroussant alors le péritoine en haut, je sectionnai la vessie. L'urine en s'écoulant permit aux bords sectionnés de la vessie de se rétracter au fond du petit bassin. Avec des pinces à pression j'allai à la recherche des lévres de la section. Cette recherche fut assez laborieuse, et c'est cette difficulté qui me suggéra l'idée de faire construire la sonde armée d'un cathéter cannelé à chaînons (représentée en Fig. 36). Plongeant alors le doigt dans l'orifice béant de la vessie, je retirai le calcul, puis les débris résultant des deux tentatives de lithotritie que j'avais exécutées les jours précédents. L'opération terminée, je laissai dans le bas-fond de la vessie un tube en caoutchouc pour favoriser l'écoulement de l'urine. Aucun accident ne survint après l'opération. Il n'y eut ni fièvre ni menace d'infiltration urineuse. Seulement la cicatrisation fut très-lente, comme on pouvait s'y attendre chez un vieillard épuisé de cet âge. Ce ne fut qu'au bout de deux mois que la cicatrisation fut complète.

J'ai promis plus haut d'exposer les raisons qui dans les cas de prostate très-volumineuse doivent décider le chirurgien à préférer la voie suspubienne.

On dit et l'on répète dans tous les ouvrages spéciaux que la principale indication de la taille suspubienne est le volume et la dureté du calcul. Cette indication serait vraie et acceptable si nous n'avions à notre disposition la lithotritie périnéale. En effet après l'ouverture périnéale de la vessie, il est toujours possible, si le calcul est volumineux, d'introduire dans la cavité vésicale des instruments assez puissants pour le briser et le morceler. Or, si cela est possible, pourquoi répéter que le volume et la dureté du calcul contreindiquent la taille périnéale? N'y a-t-il pas là contradiction dans le langage? On dit d'une part que l'ouverture périnéale de la vessie permet toujours d'introduire des instruments lithoclastes assez puissants pour broyer les calculs les plus volumineux et les plus durs; et, d'autre part, on conseille de faire la taille suspubienne parceque ces mêmes calculs sont volumineux et durs. N'est-ce pas là une contradiction évidente? Puisque, par la lithotritie périnéale, vous pouvez extraire les calculs volumineux, pourquoi conseiller la voie suspubienne alors que vous déclarez la voie périnéale suffisante? Cette indication que vous tirez du volume et de la dureté du calcul est donc fausse, puisque vous pouvez introduire par la taille périnéale des brise-pierre auxquels aucun calcul ne résiste, quelque soit sa consistance et son volume. J'ai donc le droit de conclure de ces réflexions que l'indication de la taille suspubienne tirée du volume et de la dureté du calcul est fausse et illusoire.

Il serait beaucoup plus vrai et plus pratique, à mes yeux, de fonder l'indication de la taille hypogastrique sur le volume et la dureté de la prostate. Déjà un certain nombre de lithotomistes, et notamment M. Reliquet, avaient remarqué avec étonnement que chez les individus atteints de prostate volumineuse la voie ouverte à l'extraction des calculs n'était point en rapport avec l'étendue et la profondeur des incisions. En réfléchissant bien à ce fait, en apparence paradoxal, ils en eussent probablement indiqué la cause. Elle réside en effet dans les conditions anatomiques nouvelles que la prostate contracte en s'hypertrophiant avec les parties environnantes. Son sommet, trouvant une résistance solide dans

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l'aponévrose moyenne du périnée, ne saurait se développer de ce côté, dès lors elle s'élève dans le petit bassin. D'autre part, ses faces latérales en grossissant rencontrent un obstacle non moins invincible dans les parois osseuses ou aponévrotiques du petit bassin. Lorsque l'hypertrophie est assez considérable pour que ses faces latérales arrivent en contact avec cette coque ostéo-fibreuse, la glande se trouve enchâtonnée au fond du petit bassin comme un marron dans son enveloppe. Faites une incision dans cette masse dure et compacte: elle se refermera aussitôt après le passage du couteau, et vous éprouverez autant de difficulté à faire passer un corps un peu volumineux dans cette incision que vous en éprouverez à introduire le petit doigt dans une fente faite à un marron. Non seulement le tissu prostatique proprement dit est indilatable, mais son enveloppe ostéo-fibreuse l'est encore plus; et vous aurez beau multiplier les incisions, augmenter leur profondeur, vous n'arriverez à y faire passer des calculs ou même des débris de calcul qu'en lacérant le tissu de la glande, et par suite vous exposerez l'opéré au phlegmon et même au sphacèle de l'organe, qui aura été contus et déchiré par le passage des instruments et des aspérités de la pierre ou de ses fragments.

Quelles seront les conséquences de ces déchirements de la prostate? Une hémorrhagie primitive d'abord, due surtout à la déchirure des sinus veineux prostatiques. Plus tard, lorsque les parois contuses des artères et des veines l'élimineront, des hémorrhagies consécutives, bien difficiles à arrêter à temps, deviennent presque fatales. La contusion et les déchirures du tissu prostatique favorisent l'infiltration urineuse et le phlegmon prostatique, d'autant mieux que les parois du trajet, un instant refoulés, reviendront en contact aussitôt après l'extraction des calculs, et n'offriront à l'écoulement de l'urine qu'une voie étroite, anfractueuse, très-favorable à la stagnation et à l'infiltration de ce liquide. Il serait certes facile d'énumérer les autres conséquences d'un pareil état de choses; mais elles se devinent aisément, et je ne crois pas avoir besoin d'y insister plus longtemps.

Avant de formuler les propositions qui me semblent résulter de ce travail, je tiens à déclarer que dans ma pensée la taille avec le thermocautère ne saurait être opposée à la lithotritie comme méthode générale de traitement de la pierre dans la vessie. Dans la grande majorité des cas, la lithotritie suffit, et doit être préférée. Mais lorsqu'il s'agit, en présence d'accidents graves de cystite, de pyélite, de débarrasser rapidement la vessie de ses corps étrangers, lorsque surtout l'on est en présence d'individus épuisés, anémiés, minés par la fièvre, incapables de supporter une perte de sang notable, on doit recourir à la taille avec le thermocautère de préférence à toute autre méthode, parceque c'est la seule qui ouvre à l'extraction de la pierre une voie suffisamment large sans perte de sang.

Quant aux indications du choix de la voie à créer pour retirer la pierre, je suis convaincu que ces indications doivent reposer, non sur le volume et la dureté de la pierre, mais bien sur le volume et la dureté de la prostate.

L'opération de la taille étant indiquée, je conclus:—

1.—Que la taille périnéale doit être préférée toutes les fois que la prostate n'est pas notablement hypertrophiée et enclavée dans le petit bassin.

2.—Qu'on doit donner la préférence à la taille suspubienne dans tous les cas où la prostate hypertrophiée et indurée est immobilisée et enclavée dans la loge ostéo-fibreuse et inextensible du petit bassin.

3.—Que l'emploi des instruments que j'ai décrits pour l'un et l'autre procédé, en rend l'exécution facile, méthodique, et épargne le sang des opérés.

4.—Que la plaie qui résulte des sections faites avec le cautère est plus sèche et met mieux l'opéré à l'abri des infiltrations urineuses.

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The Conditions of Stone and of the Urinary Organs favouring the Performance of Bigelow's Operation.

Mr. Reginald Harrison, Liverpool.

The object of this communication is to inquire whether the general adoption of Bigelow's method has tended to extend the limits of the crushing operation for stone, and if so, to what extent and under what circumstances.

It has been pointed out that the introduction of a new operation into surgery, or a modification of an old one, by means of which a greater success was looked for, has more immediately resulted in a larger mortality than previously existed, by reason of the indiscriminate application of the new proposal. Hence it is of importance, in all such questions as this, to fix, as far as it is possible, the barriers which prudence and experience seem to indicate.

In entering upon this subject, it would be unjust to fail to recognize, without reflection upon those who have preceded or are contemporaneous with him, the work of the greatest living lithotritist, Sir Henry Thompson, to whom the profession of surgery is so largely indebted for his contributions to its practice and literature. He has furnished us with facts and figures which must have a relative value to all proposals having for their object the improvement of lithotrity.

The first question I would submit for consideration is—Has Bigelow's method rendered the operation of lithotrity feasible in larger stones than were previously crushed? Speaking generally, I think it will be admitted that, prior to Bigelow's proposals, for stones of over an inch in diameter, a preference, as being safer, was given to lithotomy.

I have recently crushed stones which formerly, by reason of their size, I should have cut for. This point is an important one, inasmuch as if there is here a concurrence of opinion, it will alone tend considerably to extend the limits of lithotrity.

The second question I would raise is—Has Bigelow's method rendered any alteration desirable in the limits which were previously fixed, so far as they relate to the age of the patient? For I take it that experience has universally determined that in childhood and early life, lithotomy, by its safety, is, as a rule, alone admissible, unless the stone be of the smallest dimensions. As one essential in Bigelow's operation is the introduction into the bladder of a sufficiently capacious evacuating tube, I do not see that any alteration is likely to be made in the age limit, as determining the selection of lithotomy or lithotrity.

When stone in the bladder is complicated by some other concurrent disorder, such as enlargement of the prostate, cystitis, paralysis of the bladder, or structural kidney disease—all of which conditions present either an obstacle to the voluntary discharge of broken-up stone by the patient, or are likely to be intensified by the retention within the bladder of such fragments—I consider that Bigelow's proceeding has claims for consideration prior to the selection of lithotomy as an alternative; and I submit that in each of the complications mentioned it will be found to give a greater latitude, so far as the selection of lithotrity is concerned, than we previously possessed.

An analysis of considerable numbers of cases of lithotrity, as previously practised, where a succession of sittings was the rule and not the exception, proves cystitis to be the most frequent cause of death, and further, that the inflammation so intensified or provoked was the result, not of structural damage with the litho-

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trite, but of the retention of débris which led to the production of ammoniacal decomposition of urine, at a greater rate than could be provided against.

It has been stated by Sir Henry Thompson, in reference to some of the complications I have mentioned, that when they exist "it is better, perhaps, to deal with them by one operation."* Formerly such a statement could only have been applied to lithotomy; now it can be used in relation to lithotrity. Hence the latter operation is made to include ground which could hardly have been covered other than by Bigelow's proceeding.

In cutting operations for stone with co-existing kidney or advanced bladder disease, the chief dangers arise immediately from hæmorrhage, and more remotely from inability to heal; these must be put against the shock which may follow the more prolonged manipulations of Bigelow's method—shock which here shows itself most frequently in the form of urinary suppression.

When the process of breaking up the stone and removing the fragments is done by a practised hand, and no damage is occasioned to the parts, I submit that, *cæteris paribus*, the minimum of risk lies with the single crushing operation rather than with the cutting. And what appears to me to be true in theory, I have found to hold good in practice in cases in which, though not free from some degree of complication, I have decided to crush in preference to cutting. I believe that in these directions we have already decidedly gained by the crushing and complete removal of the stone. Where enlargement of the prostate co-exists with stone in the bladder, I am chiefly guided in my selection of the operation more by the facility with which I can seize the stone than by any other consideration.

There are certain states of prostate and of bladder which impose considerable difficulty in the free manipulation of the lithotrite; hence, where the stone is large, the area for action extremely limited by a permanently contracted and hypertrophied bladder, and the process of breaking and removing it is likely to be protracted, I would prefer lithotomy to lithotrity. In one such case where this question arose, the difficulty in manipulating the stone was consequently explain when lithotomy was performed, by the discovery of a large pendulous and valve-like third lobe. This was removed by the knife and fingers after the calculus had been extracted with the forceps. Here not only was the patient cured of his stone but of his enlarged prostate, which was the cause of it. Lithotrity would not have done even one-half of this for him.

In the last place, the relationship of Bigelow's proceeding to stone in the female bladder must not be overlooked. In calculi not exceeding the limits I have indicated, it will, I believe, take the place of other methods of removal, such as extraction with forceps after dilatation of the urethra, or section with the knife, proceedings which are so frequently followed by what is almost as bad as stone in the bladder—namely, permanent incontinence of urine. The greater tolerance of the female urinary apparatus to prolonged manipulations, such as Bigelow's method more or less necessitates, is not an unimportant element in this consideration.

DISCUSSION.

Professor George Buchanan, Glasgow: The few words I have to say are at the suggestion of Sir Henry Thompson, to whom I was indebted many years ago —when I was already a surgeon to the hospital and lecturer on clinical surgery—

* "Practical Lithotomy and Lithotrity," third edition, p. 229.

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for practical instruction in lithotomy, by which I became conversant with the method of operating which I have since practised. Sir Henry Thompson's conclusions are drawn from an experience of 800 cases of stone—my conclusions from an experience of 85 cases; yet I am not sure that my own conclusions, if correctly drawn, might not be of nearly equal value and interest to the majority of practising surgeons as his—because there is not, and probably will not be, a second Sir Henry Thompson who will attract to his own practice the great majority of the cases of calculus not only in his own neighbourhood, but all over the country, leaving a comparatively small number to be divided among other operating surgeons. Men like myself, who operate and teach clinical surgery, will always have a number of such cases to deal with, varying in number from four to eight or ten annually; and the point of interest for us is what is best for us and our patients—not what is best in the hands of men who are operating for stone nearly every day in their lives. Hence, conclusions drawn from a limited experience like mine might well affect the practice of others similarly situated.

Conclusions as to lithotomy and lithotrity:—

I. Small stones and moderate-sized, if not oxalate. I am perfectly satisfied that, in suitable cases as to age and state of bladder, stones of moderate size may be removed by any surgeon of ordinary dexterity by lithotrity with safety and success, and hence it ought to be generally adopted.

II. Large stones, and stones of moderate size, if very hard. I am equally satisfied that in the hands of anyone who, like myself, cannot have a very large experience, and so can never have the adroitness only to be acquired by constant practice, especially in the use of instruments of a size necessary for the removal of large stones at a single sitting, lithotomy is safest and best. I fear that I and men in my position would have great difficulty in removing by lithotrity, at a single sitting, large calculi which we could remove with safety and success by lithotomy. Within the last twelve months I have had six patients with stone in the bladder; of these I was obliged to have recourse to lithotomy in three, and they all recovered—one, a child of two years; another, a youth of nineteen, with a mulberry calculus the size of a bantam's egg; the third, a large hard calculus, but in a bladder diseased for years, with frequent, almost constant, hæmaturia, and with the urine muco-purulent, ammoniacal, and putrid.

Now, I believe that for those whom I represent—*i.e.*, general surgeons, with a limited annual experience of calculus—the only prospect of our having recourse to lithotrity, in a larger number of cases than hitherto—a thing much to be desired—is the early detection of the stone while of a size that could be lithotritized by any operating surgeon. And that is what we are constantly teaching in our clinical classes, and the maxim for all practitioners should be when one of the known symptoms of stone is present—even in the absence of others, examine the bladder. Still, the time has not yet come, and is not likely to come, when all calculi will be detected when of a small size; and, for that reason and others, lithotomy must still be largely practised by the majority of operating surgeons. In connection with that operation I shall conclude by exhibiting the rectangular staff introduced by Dr. Andrew Buchanan in 1848, and with which I and most of my colleagues in the hospitals of Glasgow perform lithotomy. It undoubtedly simplifies the operation, affording to the knife a direct road along a straight grooved rod from the angle into the bladder.

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Professor SPENCE, Edinburgh: I intend to confine my remarks to lithotomy, as my experience in lithotrity is comparatively limited, the reason being that, except in private practice, suitable cases rarely present themselves. During the last five years in my hospital practice, I have not performed lithotrity more than four times, the other cases being of a kind that the most enthusiastic lithotritist would not have attempted—cases in which I generally had thought necessary to have a lithoclast at hand amongst the apparatus. The expectation which Professor Bigelow has expressed, that lithotomy would become less frequent as patients would apply earlier, has not as yet been fulfilled in my experience, probably because the people in the districts from which the calculous patients chiefly come to Edinburgh have the habit of enduring their sufferings until these become so excessive as to force them to apply for relief. My experience of the favourable results of lithotomy in children is such that I should never think of performing lithotrity in them. In regard to lateral lithotomy, whilst, as the President has remarked, there has probably been no great advance since the days of Cheselden, still, there have been modifications so simplifying the operation as to deserve notice. The rectangular staff of Dr. Andrew Buchanan, which Dr. George Buchanan has just exhibited, is one of these. When the method was introduced I tested it on the subject, found it easy of introduction into the bladder, and that its horizontal limb has the advantage of being a straight director, not only giving ease in carrying the knife along its groove, but enabling you to define the extent of the deep or prostatic incision—the principle contended for by Aston Key, but with the advantage of leaving the left hand free. Until the last six or seven years, however, I have never used it on the living, as I had got accustomed to the curved staff. When I did try it, I found it fully answer my expectations, both in children and in adults. In old men with enlarged prostate, however, I thought Liston's curved staff much better, as being easier of introduction, and making the surgeon sure of having the point fairly within the bladder, no matter how deep the perineum, which is not the case with the rectangular staff, if the angle is kept in the membranous part of the urethra. In one of my cases of very large prostate, I found it necessary after the knife was lodged in the groove of the staff to push forward the staff with my left forefinger; although in that case all went well and those present might not notice any hitch in the operation I felt I was on unsafe ground, and I now use the curved staff as formerly. My own principles as to the lateral operation are to cut low in the perineum, with deep decided incision, and sufficiently free to give room for the deep manipulations (for I never cut direct into the angle of the rectangular staff, but in the same manner as in the curved), and then lodge the knife fairly in the groove and carry forward till you feel it impinge on the end of the groove. The cases in which unfavourable results take place are those where the prostate is not only enlarged, but so condensed in structure that it cuts almost like a piece of india-rubber. You feel that dilatation with the finger makes no progress, you require to use the knife to get room for the forceps, and, as you withdraw the stone, you feel the dense prostate wedged against the ramus of the pubis. Now, I agree with Dr. Anger, that if we could ascertain this state beforehand the supra-pubic operation would be preferable, for though you extract the stone, and all may go on favourably—even for some weeks, ultimately bad symptoms supervene not directly affecting the wound, and hence, such cases are often spoken of as dying from intercurrent disease, but they die as truly from the operation as if they had died on the operating table. I cannot, however, see the advantage of the thermo-cautery in the high operation.

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the line of incision is free from risk of bleeding; and, from what I have seen in cases where I have used the thermo-cautery in tracheotomy, the after-state of the wound has not been such as would lead me to try it in lithotomy. We should also keep in mind that, in cases of stone with greatly enlarged prostate, the bladder is pushed up into the abdominal region from the pelvis, and is more accessible for incision and after-treatment, and from what we heard yesterday, in regard to intra-peritoneal surgery, I think the high operation should have in such exceptional cases every prospect of success.

Mr. Walter Coulson, London: It may be confidently asserted that Dr. Bigelow's method of performing lithotrity deserves to take a very high place among the most important improvements in modern surgery. The advantages of withdrawing all the fragments of calculus at one sitting are perfectly obvious, and that this can be done without undue risk to the patient has been proved to demonstration by the success which has attended the numerous cases which have been placed on record. The limits of the operation for cases of larger calculi have yet to be determined, and it is absolutely necessary that certain improvements should be effected in the construction of the instrument, if we are to attempt to crush every large and hard calculus that comes before us. Nearly all the portions of Dr. Bigelow's instrument appear to be admirably adapted for the purposes they are designed to serve. The ball by which the screw power is applied and the revolving cylinder handle, for opening and closing the lock for dealing with large stones, are decided improvements upon the wheel and button of previous instruments, and the power of the blades has been sufficiently tested by numerous cases in my own practice. There appears, however, to be an important defect—a defect, moreover, of a serious character, and one which is liable to show itself when a large and hard stone has to be crushed. I allude to the manner in which the handle is connected with the shaft. This appears to be the weak part of the instrument, as I discovered in crushing a hard uric-acid calculus last November After I had broken up a few fragments, I suddenly found that the screw-power no longer acted on the male blade. I discovered that a small screw and two pins by which the handle of the lithotrite is fastened to the shaft had given way, so that these two portions of the instrument became separated. It was found impossible to keep them together while the screw-power was applied. And no substitute for the broken screw and pins could be readily extemporized. By percussion, however, upon the handle, the blades were freed as much as possible from débris, and the instrument was removed. I completed the operation by means of a fenestrated lithotrite and removed fragments weighing 500 grains. In another case, occurring soon afterwards, a similar accident happened. I was using a strong fenestrated lithotrite, and when the screw-power was forcibly applied, the handle separated from the shaft. I managed to withdraw the instrument, and I completed the operation by using several lithotrites in succession. The weight of the fragments removed was 4 ounces 140 grains. The calculus was therefore the heaviest and probably the largest ever removed by lithotrity. These two cases show, I think, the necessity of increasing the strength of the lithotrite. The accident described caused the operations to be considerably prolonged, and, as a matter of course, increased the risk. It may be that for dealing with large stones, some modification of the method of percussion will be found advantageous. In the first of the cases I have related, percussion was the only means at my command for freeing the jaws of the instrument from débris.

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Another point of some interest on which I should like to hear Dr. Bigelow's experience is whether he has found any considerable difficulty in introducing the evacuating tubes. I find that the repeated introduction of instruments causes an accumulation of sand at the prostatic portion of the urethra, which accumulation interferes with the use of the evacuating tube, but I have always overcome it by attaching the globes, and by compression throwing a little water into the urethra. Two cases have come to my knowledge in the practice of two different surgeons, when after crushing the stone they could not introduce the evacuating tube, and both terminated fatally. With the view of avoiding repeated introduction of instruments, I am in the habit of employing a fenestrated lithotrite for crushing the larger fragments after the stone is broken. In the operation one point should be borne in mind, and that is to be sure the bladder is perfectly empty after the operation, as that saves the patient much suffering. In conclusion I have only to say that in my opinion we have to thank Dr. Bigelow for the greatest advance in the treatment of calculous affections.

Professor PIRRIE, Aberdeen, made some remarks on Cheselden's and Liston's lateral operations of lithotomy and their results.

Mr. TEEVAN, London: If three years ago reference had been made to all the books on the subject then extant it would be found:—(1) That there was a general consensus of opinion that the first sitting of lithotrity ought to be as short as possible, that the surgeon ought not to leave the lithotrite in the bladder for more than two minutes, and that the débris ought to be left to Nature to expel, unless she could not do so. (2) That after the surgeon had found a stone, he was on another occasion to introduce a lithotrite to ascertain its size, in order to determine whether the calculus was to be removed by a crushing or a cutting operation. (3) That no case of lithotrity at a single sitting, followed immediately by external lithotomy, was to be found in the books. Now, regarding the first point, it is clear that Bigelow's operation is not an extension of a recognized principle, but its complete abandonment. Under the old *régime* as little as possible was done at the first sitting, for fear of inflammation being set up by the manipulations. In Bigelow's operation everything is done at the first and only sitting, however long it may have to be extended. Nothing is left to Nature to expel: everything is done for her. Thus, therefore, the principle upon which Bigelow's operation is founded is as opposed to that upon which the older operation was established as the two poles are. It is a new operation; and it will prove an inestimable boon to mankind, for it kills fewer patients and cures more of those it saves than the old operation. It has furthermore greatly extended the field of lithotrity. Under the old *régime* stones up to one inch in diameter only were crushed, but hard calculi double that size can be safely dealt with by the new method. I have myself removed with perfect success an oxalate of lime calculus two inches in diameter from a gentleman sixty-four years old, a patient of Dr. Hewers, suffering from diabetes and albuminuria. The great opprobrium attached to the old operation was that it so often neither killed nor cured; it removed the stone, but failed to cure the patient, who lingered on in a miserable condition for a year or so. Not so, however, with Bigelow's operation, which is not followed by chronic cystitis or phosphatic deposit. Regarding the second point, it was always usual, after the detection of a stone, to introduce a lithotrite on a separate occasion to measure it, and determine whether it ought to be removed by cutting

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or crushing. Now I have for some time abolished the second examination as unnecessary and prejudicial. When once a stone is found, the less the patient's bladder is irritated by the introduction of instruments, the greater are the chances of a successful result. The introduction of a lithotrite for the purpose of measuring the stone can only worry an already inflamed organ. I consider a surgeon ought to go to a stone case prepared to crush or cut—to crush, if possible, to cut, if necessary. The introduction of Bigelow's operation has entirely revolutionized the treatment of stone in the bladder, and there are but few calculi which cannot be crushed. Lastly, regarding the third point, there is a class of cases unfitted for lithotrity, pure and simple. When a stone occurs as a local formation in an old man with an enlarged prostate, who cannot make water except with the aid of a catheter, lithotrity may remove the calculus, but it fails to cure the patient: his aches and pains persist, the urine does not clear up, and a speedy recurrence may be looked for. In such a case there are three indications. To remove the stone, to allow the bladder a physiological rest, and to give the patient a short and ready route to his bladder. This can only be accomplished by lithotrity, followed immediately by external urethrotomy, which combination, I was the first, I believe, to practise. The combination differs in many respects from Dolbeau's operation of perineal lithotrity, to which there are many objections.

Dr. Reliquet, Paris: I wish to describe a crushing instrument devised by myself. The male blade is provided with teeth; the female blade is widely fenestrated, and has transverse teeth in its lateral and upper surfaces, into which those of the male blade fit. Its power is great. The fragments fall to each side; and the remaining slice of stone between the blades is reduced completely into pieces, small enough to be removed by the evacuating instruments. These fragments fall beyond the beak of the instrument, so that it never becomes clogged. With this instrument I have, as long ago as 1870, crushed, by means of the hammer which is a part of it, a large hard stone successfully, as a preparatory measure to lithotomy, thus anticipating the recommendation now given by Sir Henry Thompson. The instrument has been further modified, so as to enable the operator to obtain a larger number of fragments at each act of crushing. The teeth of both blades are provided with sharp smaller teeth, which effectually prevent the stone slipping from the grasp of the instrument. I exhibit also an excavator, (size 23-25 Charrière), provided with a stylet, which is found efficient in freeing the eyes of the instrument if they become obstructed by débris. If, however, the bladder do not contract well, or if sacculi, or a pouched fundus be present, I employ Clover's evacuator, or one of its modifications. These powerful-toothed blades work well both with the rack and pinion, and with the hammer arrangements; a very powerful breaking as well as crushing action being thus obtained, far superior to that which is effected by the simple screw. Whichever mechanism be employed, the male blade should be provided with a ferule, so arranged as to limit the distance to which the male blade enters the female—*i.e.*, to allow its teeth either to project beyond the convexity of the curve of the female blade, or, on the other hand, scarcely to become approximated to it. In passing the instrument into the urethra the teeth of the male blade should obviously not project beyond the level of the female blade.

Mr. Clover, London: Without disparaging the improvements of Sir Henry Thompson and Dr. Bigelow, I wish to point out that it is desirable to make the

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catheter as short as possible; and I object to the elongation in Dr. Bigelow's instrument by turning the handle lengthways, and the interposition of a stopcock between the catheter and the trap. Fragments that would be caught in my own instrument, would have to be drawn two inches further before they reached the Bigelow trap. I also object to the valve, as it obstructs the passage of a large fragment just as it is about to be caught. I arranged my instrument at first with a small trap and an india-rubber valve; but found by enlarging the trap two inches in diameter no valve was needed. I think it would be better to have the inner end of the catheter truncated. The greatest improvement in the instrument is, I think, the arrangement, by a small funnel and stop-cock, for adding to or taking away the water in the aspirator.

Mr. Berkeley Hill, London: I am of opinion that Professor Bigelow has established a fact of which surgeons were previously unaware—namely, that the bladder can bear without injury manipulation of sufficient length to remove a large calculus at one sitting. But I desire to express my firm conviction that there are many patients for whom the gentle operation of Civiale was more suited. These are generally patients of middle age; in whom the deeper portion of the urethra is of extreme sensibility; in such persons dangerous complications are more apt to follow instrumentation during long sittings, than gradual removal of the stone by successive comminution of each large fragment at separate sittings.

Mr. Buckston Browne, London: I have been associated with Sir Henry Thompson since 1874, and therefore I may not be deemed presumptuous in offering a few remarks. (1) I beg to affirm most seriously that the lithotrity of 1877 was not the lithotrity of Civiale. Since, at any rate, the time when I joined Sir Henry, we have always operated under ether, and used Clover's apparatus. (2) We could evacuate bladders before 1878, as witness 600 calculi here present, many showing large fragments, and entire calculi washed out by Clover's bottle. (3) Professor Bigelow said, "before 1878 surgeons had no idea how the bladder would behave after complete evacuation." Ever since I have known Sir Henry, he has taught—if after crushing, there is cystitis—crush, and evacuate completely, however ill the patient, and you will probably cure him. Professor Bigelow will always be splendidly associated with lithotrity, not by reason of his instruments, but by reason of his grand theory, which practice so far has undoubtedly verified.

Mr. Lund, Manchester: Much that I had to say has been anticipated by previous speakers; but I have seen in the practice of the Manchester Royal Infirmary before the year 1878, small calculi crushed by lithotrity, in which the fragments were removed through Clover's bottle. I am not prepared to say the entire stone has been so evacuated, but a very large portion of it, the remainder passing naturally, and no second operation being needed. I would also direct attention to the fact that, in Clover's bottle, the end of the evacuation tube projects so far into the glass reservoir which collects the fragments, as to render it not possible for a particle so caught to return into the tube.

Mr. Teale, Leeds: It is clear that Dr. Bigelow has moved lithotrity forwards from its restriction to calculi of moderate size to those hitherto abandoned as a matter of course to lithotomy, and has practically made the possibility of crushing a stone the only limit to lithotrity. Such calculi are of a size in which

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lithotomy is necessarily attended with considerable risk, and may be termed "boundary stones," being on the now debatable ground between lithotomy and lithotrity. In the hands of the most expert lithotritists I think that a greater number of recoveries may possibly be obtained by Bigelow's operation than by lithotomy. But I agree with Mr. Buchanan, of Glasgow, that there is a risk that, tempted by the brilliant results reported, surgeons, who are not specialists in lithotrity, may be induced to abandon lithotomy for a proceeding which is not without dangers of its own—dangers which can only be overcome by extreme skill and practice. At the same time I feel that lithotomy is maintaining a stronger position in reference to mortality, and in comparison with lithotrity, than in former days, and has advantages of its own. The advantage of lithotomy, in my view, is in the better chance it affords of the cure of the cystitis attendant upon, or in some cases causing, vesical calculus. The fatality of lithotomy I consider to have been reduced in late years mainly by two factors—first, by improved sanitary conditions of hospitals; second, by the more gradual extraction of stone—the surgeon taking pride not in the rapidity, but rather in the care and gradual manner in which he extracts it.

Dr. Otis, Boston: My own experience in the various operations and procedures for the removal of stone from the human bladder has been sufficient to enable me to appreciate very highly the advances which have been made in this department of surgical science and practice within the last few years. The summing up of these advances here to-day and the demonstrations by Sir Henry Thompson, Professor Bigelow, and others, appear to leave but little else to be said, and little room for improvement in regard to instruments, apparatus, and methods. My own personal experience in removal of stone from the bladder by crushing and evacuation at a single sitting, consists of ten operations, prolonged from twenty-five minutes to nearly two hours; with three exceptions (one in a female), these operations have been performed strictly in accordance with the plan and principles of Professor Bigelow. The lithotrites used were Sir Henry Thompson's alone, in four operations. In the remainder Professor Bigelow's large lithotrite was used at the commencement of the operation. The evacuating apparatus was that which (in somewhat less degree of perfection) Professor Bigelow has so successfully exhibited here to-day. The evacuating tubes used were 30 and 31 millimètres in circumference. Recovery took place promptly, and without an unfavourable symptom in every case, thus fully supporting all the claims which have to-day been made in favour of this method of dealing with stone in the bladder. During the present discussion, however, it has appeared to me that while the bladder has been fully considered, and has been shown to possess an infinite tolerance to instrumental procedure in connection with the crushing of stone and the removal of the débris, sufficient importance has not been conceded to the urethra, which, as an element of danger to the patient, may, I think, with the improved instruments for crushing, now be said to constitute the chief factor in the operation. The simple passage of a sound, or even a bougie, has been known to cause an epididymitis, frequently to cause a troublesome retention of urine, and not unfrequently (when disease of the bladder or kidneys was present), to induce a fatal suppression of urine, and this simply from the irritation caused often by the not unskilful passage of such instrument. It becomes, then, a matter of the first importance to secure the greatest possible immunity from danger to the urethra in operations which of necessity draw so largely on the sen-

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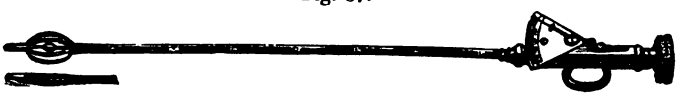
sibility and endurance of this complex and sensitive canal. It cannot, then, ever be otherwise than an imperative duty for the surgeon to ascertain, by all possible measures, the actual condition of the urethra in every case as an essential preliminary to any operative measures for the removal of stone by lithotrity. The urethral orifice is known to be congenitally contracted in the greatest majority of persons. In an operation, then, which makes it desirable to secure the full normal capacity of the urethra throughout, it becomes necessary to measure exactly, or by some reliable means to estimate, the normal urethral calibre in every case, as the first preliminary to an operation for removal of stone by crushing. This may be done by means of the urethrometer which I presented to the profession in 1874. This instrument here shown is introduced into the urethra, closed, as far as the bulbo-membranous junction, is then expanded at the extremity by means of a screw at the handle until the normal capacity of the urethra has been reached. I would here like to be distinctly understood, by the term normal capacity, as meaning the limit of easy distension without injury to healthy mucous membrane. The instrument is

Fig. 37.



Urethrometer.

then slowly withdrawn, turning the screw so as to diminish or expand the bulbous extremity as variations of calibre require. By means of a register or dial on the handle the amount of the variations and of the normal capacity of the anterior urethra may be easily, accurately, and safely ascertained. Division of the meatus to the size thus indicated having been performed, all friction at this point from passage of suitable instruments into the urethra is effectually removed. It appears to me highly desirable that this simple operation should be done, and the parts allowed to heal completely before lithotrity is commenced. This was the practice of the great lithotritist M. Civiale, who is said to have divided the urethral orifice, as a preliminary to further operative procedure, over 2,000 times. When the urethrometer is not available, the normal calibre of the urethra may be estimated by a rule based upon the proportionate relation which the urethra has been found to bear to the size of the organ in which it is situated. Thus, if the flaccid penis measures 75 mm., or 3 inches English, in circumference, the urethra will be found to have a normal capacity of 30 mm. in circumference. If the penis measure 81 mm., or 3¼ inches, the urethra will have a capacity of 32 mm., and so on: 6¼ mm., or ¼ inch, increase in the circumference of the penis, at about the middle of the pendulous portion, will indicate an increase in size in the urethra of 2 mm., the range of the normal urethra, as demonstrated by measurements in several thousand cases, being from 28 to 45 mm. in circumference. Until my observations with the aid of the urethrometer, the normal calibre of the urethra, as claimed by surgeons in every part of the world, did not exceed 21 mm. in circumference. In France, it was fixed at 21. In England and America at 8 to 9 of the English scale, or 17 to 18 mm. in circumference. The estimate by the urethrometer gave an average of 31·84 mm. in 100 carefully-measured cases, where more or less stricture was present, and 32·95 in 100 similar measurements of supposed healthy urethræ. It will thus be seen that much larger instruments may be passed through the urethra, for purposes of crushing and extracting stone, without distension

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sufficient to fracture the urethral mucous membrane, than has been heretofore supposed. Hence, we can understand why the large lithotrites and evacuating tubes of Professor Bigelow, upon which the brilliant success of his operation so essentially depends, have been thus far used, apparently, with as much ease and safety as the instruments of a size of 20 or 21 mm. in circumference, formerly in use by all surgeons who practised lithotrity and evacuation by means of an evacuating tube or catheter. Still further, in regard to the measures to secure immunity from damage to the urethra, and to admit the use of the largest instruments considered desirable, any localized points of constriction in the deeper portions of the urethra should be removed, as an important preliminary to the operation of lithotrity. Subjects of lithiasis are found to have more or less urethral constriction at the peno-scrotal angle from three to four inches from the meatus urinarius, and while this is not sufficient to interfere materially with the function of micturition, it is quite sufficient to obstruct the passage of an instrument of ordinary size into the bladder, and, in an especial manner, to obstruct the withdrawal of a lithotrite, often more or less loaded with the débris of a calculus. From this cause, on one occasion, I have seen the urethra ruptured by the force necessary to withdraw a loaded lithotrite, and on the next attempt to introduce it into the bladder witnessed its departure from the urethra at the point of rupture, and its penetration down into the perineal cellular tissue. I have also seen instances where deep wounds have been inflicted throughout the entire length of the urethra, anterior to this point, by fragments of stone, which, coming readily to this constriction, in the jaws of the lithotrite, have been here arrested, leaving the operator no alternative but to withdraw the instrument, at whatever cost to the urethra; an accident which might have been avoided, had the stricture at this point been recognized and divided previous to operation. I called the attention of surgeons to the significance and importance of contractions at the peno-scrotal angle, in my volume on "Stricture of the Male Urethra," published in New York and in London in 1878 (page 212). Citing the case occurring in my practice some ten years previously, when a young man nineteen years of age was found the subject of contraction at this point. He had never been the subject of any acute urethral inflammation, but he had an oxalate of lime calculus 1¼ inches in diameter. During the operations for crushing this stone, the lithotrite, slightly clogged, was repeatedly arrested at three inches, and withdrawn with difficulty In my subsequent examinations of the urethra with the urethrometer I found similar bands present at between three and four inches (and at no other point), in otherwise healthy urethræ, to a greater or less extent, in nearly every one of the 200 cases thus critically measured. A urethra of full size, say 32 or 33 mm. in circumference, may be thus contracted by linear bands of stricture, insignificant in degree as far as urination is concerned, but which alone may prevent the use of instruments of the desired size in operating upon stone by "lithotrity at a single sitting." All such bands, as well as any other localized constrictions of the urethra, from whatever cause, may be divided (preferably by dilating urethrotomy*) at any point in the ante-bulbous portion of the urethra, with but slight delay or risk to the patient, and thus often materially lessen the dangers of urethral injury during the operation of crushing or evacuating vesical calculi. Such injuries, and in much greater degree those inflicted on

* For method, see Otis on "Stricture of the Male Urethra." New York: Putnams & Sons. London: Smith, Elder, & Co.

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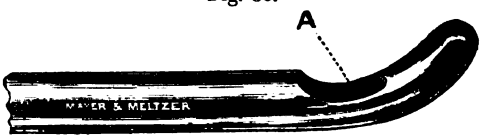
the fixed or deeper portion of the urethra, I believe now constitute the greatest perils to the patient in the operation of lithotrity by any plan yet devised. I also believe that freedom from localized constrictions in the ante-bulbous urethra, affords the greatest security against instrumental injuries to the deeper urethra during such operations. Operations by dilating urethrotomy for the removal of such constrictions do not necessitate traversing the urethra beyond the locality of the constricting band. I fully coincide with Mr. Teevan in the importance of withholding as far as possible the passage of instruments into the bladder, or in any way interfering with the membranous and prostatic portions of the urethra, until the patient is fully under the influence of an anæsthetic preparatory to the operation for the removal of the stone, and more especially in cases of known or suspected organic disease at any point in the urinary tract. Professor Bigelow has given preference to straight evacuating tubes in place of those more or less curved, as more effective in removing the calculous material. I have frequently verified this in my own experience; but I have also found a greater difficulty in entering the bladder with them, especially in cases complicated with enlarged prostate; in such cases it has usually been necessary to introduce a finger into the rectum before the prostatic urethra could be traversed. In order to reduce the friction from introduction of instruments to the minimum, I have modified the straight evacuating tube of Professor Bigelow, by adding a small curved projection, which, with all the advantage of the former, I have found more easy of introduction, and hence less likely to be a source of obstruction and consequent irritation in entering the bladder, and also to prevent in greater degree the closure of the opening in the tube from the engagement of mucous membrane in it during the progress of the evacuation.

Fig. 38.



Modified Evacuator.

The Relations between Adenoma, Sarcoma, and Carcinoma of the Mammary Gland in the Female; their Diagnosis in the earlier stages of the Disease, and the Results of their Treatment by Operations.

Dr. Samuel W. Gross, Philadelphia.

The term adenoma is employed in this paper to designate a neoplasm of epithelial origin, which is composed of hyperplastic or newly-formed acini and ducts closely grouped in a vascular connective-tissue framework. This definition excludes, therefore, those tumours in which normal or pre-existing glandular elements are sparsely enclosed in a fibrous, myxomatous, or sarcomatous matrix.

By sarcoma is meant a morbid growth which is derived from the peri-glandular connective tissue, and which is made up of embryonic or transitional spindle or round cells. These elements do not evince any tendency to develop into higher tissue, but retain their immature type throughout the entire course of the disease.

Carcinoma originates in the glandular epithelium, and consists, structurally, of a cavernous fibrous stroma, the alveoli of which are occupied by loosely-heaped, polymorphous epithelial cells, which are suspended in a serous fluid, without, however, the intervention of a cementing intercellular substance.

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From a clinical standpoint adenoma does not infect the contiguous structures, the lymphatic glands, or the viscera; sarcoma invades the adjacent tissues, and reproduces itself in the internal organs, but without the intervention of glandular implication; while carcinoma evinces its malignant properties by invading the tissues in its immediate neighbourhood, by reproducing itself in the lymphatic glands, and by the development of metastatic growths in distant organs and tissues.

It will thus be perceived that there is a genetic relation between adenoma and carcinoma, but that the affinity ceases with the origin, since the elements of the former neoplasm do not extend into the connective-tissue framework of the mamma or into the contiguous structures. These peculiarities account for the widely different course which it pursues when contrasted with the life history of carcinoma. Sarcoma, on the other hand, has no histogenetic connection with adenoma or carcinoma, but it resembles the latter in its malignant attributes.

1. *Adenoma.*—A peripheral, slowly and equably growing, smooth, nodular or bossed, hard, heavy, and mobile tumour, occurring in married prolific females between the ages of twenty and forty, without any change in the nipple, integuments, or lymphatic glands, may be regarded as an adenoma in its early stages, or before it has undergone cystic transformation, to which it is very liable. The only neoplasm with which it is likely to be confounded is a small fibroma; but the latter is more distinctly circumscribed and isolable, and far more mobile in or upon the mamma, and its outline is not so decidedly bossed. If the growth is observed before the twentieth year, the chances are greatly in favour of a fibroma.

Adenoma recurs in one-half of all cases as the result of inadequate operations; but there is no evidence that it infects the lymphatic glands or distant organs.

2. *Sarcoma.*—The diagnosis of sarcoma in its earlier stages depends upon its constitution. When it is of the fibrous or firm spindle-celled variety, its rate of increase, consistence, situation and mobility render the discrimination between it and fibroma extremely difficult. When, however, a fibrous sarcoma has attained some volume, its progressively rapid increase, elastic consistence, lobulated outline, the attenuated and discoloured condition and tendency to ulceration of the skin, occurrence in married prolific females between the thirtieth and the fortieth year, and freedom from deformity of the nipple and lymphatic involvement, serve to reveal its true nature. A medullary sarcoma, or one composed either of small spindle or round cells, or of an admixture of both forms, is known by its excessively rapid growth, large volume, pseudo-fluctuating consistence, lobulated outline, tense skin, enlarged veins, absence of glandular implication, tendency to the speedy development of metatastic tumours, and by its occurrence in young females.

Of thirty-one cases subjected to operations, which I have collated, seven were living free from disease for an average period of three years; one died in two years and a half without signs of local or general infection; and twenty-three were examples of local return or dissemination. Of these twenty-three, ten had had one or more recurrences, and were still alive at the date of the last reports; five died with recurrence, but without metastases; one died with recurrence and supposed visceral implication; three died with both local and general tumours; and four died of metastases, but without local reproduction.

Among these cases of reproduction at least four are worthy of special attention, since they demonstrate that the removal of the recurrent growths, as rapidly as they appear, not only averts visceral contamination and prolongs life, but that it may also terminate in a final cure. In a case recorded by Heath,* there were

* *British Medical Journal*, vol. i. 1878, p. 194.

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seven recurrences in thirteen years, but the last was not subjected to operation In that of Haward,* the primary growth was excised in 1860, and reproductions were removed in 1863, 1869, and 1873. The patient died from the effects of the last operation, but life was prolonged, as in the preceding instance, for thirteen years. In May, 1865, Gay† enucleated from the same breast two cystic spindle-celled sarcomata. In July, 1867, in May, 1869, in May, 1874, in June, 1877, and in May, 1880, recurrent growths were excised, so that life was extended for fifteen years from the date of the first operation up to the date of the last report. Gross‡ succeeded, after removing fifty-two recurrent tumours, by twenty-three distinct operations, in a period of four years and a half, in checking the course of the disease, and the patient was perfectly well when last heard from, nearly eleven years subsequently. In these four cases the tumours were made up of spindle cells.

From the preceding considerations it follows that the knife cures about one-fourth of all cases of sarcoma; that the disease recurs locally in 61·29 per cent. of all instances after operation; and that the formation of metastatic tumours is not always arrested by interference, since they were discovered in six out of nine post-mortem examinations. It is, moreover, evident that repullulation does not forbid further interference, as the cases of Heath, Haward, and Gay show that life may be prolonged for many years by freely excising the growths as rapidly as they develop, while the example of Gross demonstrates that the remedy may be final.

In view of the malignant nature of the disease—and it is more infectious as regards the general system, but less so as regards the surrounding tissues, than carcinoma—the rule of practice should be to sacrifice the entire breast without trying to save its integuments, to dissect off the fascia of the pectoral muscle, and permit the wound to heal by granulations.

3.—Carcinoma.—The points in favour of scirrhous carcinoma are a history of heredity; antecedent psoriasis or eczema of the nipple, or development from an induration left by puerperal mastitis; appearance during the period of involution of the breast, or after the forty-fifth year in prolific married females; non-occurrence before the age of twenty; irregular, knobby outline; densely hard consistence; relatively slow growth and small volume; dimpling or pitting or adhesion of the skin; retraction of the nipple; immobility of the breast; and induration and enlargement of the associated lymphatic glands.

A soft, lobulated, voluminous, rapidly increasing tumour, occurring in the same class of women, at about the fiftieth year, and accompanied with infection of the glands, discoloration and adhesion of the skin, and retraction of the nipple, is a medullary carcinoma in its early stage.

Colloid carcinoma is indistinguishable from the scirrhous variety; while atrophying scirrhus is characterized by its withering nature, through which its volume is rendered exceedingly small, and its outline is nodular and irregular.

The results of operative interference indicate not only that life is prolonged for twelve months, but that a permanent cure is attained in 10·87 per cent. of all cases.

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* "Trans. Clin. Soc. London," vol. vii. p. 107.

† "Trans. Path. Soc. London," vol. xvi. p. 240; vol. xx. p. 359; vol. xxv. p. 233; and vol. xxxi. p. 272.

‡ "System of Surgery," fifth edition, vol. ii. p. 985.

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The question of permanent recovery must be based upon the study of the typical course of the disease, which demonstrates that certain events which happen at certain times should determine the date at which a cure may be regarded as being assured. Volkmann* states that "If a year has elapsed after operation, and the most careful examination fails to detect evidences of local recurrence, enlargement of the glands, or visceral implication, we may begin to hope that a permanent result will be attained; and that a cure is generally assured if two years have elapsed, while it is rendered almost certain after the expiration of three years." As I have pointed out elsewhere,† the average date of death, with or without interference, is thirty-three months, and local reproduction is witnessed in only one example out of every two hundred after the expiration of three years. Hence I assume, with Volkmann, that the result may be said to be final if the patient survives over three years without local or general recurrence after the last operation, or has died of some intercurrent malady under the same conditions.

Of five hundred and twenty-four cases of scirrhous, medullary, colloid, and atrophying carcinoma, which I have analysed, and in which the result is known,‡ fifty-seven, or one in nine and one-fifth, fulfilled these requirements, fifty-three being alive at the date of the last reports. The average duration of life after operation was six years and five months, and the disease had existed, on an average, for seventeen months and a half before surgical intervention, so that the mean duration of life was seven years and ten months. Of the cases which ran a natural course only 1·5 per cent. survived six years; while of the cures 31·57 per cent., or nearly one-third, were free from disease at the end of six years. In speaking of the duration of life after operation, Sir James Paget says: "I am not aware of a single clear instance of recovery: of such recovery, that is, as that the patient should live for more than ten years free from the disease, or with the disease stationary."§ Applying this severe test, I find that ten fulfilled this requirement, four being free from recurrence for between ten years and one month and ten years and ten months; two for between eleven years and one month and eleven years and nine months; one for twelve years; one for thirteen years and eight months; one for fourteen years and seven months; and one for fifteen years and seven months.

In the final cures the operation was performed when the affection had already existed, on an average, for seventeen months and a half. Hence, it was not practised early, or at a period which is best fitted for securing immunity from reproduction; nor was it by any means always complete. Thus, of fifty-three cases in which the extent of the operation is noted, the breast was amputated in twenty-five; the breast was amputated and the axilla was cleaned out in sixteen; the tumour alone was excised in ten; and extirpation with the removal of infected glands was resorted to in two. Of the entire number, the sixteen in which the breast and the lymphatic glands were completely removed are alone to be regarded as having been subjected to thorough operations.

In connection with the fifty-seven cures there are two interesting points which are of the first importance in influencing the prognosis after surgical interference

* "Beiträge zur Chirurgie," p. 325, 1875.

† "A Practical Treatise on Tumours of the Mammary Gland," p. 164.

‡ These data are derived from the recent publications of Winiwarter, Oldekop, Henry and Estlander.

§ "Lectures on Surgical Pathology," third edition, p. 649.

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In nine instances there was repullulation. In six there was one recurrence; in two there were two reproductions, respectively, in twelve months and four years; and in one there were three recurrences in four years. In these three cases the subjects were free from disease for three and a half, four and a half, and twelve years after the last operation; so that, as in sarcoma of the mammary gland, recurrent tumours should be freely extirpated as soon as they appear. The second practical point is that glandular implication is not a bar to operation. In eighteen, or nearly one-third, of the fifty-seven permanent recoveries, infected axillary glands were removed, and in several of these cases there were nodules in the skin, and the upper layer of the great pectoral muscle was excised. The results of operations indicate, moreover, that infection of the lymphatic glands is more frequent by eleven per cent. when the breast alone is removed than when the glands are simultaneously extirpated, and that glandular involvement is witnessed four months earlier after the former than after the latter procedure.

In conclusion, I believe that I am justified in presenting the following propositions for discussion:—

1.—That from a genetic standpoint there is a distant connection between adenoma and carcinoma, since they both originate from the glandular constituents of the mamma. In the former neoplasm, however, there is a numerical increase of the lacteal glands; in the latter, there is merely a multiplication of the epithelial cells, the descendants of which extend into the lymphatic vessels and the peri-vascular sheaths of the blood-vessels. From a clinical standpoint adenoma is a benign tumour, and carcinoma is a malignant growth.

2.—That sarcoma has neither a genetic nor a structural affinity with adenoma or carcinoma; but that it resembles the latter in its malignant attributes.

3.—That, in view of the recurring tendency of adenoma after simple enucleation, the entire breast should be extirpated with it.

4.—That surgical intervention in sarcoma and carcinoma not only retards the progress of the disease by preventing local dissemination and the development of visceral tumours, but that it also not infrequently results in permanent recovery.

5.—That local reproductions in sarcoma and carcinoma do not militate against a final cure, provided they are freely excised as soon as they appear.

6.—That lymphatic involvement does not forbid operations in carcinoma, since infected glands were removed in nearly one-third of the examples of permanent cure.

7.—That the subjects of sarcoma and carcinoma are, almost without exception, safe from local and general reproduction if three years have elapsed since the last operation.

8.—That all sarcomata and carcinomata of the mammary gland, if there are no evidences of metastatic tumours, and if thorough removal is practicable, should be dealt with as early as possible by amputating the entire breast along with its integuments, and dissecting off the subjacent fascia. In carcinoma, moreover, the axilla should be opened with a view to its exploration and the removal of any glands which were not palpable prior to interference.

DISCUSSION.

Mr. Butlin, London: This subject, which seems so clear to Dr. Gross, is in many parts very obscure and difficult to me. For I have found it almost impossible to classify the connective-tissue tumours with the same care and accuracy as those of many other regions of the body. Their structure is so confused, and so many

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different tissues, developed and embryonic, are mingled in the same tumour, that I have seen only three or four cases of pure round-celled or spindle-celled sarcoma of the breast; although I have sections and drawings of about fifty encapsuled tumours of the breast (*i.e.* non-carcinomatous tumours). On account of this difficulty of classifying these tumours satisfactorily, I have naturally found great difficulty in diagnosing between the various kinds, although I think there is little difficulty in diagnosing between the encapsuled and non-encapsuled (carcinomatous) umours. But Ican quite agree with Dr. Gross that, in the case of any tumour of suspicious character, the sooner it is removed, and the more freely it is removed, the greater are the prospects of complete recovery. And, further, I agree thoroughly with him that, in the case of carcinomatous tumours, it is highly desirable to extend the operation so as to examine the axilla if there be any doubtful fulness, even though no definite enlargement of the glands can be detected.

Mr. Banks, Liverpool: I shall confine my remarks solely to the question of the nature of the operative treatment of mammary scirrhus. Only sixteen years ago I left my medical school thoroughly satisfied that any axillary glandular affection was an absolute bar to operation, believed that the proper mode of removing the breast was to make two incisions, parallel to the pectoral fibres, and enclosing between them the nipple and a bit of skin, to dissect cut the gland, and to lay down the flaps of skin. Doubtless recovery was rapid, and the mortality small from the operation; but recurrence was almost certain. I believe that in all the operations which I saw performed as a student, and for some years afterwards, in only a very few indeed was there any thorough removal of the disease effected. My own practice from the first has been conducted on sweeping principles, and every year becomes more so. I advocate complete removal of the skin over the mamma, leaving a large surface to granulate over rather than that any trace of mischief should be left in the skin, which is, of all the tissues, the one in which it most frequently is widely disseminated. I also advocate the clearing out of the axilla, not merely when the glands can be distinctly felt to be affected, but in every case, as it is impossible to say by external feeling whether morbid action has invaded them or not. I admit the greatest danger to life after such serious and formidable operations, but I consider that by nothing short of this can we hope for a permanent cure.

Des Résections Hatives et des Résections Tardives dans les diverses formes de Maladies Articulaires.

Professor Ollier, Lyons.

In 1860, j'exposai au Congrès d'Oxford (*British Association for Advancement of Science*), mes expériences sur la régénération des os et la reconstitution des articulations, et je formulai les principes fondamentaux de la méthode sous-périostée, c'est-à-dire, de la méthode opératoire qui devait nous permettre d'obtenir sur l'homme les mêmes résultats que chez les animaux. Les résections articulaires venaient de prendre une grand importance dans la pratique, sous l'influence des chirurgiens éminents qui étaient alors à la tête de la chirurgie anglaise; mais on ne se préoccupait guère des moyens de faire reproduire les parties retranchées; on entreprenait ces opérations avec les mêmes idées, et on les exécutait par les mêmes procédés, que du temps de Park et de Moreau.

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On ne tarda pas, cependant, à se préoccuper des ressources nouvelles que les expériences physiologiques offraient à la chirurgie et bientôt affluèrent de toute part des observations de résections sous-périostées. Bientôt même la méthode parut tellement se généraliser, qu'aux discussions passionées qui avaient marqué son début, succéda une acceptation trop facile des résultats annoncés.

Le temps qui nous est accordé par les règlements du Congrès ne me permet pas de vous parler des bases scientifiques de la méthode sous-périostée, ni d'examiner les phases diverses par où elle a passé. Je me bornerai à étudier dans cette vaste question quelques points encore indécis et diversement interprétés, je m'attacherai surtout à l'appréciation des avantages et des inconvénients des résections primitives et des résections tardives.

Les résultats orthopédiques et fonctionnels des résections articulaires sont subordonnés à deux éléments principaux : la méthode opératoire employée et l'état d'altération plus ou moins grande des tissus qui constituent ou entourent l'articulation.

Quelque parfaite que soit la méthode opératoire au point de vue de la conservation de tous les tissus qui doivent reconstituer l'articulation réséquée, cette méthode peut rester stérile si elle est appliquée à des tissus désorganisés ou trop altérés par l'inflammation antérieure.

C'est parceque l'on n'a pas assez tenu compte de ces conditions que les chirurgiens ont eu des résultats divers par l'emploi de la méthode sous-périostée, et que l'on trouve encore quelques opérateurs distingués qui croient pouvoir s'en tenir à la méthode ancienne.

Pour s'entendre, il faut, avant tout, bien poser la question, et surtout ne pas mettre en parallèle des cas qui ne sont pas comparables. C'est parcequ'on a voulu comparer des faits complétement différents par les circonstances dans lesquelles ils avaient été observés, que la solution du problème a été obscurcie ou rendue impossible. De là l'inconvénient des statistiques reposant sur un grand nombre de faits, venus de toutes parts, et qui n'avaient de commun que le titre sous lequel on les avait publiés. Ces statistiques encombrent déjà la science et sont plus nuisibles qu'utiles ; il ne faut pas les rejeter, sans doute, car elles ont, malgré leurs inconvénients, un certain degré d'utilité ; mais il ne faut les accepter que sous bénéfice d'inventaire.

Les faits cliniques sont souvent difficiles à comparer, malgré l'analogie des conditions apparentes, car une foule de circonstances, indépendantes du chirurgien, viennent établir des différences entre eux ; aussi faut-il, quand la chose est possible, recourir à l'expérimentation qui, en nous permettant de reproduire les faits dont nous avons besoin, nous met à même de les analyser et de débrouiller les conditions dans lesquelles ils se produisent.

Aux expériences que j'ai fait connaître à diverses époques, depuis 23 ans, on a souvent opposé la différence qui existe entre l'homme malade et les animaux sains, et la facilité plus grande d'obtenir chez ces derniers des processus réparateurs.

Il n'y a pas de différence sensible entre les actes de la vie végétative des éléments anatomiques chez ces deux ordres de sujets ; chaque jour, à mesure que mon expérience s'accroît, je reconnais que la reproduction des os, et la reconstitution des articulations se font aussi bien et même mieux chez l'homme que chez les animaux. J'ai obtenu dans de nombreuses circonstances, chez l'homme, des extrémités osseuses plus larges, plus régulières, plus mobiles entre elles et plus solidement articulées que celles qu'on parvient à faire reconstituer chez les animaux. J'arrive à ce résultat par la conservation de la gaîne périostéo-capsulaire

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qui constitue le principe fondamental de la méthode opératoire que j'ai proposée il y a vingt ans et que je préconise depuis lors.

Un fait de première importance est venu dans ces dernières années modifier les conditions des résections articulaires, comme celles de la plupart des opérations chirurgicales. Je veux parler de la méthode antiseptique dont le procédé de Lister me paraît être jusqu'ici l'expression la plus pratique.

Ce pansement, auquel je me suis rallié après avoir essayé un grand nombre de procédés antiseptiques a, au point de vue des résections, le grand avantage de faire rendre à la méthode opératoire tout ce qu'elle est susceptible de donner.

En supprimant la suppuration ou en enrayant les inflammations destructives, elle met l'homme dans les conditions des animaux plus plastiques, sur lesquels on a généralement expérimenté ; et l'on obtient beaucoup plus fréquemment qu'autrefois, les résultats sur lesquels l'expérimentation nous avait permis de compter.

Le danger des résections a été tellement diminué par la méthode antiseptique que les indications de ces opérations se sont considérablement étendues. Beaucoup de résections qu'un chirurgien consciencieux ne pouvait pas entreprendre il y a quelques années dans les grands hôpitaux peuvent se faire aujourd'hui, pour ainsi dire, sans dangers. C'est là un des immenses services rendus par la méthode antiseptique qui, en nous montrant que les os et les articulations peuvent, dans certaines conditions, être réséqués ou sectionnés sans accidents, ouvre à notre art es horizons nouveaux.

Mais la puissance de son art crée de nouveaux devoirs au chirurgien, et plus il lui est permis de faire des opérations sanglantes, plus il doit se mettre en garde contre les opérations inutiles ou que des méthodes plus simples peuvent encore remplacer.

La diminution du danger des opérations est un des grands arguments en faveur des résections hâtives. La crainte de la généralisation de la tuberculose est la principale raison qui doit nous décider à retrancher les tissus morbides dans certaines formes d'ostéo-arthrite. Autrefois la fréquence des accidents post-opératoires nous faisait insister plus longtemps sur le traitement expectant ; et quand nous nous décidions à intervenir, c'était déjà trop tard, à cause de l'étendue des altérations locales, pour obtenir une régénération osseuse suffisante et faire reconstituer une articulation sur son type primitif.

On peut, en effet, par l'application rigoureuse des règles de la méthode sous-périostée, obtenir des articulations de même type que les articulations normales. Je veux parler du type physiologique de l'articulation et non de son type anatomique ; car si les fonctions peuvent se rétablir comme auparavant, il n'en est pas de même de la structure de l'articulation nouvelle. Cette structure diffère toujours sensiblement, et quelquefois complétement, de celle de l'articulation ancienne. Chaque articulation présente sous ce rapport des particularités qu'il me paraît intéressant de signaler.

A l'épaule, le type anatomique se reproduit sensiblement ; on a une tête nouvelle, réduite de volume il est vrai, mais s'articulant avec la cavité glénoïde. Cette tête est généralement supportée par un col plus mince que la diaphyse elle-même. Au coude l'articulation nouvelle prend la forme bi-malléolaire, c'est-à dire, qu'elle ressemble à une articulation tibio-tarsienne ; les condyles de l'humérus en se reconstituant par le périoste conservé, prennent la forme de deux apophyses divergentes se terminant en pointe plus ou moins marquée, et embrassant dans leur concavité les deux os de l'avant-bras. Au poignet l'articulation ressemble assez à

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l'articulation normale ; la surface des deux os de l'avant-bras est plane ou moins concave qu'à l'état normal, mais malgré l'irrégularité ou le peu de saillie des apophyses styloïdes (chez les sujets adultes) le type physiologique finit par se rétablir.

A la hanche, le mode d'articulation du fémur et de l'ilium est complétement changé ; le fémur est articulé avec le bassin par de forts trousseaux ligamenteux, mais la tête ne se reproduit pas en général, et bien qu'il soit possible d'obtenir un renflement qui puisse s'insinuer dans la cavité cotyloïde, c'est l'autre forme qui nous paraît devoir être le plus souvent observée, c'est-à-dire, la forme par union ligamenteuse serrée avec changement des rapports du fémur avec l'ilium. Le bassin n'est pas alors supporté par une tête qui s'introduit dans la cavité cotyloïde, mais il est comme suspendu au fémur par des ligaments qui ont de la tendance à s'allonger dans la marche et la station.

Au genou, c'est l'ankylose qu'on a cherchée jusqu'ici ; mais dans quelques cas rares de résection superficielle on peut, par la conservation de tous les moyens d'union, et par leur réunion consécutive par la suture, chercher à obtenir une articulation mobile ou, du moins, se mettre dans les conditions favorables pour avoir une articulation solide dans les cas où la soudure osseuse ne se ferait pas.

Au cou-de-pied on peut avoir une articulation mobile solidement constituée, bien que dans la généralité des cas l'articulation finisse par s'ankyloser, lorsqu'il s'agit d'opérations faites pour des ostéo-arthrites suppurées. Lorsqu'il s'agit, au contraire, d'une résection traumatique primitive, les mouvements tendent à s'accentuer de plus en plus.

Ces résultats ne sont pas constants ; ils sont subordonnés, je le répète, au soin qu'on a mis d'appliquer la méthode sous-périostée et à l'état d'altérations des parties qu'il était nécessaire de conserver.

En opérant de bonne heure, avant la destruction du périoste, des ligaments de la capsule et l'altération trop grande des muscles périphériques, on obtient le maximum des avantages que peut fournir la méthode. Il est évident en effet que pour faire reconstituer une articulation, il faut avoir les tissus qui sont propres à cette reconstitution, et que, si on les a laissés se détruire ou se transformer en tissus incapables de processus plastiques, on ne devra s'attendre qu'à des actes réparateurs incomplets.

Ici se présente une des plus importantes questions qui se rattachent à notre sujet. Les tissus enflammés, suppurés, couverts de granulations, sont-ils inaptes à reconstituer des extrémités osseuses nouvelles ? S'il en était ainsi, il faudrait renoncer à obtenir des régénérations osseuses chez l'homme. Ces conditions inflammatoires, lorsqu'elles ne dépassent pas un certain degré, sont, au contraire, des plus favorables aux processus plastiques. C'est dans ces cas là qu'on obtient les plus belles régénérations.

C'est même en irritant méthodiquement le périoste des animaux âgés qu'on redonne à cette membrane, devenue stérile, ses propriétés ossifiantes.

Je me suis attaché autrefois à démontrer cette proposition par des expériences nombreuses, car le périoste normal, c'est-à-dire, non-préparé par une inflammation préalable, est inapte chez les sujets adultes aux régénérations osseuses. Il semblait donc alors, d'après l'expérimentation, que la régénération des os serait absolument impossible après un certain âge, c'est-à-dire, de l'âge de 30 ou 35 ans à la fin de la vie. Les résections que j'avais faites chez les animaux adultes avaient été, en effet, à peu près steriles ; mais quand j'eus soin d'irriter préalablement le périoste des sujets adultes, par des dilacérations, des déchirures, des

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décollements sous-cutanés, de manière à lui donner le temps de se vasculariser de s'épaissir, j'obtins des résultats tout différents. Ses cellules se remirent à proliférer sous l'influence de cette irritation, et elles devinrent aptes à produire des éléments ossifiables. Le périoste âgé reprenait ainsi les propriétés du périoste jeune.

Il serait trop long de passer en revue toutes les grandes articulations pour justifier les propositions précédentes: je vais prendre le coude pour type, et exposer avec plus de détails ce que j'ai observé à propos de cette résection.

J'ai pratiqué depuis 18 ans 104 résections du coude, et sur ce nombre 99 peuvent être mises en parallèle, parceque j'ai conservé sur elles les documents et pièces anatomiques nécessaires. J'avais eu dans les premières années une mortalité très grande, due en particulier à l'érysipèle ; je perdais un malade au moins sur cinq. Aujourd'hui, grâce à la méthode antiseptique, les conditions sont bien changées et je n'ai pas perdu de l'opération un seul malade sur mes trente-neuf derniers opérés.

C'est déjà là un argument en faveur de la résection hâtive du coude. Dès que sur 39 opérations faites pour la plupart dans un hôpital encombré, on ne perd pas un seul malade, il est permis de pratiquer cette opération toutes les fois qu'on pourra obtenir un résultat orthopédique et fonctionnel supérieur à celui de l'ankylose osseuse.

C'est dans cette considération que nous devons puiser notre règle de conduite ; nous pourrons, grâce à la méthode antiseptique, opérer des résections, bien que la vie ne soit nullement ménacée par la maladie articulaire.

Les résections de l'articulation du coude pour ankylose osseuse donnaient déjà autrefois, avant la méthode antiseptique, une très faible mortalité. Aujourd'hui, toutes les fois qu'on opère sur un sujet non-dominé par une de ces affections dyscrasiques qui constituent une contre-indication à toute opération, on pourra compter sur un succès.

Donc il ne faut pas, comme on le faisait autrefois, respecter les ankyloses osseuse du coude, même celles qui sont réputées en bonne position, et lors qu'on aura affaire à un sujet jeune dont l'existence est considérablement gênee ou la carrière entravée par une ankylose osseuse, on pourra l'opérer.

Je parle des jeunes gens et non des adultes ou des vieillards, chez lesquels une ankylose osseuse datant depuis longtemps a pu conduire les muscles à un état d'atrophie irrémédiable. Mais ici encore en conduisant avec prudence et méthode la traitement consécutif, on pourra considérablement étendre les indications de cette opération. On cherchera toutefois plutôt la solidité que la mobilité ; on peut obtenir, en effet, l'une ou l'autre de ces conditions quand on ne peut pas les rechercher toutes les deux. Il faut, dans le cas de résection du coude pour ankylose, chez l'adulte, tenir les extrémités osseuses rapprochées, et prolonger après l'opération la période d'immobilisation du membre.

Chez les jeunes sujets, dont les propriétés plastiques sont très développées, on peut compter sur une force toujours suffisante, unie même à une mobilite très étendue. Je dois dire, cependant, que mes meilleurs résultats, au point de vue de la force, coïncident avec la limitation des mouvements extrêmes.

Une mobilité complète, aussi étendue qu'à l'état normal, est très avantageuse pour les sujets qui n'ont pas à gagner leur vie par un travail pénible ; elle est particulièrement avantageuse pour les femmes qui ont plus besoin d'adresse que de force dans leurs mouvements. Mais chez les hommes je préfère des mouvements un peu limités: flexion à 45 degrés; extension bornée à 160. C'est dans ces conditions que les sujets peuvent déployer le plus de force. Le crochet olécranien

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de nouvelle formation soutient l'articulation en arrière. Il appuie, dans l'extension, dans une dépression, à la partie postérieure de l'humérus ; il est soutenu latéralement, en dedans et en dehors, par les saillies en forme de malléole dont nous avons déjà parlé, et qui remplacent les condyles de l'humérus toutes les fois que le détachement de la gaîne périostéo-capsulaire a été régulièrement fait à ce niveau.

L'articulation qui se forme ainsi de toutes pièces, entre des extrémités osseuses reproduites, est un véritable gynglime. Elle n'est pas due, comme après les quelques cas heureux qu'on obtenait de loin en loin par la méthode ancienne ou extrapériostée, à la rencontre des surfaces de section de l'humérus et des os de l'avant-bras ; c'est une formation nouvelle ou plutôt le rétablissement de l'articulation ancienne entre des extrémités osseuses de nouvelle formation.

Les autopsies ont été déjà assez nombreuses chez l'homme pour confirmer, à l'égard de ce processus réparateur, les diverses propositions que j'avais avancées dès 1898, en me basant sur l'expérimentation et en analysant la part relative des divers tissus conservés par le détachement de la gaîne périostéo-capsulaire.

Tous les procédés opératoires de la nouvelle méthode doivent être combinés en vue de réaliser autant que possible la conservation intégrale de cette gaîne. Il ne s'agit pas de racler plus ou moins les os qu'on veut réséquer, de rabattre ça et là quelques lambeaux de périoste, comme l'ont fait beaucoup de chirurgiens qui ont publié de prétendus exemples de résections sous-périostées. Il ne suffit pas d'adopter le nom de la méthode : il faut se pénétrer de son esprit et s'entourer des précautions propres à la faire réussir.

La connaissance exacte des processus réparateurs qui suivent les résections doit faire modifier les procédés dans certaines circonstances. Au coude, par exemple, dans les cas d'ankylose osseuse survenue chez les jeunes sujets, la résection sous-périostée, expose toutes choses égales d'ailleurs, plus que la méthode ancienne à la réapparition de l'ankylose. Aussi recommandons nous toujours dans ces cas d'enlever une zone circulaire de la gaîne périostique au niveau où nous voulons rétablir l'interligne articulaire. Une ankylose osseuse a supprimé l'articulation et transformé, pour ainsi dire, les os contigus en un seul os. Il n'y a plus de synoviale et la gaîne périostique a de la tendance à produire des tissus osseux dans toute sa continuité. L'interruption de cette gaîne permet seule d'obtenir sûrement une articulation mobile.

Bien que nous soyons en principe partisan des résections hâtives, il est indispensable de faire les plus grandes réserves à l'égard de cette proposition.

Partout où la résection devra nous donner un résultat orthopédique et fonctionnel supérieur à celui de la terminaison par ankylose, on sera autorisé à intervenir de bonne heure. Au coude, à l'épaule, par exemple, les avantages de la résection hâtive sont évidents. Mais pour d'autres articulations, au genou, à la hanche, les conditions sont différentes et l'on ne saurait trop se mettre en garde contre l'abus des résections. La grande proportion des guérisons qui suit aujourd'hui ces résections ne doit pas nous faire perdre de vue qu'une ankylose en bonne position est encore préférable, en géneral, pour un bon usage du membre. Aussi devons-nous nous armer de patience et ne pas nous presser de réséquer des articulations dont la lésion ne menace pas l'existence. La moindre gravité de ces opérations depuis les progrès de la méthode antiseptique, a porté quelques chirurgiens à en exagérer le nombre et à les pratiquer dans des cas où un chirurgien prudent et sage ne les conseillera jamais.

En recommandant les résections hâtives nous ne conseillons pas de se presser

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pour réséquer des articulations qui pourraient guérir autrement ; nous voulons dire seulement qu'il faut se hâter de pratiquer des résections dès que l'indication de l'opération est nettement établie. Cette indication arrivera hâtivement pour les articulations dont la résection devra donner un résultat orthopédique et fonctionnel supérieur à celui de l'ankylose ; mais elle n'arrivera pour les autres articulations que lorsque la vie sera mise en danger par les progrès de la maladie.

Nous n'avons en vue jusqu'ici que les résections pathologiques, c'est-à-dire, celles qu'on doit pratiquer dans les cas d'arthrite et d'ostéo-arthrite déjà suppurées ou à suppuration imminente. Nous devons examiner maintenant les résections traumatiques, c'est-a-dire, celles qui sont pratiquées pour des plaies articulaires et, en particulier, pour les plaies par armes à feu.

Ici encore la méthode antiseptique est venue modifier les indications des résections; mais, contrairement à ce que nous venons de voir pour les affections spontanées, au lieu de multiplier les indications des résections, les pansements antiseptiques en ont plutôt réduit le nombre, car ils nous permettent de guérir sans opération des plaies articulaires pour lesquelles on avait jusqu'ici regardé la résection comme nécessaire. On peut en effet aujourd'hui, grâce au pansement de Lister, guérir complétement, même avec la conservation des mouvements, certaines fractures articulaires qui étaient autrefois fatalement suivies de la suppuration de l'articulation. Il y a donc là un premier argument contre les résections hâtives, dès que les désordres articulaires ne sont pas très étendus. Nous en trouverons un second dans l'état des tissus interessés dans le traumatisme et dans l'âge habituel des blessés sur lesquels on observe les plaies par armes à feu.

Nous avons vu que l'âge des opérés a la plus grande importance au point de vue de la régénération des os. Dès que la croissance du squelette est achevée, et surtout à partir de l'âge de 25 à 30 ans, les propriétés ostéogéniques diminuent de plus en plus, et semblent même disparaître complétement. Des résections pratiquées sur des animaux adultes sont suivies d'une régénération osseuse très incomplète, et le processus ossifiant fait à peu près complétement défaut chez les animaux plus âgés.

Mais nous avons vu d'autre part qu'on pouvait réveiller dans ce périoste ses propriétés ostéogéniques ; qu'il s'agissait seulement de le soumettre à une irritation préalable. En irritant ainsi le périoste des sujets adultes on redonne en partie à cette membrane les propriétés de la jeunesse ; on réveille l'activité de ses éléments ostéogéniques ; on obtient alors des résultats comparables à ceux qu'on constate sur les os atteints d'inflammation chronique.

Le même fait s'observe après les blessures par armes de guerre. Si l'on fait des résections immédiates, le jour ou le lendemain de l'accident, on trouve un périoste mince, très adhérent à l'os et dont les propriétés ostéogéniques ne pourront pas se réveiller, surtout si une inflammation aiguë envahit le blessure. Le résultat final sera mauvais au point de vue de la reproduction.

Si, au contraire, on ne se hâte pas de faire des résections ; si l'on cherche d'abord à conserver le membre sans opération ; on se ménagera les meilleures chances pour une reproduction osseuse dans le cas ou l'on sera obligé d'en venir plus tard à l'opération. On trouvera un périoste épaissi, vascularisé, à couche ostéogène reconstituée, et l'on obtiendra des régénérations bien plus belles qu'après les résections primitives.

Il ne faut pas se laisser guider en principe par cette considération. Il faut avant tout songer à conserver la vie de son blessé ; or il est des cas où une résection immédiate est impérieusement indiquée. Mais lorsqu'on pourra attendre,

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les pansements antiseptiques éloigneront le danger de l'infection pyæmique, et l'on aura tout avantage à ne faire que des résections secondaires.

Les résultats orthopédiques et fonctionnels des résections pratiquées dans les dernières guerres ont donné, il est vrai, beaucoup de déceptions ; mais il suffit de considérer dans quelles conditions ils ont été obtenus pour voir qu'ils n'ont qu'une valeur limitée au point de vue de la question en litige. L'imperfection des procédés opératoires, le défaut de soins consécutifs, le trop grand nombre des résections primitives expliquent l'absence de régénération ou la mauvaise reconstitution de l'articulation dans la plupart des cas. A côté des nombreux insuccès, quelques résultats brillants ont été publiés ; ils sont peu nombreux, sans doute, mais il suffit qu'on ait pu les obtenir pour qu'on les obtienne toujours quand on se mettra dans des conditions analogues.

La plupart des résections pratiquées n'avaient de sous-périostées que le nom. On les a généralement exécutées sans suivre les règles opératoires de la méthode sous-périostée, sans se pénétrer de son esprit, et on n'avait qu'un appareil instrumental défectueux.

Il ne faut donc pas juger la valeur de la méthode sous-périostée dans la chirurgie d'armée, par les faits qu'on a publiés sous son nom. Il faut au moins attendre de nouveaux matériaux pour porter un jugement définitif, et, tout en tenant compte des difficultés spéciales que comporte le traitement des blessés militaires, ne pas oublier que les résections traumatiques faites dans la pratique civile sont tout-à-fait d'accord avec la théorie que nous venons d'exposer.

Results of fifty-two Cases of Excision of the Knee-joint for Strumous Disease.

Professor Kocher, Berne.

For strumous disease, in the sense of what Germans call "Arthritis fungosa," I have performed excision of the knee-joint fifty-two times. Though this number is not a large one, I think it to be of a certain value to have all the results of one surgeon taken without any selection, the more so as the advisability of the operation in question is not so generally acknowleged as, I think, it ought to be.

If, in 1872, Thomas Bryant, in his well-known "Manual of Surgery," after a careful comparison between amputation and resection, still thought the former to be generally preferable, and only surmised that resection might become a remedy for the cure of recent cases more rapidly, it may, perhaps, be a matter of surprise that since the introduction of Lister's method, resection of the knee-joint has not been executed on a larger scale.

Since the introduction of the antiseptic method, based on real principles by your, and I may be allowed to say our, great Lister, the mortality of operations is not the same as it was, and I hope Professor Volkmann will some day show you that, in a considerable series of operations done by his skilful hands, no fatality has occurred. Sack, Nussbaum, and Fowler of New York, have demonstrated that the mortality after excision of the knee has greatly diminished.

I have had six cases of death out of my fifty-two operations. If I add to these twelve others with no fatal termination, in the practice of my former

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assistants, Dr. Kaufmann, at Zurich, and Dr. Feurer, at St. Gall, who have kindly allowed me to make use of their cases, we have sixty-four cases in all, with a mortality of 6—*i.e.*, 9 per cent. Of these, three died of septic poisoning, one of hæmorrhage, one of carbolic intoxication, and one of acute miliary tuberculosis. Two of the septic cases occurred soon after the introduction of the antiseptic method, and were made septic, as I am convinced, by the excessive application of strong solutions of carbolic acid. The third case was the result of a large bedsore in an idiotic individual, who could not be kept clean.

So we may be justified in saying that for all those surgeons who know how to treat their cases on antiseptic principles, excision of the knee-joint has, by this time, lost its dangers. There is, nevertheless, one danger which is connected with the employment of Listerism—that is, carbolic acid intoxication, of which a good many cases have been published. I, therefore, for these operations, employ a solution of 25 centigrammes of chloride of zinc to 100 grammes of water, after having carefully ascertained the antiseptic properties of such a solution. Out of our twelve cases in which we employed this method none died. But there are two other objections made generally to excision of the knee-joint: one, that we do not always cure the disease by the operation; the other, that it only aims at producing a stiff limb. As to the first point, my statistics give the following answer: after deduction of the six cases of death and five cases still under treatment, the result of the other cases is this: in four cases amputation had to be performed afterwards, of which all recovered, but one has since died of tuberculosis. Two others of the resection cases have also died of tuberculosis since they left the hospital; in four cases re-excision had to be performed, twice because a curved knee was the result; twice the indication was the non-healing of the wound. So there remain thirty-one cases for the question of final result. Of these, five cases could not be examined, the addresses of the patients being undiscoverable. Three of them had left the hospital with sinuses, two were entirely healed. All other cases I have seen and examined, and I was able to take photographs of most of them. Only two of these twenty-six cases have sinuses remaining; all the others are entirely healed; and all, including even the cases with sinuses have useful limbs. In two cases only has any apparatus to be kept on, because there is false ankylosis or even "schlottergelenk" (flail-like joint). As to the last point, the question of stiff joint. In only one of the last cases have I expressly tried to get a movable joint. The patient left his bed after six weeks, and has now the power of flexion to an angle of 10–15° and of active extension.

Now, allow me to make a few remarks as to the method adapted for late and early cases. Where there is already stiffness, contraction and atrophy of the muscles, there can be no question that we must try to get a stiff limb. I greatly prefer to all others the incision of the late Sir William Fergusson, who deserves such great credit for his labours in connection with the question of resection; but I would lay great stress upon two points, which I think are quite indispensable to a good result. I not only excise the bones, but I leave only skin, nerves, and vessels, and remove entirely all the other parts belonging to the knee-joint. So I obtain a very simple and entirely clean wound, and am able to get rid of all that fungous material which is so apt to cause sinuses.

The other point is the mode of sawing the bones. The femur is cut convex, the tibia concave, so as to give an exact co-aptation of the two bones without any other artificial means. The method of excision is entirely different when my aim is to preserve a movable joint. Here I adopt Langenbeck's incision with such

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modifications as are necessary to secure good drainage. With our method of cutting the bones there is no danger of getting lateral movement. But it is essential not to cut the connections of the capsule with the bones on the anterior and posterior aspects of the joint. For this purpose also, the curved section of the bones is excellently adapted. We leave the quadriceps, outer layer of patella, and ligamentum patellæ, as well as the greater part of the crucial ligament, intact, so that passive movements may be begun during the first weeks after the operation as soon as the wound has healed.

My conclusions drawn from these too limited observations are:

1.—Excision of knee-joint is not dangerous, when done according to Lister's antiseptic principles, and when carbolic intoxication is avoided.

2.—Most cases of strumous disease of the knee can be entirely cured by excision, if no fungous material be left behind.

3.—A cure is obtained much more rapidly than by any conservative method. I have had two cases healed completely without any sinus remaining in a fortnight, two cases in three weeks, one in four weeks, and one in five weeks.

4.—With very few exceptions, excision gives very useful limbs, much more so than the conservative method.

5.—In early cases an attempt should be made to obtain a movable joint.

Specimen of reproduction of the Hip-joint after Exsection, in a Case complicated with Pott's Disease and Albuminuria: with a Synopsis of the Results in Seventy-one cases of Exsection of the Hip-joint.

Dr. Lewis A. Sayre, New York.

The patient, a little girl, first came under the care of Professor Sayre, at the age of 2 years and 9 months. The disease, which commenced spontaneously, was then of about nine months' standing, an abscess having opened about one month before admission.

The child was very small for her age, being apparently not above 18 months old, she was very emaciated, and the abdomen was much distended. When placed in the recumbent position, the left lower extremity was slightly flexed, rotated inwards, and adducted, so that it crossed the limb of the opposite side. Two months after admission the left hip-joint was excised. The head and neck of the femur and part of the great trochanter were found to have been absorbed, and the acetabulum was perforated. The upper extremity of the femur was sawn off opposite the trochanter minor. Three small pieces of dead bone were prised off the acetabulum. The wound was washed out with carbolic lotion, dressed with oakum and balsam of Peru, and wire breeches applied.

The child progressed favourably with the exception of occasional elevations of temperature and a slight attack of bronchitis. At the end of a month, the excision wound had closed but a sinus behind still remained open. Six weeks later, this sinus was still open, but there was little discharge, and the child was much improved in every respect. Dr. Sayre's side splint was applied.

Seven weeks after operation, the patient could sit up in a chair.

Six months after the operation, the posterior sinus still remained open, but the patient was much improved in general condition, the splint was discontinued,

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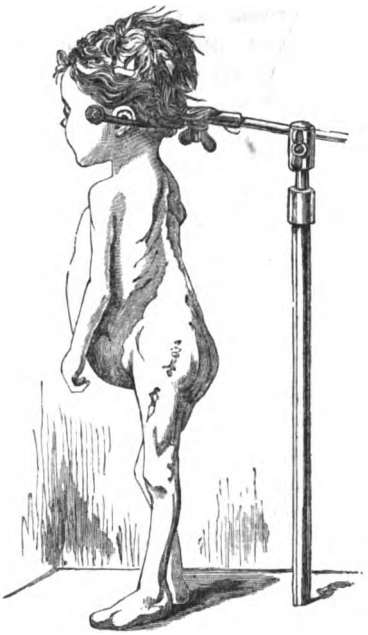
and passive movements were made daily. The diseased limb was at least a quarter of an inch longer than the sound one. Six weeks later, an abscess formed, and was opened, and after this time Dr. Sayre saw no more of the case until two years and two months after the operation.

She then came again under his care, with an enormous waxy liver, and in a hopeless condition. The limb was slightly flexed and adducted, and one sinus existed over the left hip, from which pus escaped. She suffered no pain, but remained quietly in bed, the urine and fæces were passed involuntarily. She lingered on for four months, when it was noticed that there was a very sharp prominence over the spinous processes in the dorsal region, and one also in the lumbar region. The breathing became rapid and laboured; and steadily sinking, the child died three years and six months after the operation.

On post-mortem examination, the organs were, as was diagnosed during life, in a waxy condition, and traces of old tubercular disease were found in the lungs.

Fig. 39.



Showing position of limbs after death.

After death, a photograph, from which the accompanying woodcut was obtained, was taken by Mr. Mason, of Bellevue Hospital, by simply suspending her in a head rest. It will be observed that the limbs are nearly normal in position, and they assumed this position by their own gravity, without any extension or traction being applied to them. The limb operated upon is, in fact, the straighter of the two, and is not so much flexed at either hip or knee as the other. A sharp angular projection is distinct over the third dorsal, and another not so prominent over the first lumbar, vertebra; the enormous abdomen is markedly conspicuous.

Length of body, 30 inches; left lower extremity 13 inches from anterior superior spine of ilium to external malleolus, right limb 13⅓ inches long between same points. Length of both limbs from trochanter major to external malleolus 13⅓ inches (these measurements were made by Professor Stephen Smith).

Circumference of body over umbilicus, 40 inches; over false ribs, 20 inches. A sharp, angular projection observed over third dorsal vertebra, and another not quite so prominent over first lumbar. Over left hip exists the line of an incision, 2 inches in length, all healed except a minute opening at its upper part. A second cicatrix, one and a half inch long, on the outer surface of the thigh. Behind is a third incision, which discharges a drop or two of serum on being squeezed. As the body is placed on its back, the left limb becomes straight, and movements at the hip-joint can be made.

Spinal column, pelvis, and upper half of both femurs removed *en masse*. Left hip covered by a thick mass of fat. A transverse section of left hip made through

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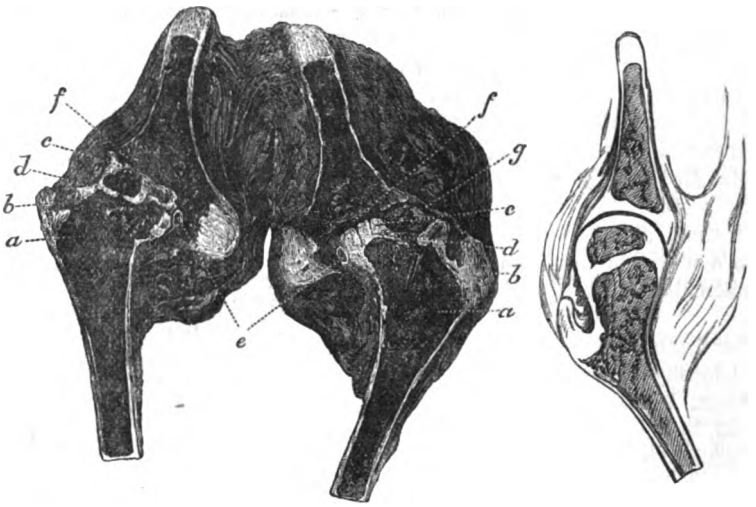
joint. Upper end of femur found to rest in old acetabulum, and to be united to it by a mass of vascular and apparently fibrous tissue, which admits of considerable movement. A probe can be passed from the posterior sinus down to this mass. This is the only sinus left, no carious bone found.

Fig. 40.



aa, newly formed head and neck of thigh-bone. Right coxa, cut open, normal.
bb, newly formed trochanter.
cc, newly formed articular cartilage (proved by microscopic examination).
dd, newly formed diaphysal cartilage (proved by microscopic examination).
c, only sinus left, permeable for a small probe leading to no dead bone.
ff, loose connective tissue, uniting the head with femur, allowing some motion.
g, Newly formed firm capsule.

The specimen was immediately sent to Dr. Heitzmann for examination, who made a very accurate drawing of both hips, from which the photograph seen above was taken; and also a minute microscopic examination of the newly-formed hip, by which it will be seen that not only was the bone reproduced very nearly in form and size as well as length of the opposite one; but also, that true articular cartilage had been newly formed, and the motions of the joint were quite free.

The lesson to be learned by this specimen is, that if Nature can produce such good results under such unfavourable circumstances, and in such a depraved constitution, that we certainly are justified in performing the operation under more favourable conditions.

Synopsis of 71 cases of exsection of hip-joint for morbus coxarius :—

Up to 10 years of age	45
From 10 to 20	19
Over 20	2
Unrecorded	5
	71

of which 46 were males and 25 females.

Forty-seven cases are now living with more or less useful limbs, and twenty-four are dead.

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Of the twenty-four cases that died, nine had recovered from the operation some time previously to death, which was caused in each case by some other disease foreign to the operation.

Of the fifteen remaining, four died from acute intercurrent diseases, such as tetanus, double pneumonia, dysentery, and sunstroke, this leaves but eleven, which have died from the exhausting effects of hip disease without some intercurrent complication.

DISCUSSION.

Mr. John Croft, London: I regret that, owing to want of time, I cannot dwell upon the interesting papers of the preceding speakers. I would define as "late excision" an operation performed on a wasted exhausted patient, about whose diseased joint are old-established sinuses, and signs of displaced bones. Two points bearing on the motives for relatively early excision are, the frequent presence of sequestra in joints, and the frequency of tubercular disease. It is a known fact that many patients suffering from chronic articular disease, and presumably tubercular disease, get well without any operative interference. I would direct attention to the interesting fact that the statistics contained in the Report of the Clinical Society's Committee on Hip-joint Disease show a saving of one year on the average duration of disease, when treated by excision. I express my confident expectation that the rate of mortality after excision of joints will steadily diminish as improvements are established in the methods of operating, and of after-treatment. Seven out of thirty-three cases of hip-joint disease, cured in the first stage, presented 3¼ inches shortening, or as much as ordinarily occurred after excision; and I look upon the objection raised against excision—that it causes shortening—as a trivial one. Time prevents me from referring to any of the other interesting questions contained in the papers which have been read.

Mr. Howard Marsh, London: The soundness of the proverb that "Prevention is better than cure" is daily becoming more clear in the field of medical science. It is possible that we may one day be able to meet typhus and typhoid fever by a direct antidote; but the great probability is, that the true method of dealing with these diseases is by prevention. The same is true in surgery. We must prevent disease when we can; and when we cannot prevent, we must, for the next principle—as we saw yesterday in the discussion on lithotrity—aim at applying treatment in the early stages of disease. Many large and formidable operations, involving considerable risk to life, are now in use because we are called upon to treat cases that have reached so advanced a stage that nothing but some radical proceeding is sufficient. This is notably the case in diseases of the joints. In former times amputation was the only resource within the surgeon's reach. But at length Sir William Fergusson, developing the principle of conservative surgery, which was his constant guide, and of which he became so great a master, substituted excisions for amputations—that is, he cut out the affected joint, and so saved the limb. This, however, is only a link in the chain of progress, and when we are considering, in our turn, what is the next step to take, we are, I think, called upon to remember what excision really is. It belongs to the same class of treatment as amputation, for in both cases we give up the attempt to cure the disease. Instead of this, we cut away the part in which the disease is placed. To excise a knee-joint is the same form of practice as to excise an eye or a testis; but in the case of these latter organs the great progress made in recent years lies in this: that we have learned how to save them by curing the diseases by which they are

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attacked. And when we find ourselves discussing the comparative value of early and late excisions of the joints, are we not bound to follow the parallel I have drawn, and ask how we could, at the present day, seriously discuss the question of late or early excision of the eye, or the testis—that is, their removal for incipient disease? Surely, to work in this direction would be to turn the dial of progress many degrees backward. And I feel some difficulty in speaking to this question, for we must answer it differently, from different points of view. No doubt, as to mere healing of the wound, early excision will give a better result than late excision. But as regards the future condition of the patient, we must remember that, by taking this short cut to what we call his cure, we deprive him of one of the most important organs of his body. I cannot think that surgery has reached its final stage when, instead of attempting to cure incipient disease, we are invited to adopt the simple expedient of cutting out the affected part. For let us bear in mind that these cases, if treated while they are incipient (that is, in the stage in which it is now proposed to cut them out) are certainly curable. Many are cured every year in hospital practice, while in private practice excision is almost unknown. Our true course, I believe is, first, to let it be known that these cases are curable, if only we can get them in their early stage; and, secondly, to point out the necessity for extended hospital accommodation, which will allow us to treat the children of the poor, as we do those of the rich, by rest and good apparatus. When these objects can be obtained, surgery in this department will be, as it is becoming in others, not a mere mechanical art, by which parts are cut out, but a science engaged in cultivating a knowledge of disease, and of its cure by the gentlest means, and with the least possible sacrifice of, or interference with, the structures in which the disease is placed.

Mr. T. P. Teale, Leeds: I had intended to write a paper on the arrest of incipient joint disease by subcutaneous incision of the capsule, but have been unable to accomplish it. In respect to hospital patients we rarely face the question of treatment until considerable damage to joint structures has taken place—they continue for a time out-patients, or they are put under a period of rest and splints in hospital. It is probably during these periods that surgical attempts to relieve joint distress will be of most avail, and will render effective the necessary adjuncts of "rest," whether by subcutaneous drainage of serous effusion, or external drainage of pus, or even, as has been already done by some surgeons, actual boring into congested distressed bony structures contiguous to the joint cavity.

Mr. Frederick Treves, London: I would approach the present subject from a standpoint somewhat neglected in the discussion—viz., from a pathological point of view. There are two questions to be asked on this subject: 1. Is the process in strumous joints and bone disease tubercular? If tubercular, what significance does one attach to that term? In the first place there is now no doubt that the process in white swellings and strumous bone disease is decidedly tubercular. Microscopic research and experimental inoculation have proved this. If this be the case what is the clinical indication? Many assert that if the process is tubercular the diseased part must be removed at once, because the process is sure to extend, and in time to end probably in phthisis or some fatal tubercular disease. This argument in favour of early excision is ill-founded. These patients do not die of tubercular disease except in a few cases. M. Keiner, in *L'Union Médicale*, refers to eighty-one cases of strumous bone and joint disease whose

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future was known, of the number only six died of tuberculosis. The process in these joint affections may undergo spontaneous cure, but that cure is not apt to take place except under the most favourable circumstances. In the case, therefore, of the rich, and in those who can be placed under like favourable circumstances to the rich, the expectant treatment appears to be indicated; but in the poor, in those whose hygienic surroundings are bad, who cannot undergo long treatment, it would appear well, in marked cases, to remove the disease, and, what is more, to remove it freely and at once.

Professor REDFERN, Belfast: I am glad of the opportunity of inquiring if any member of the Section can explain why changes induced in cartilage tend to heal, whilst diseased actions in bones and synovial membranes so frequently extend and destroy the joint or the life. I have shown that incisions in articular cartilages in animals heal by the production of fibrous tissue, that cicatrices of fibrous tissue are common in the cartilages of the joints of man, that healing takes place after the destruction of portions of the articular cartilages of animals by caustics or cautery. Yet articular cartilages may disappear rapidly in inflamed joints, and the costal cartilages are very rapidly converted into a connective-tissue mass at parts which have been injured by setons or ligatures. In healthy animals I have failed to excite disease in the articular ends of bones or their cartilages by passing wires through the bone close to the cartilage, and keeping them in for months.

Mr. J. K. BARTON, Dublin: The whole subject of excision of joints is a wide one; and may be broadly divided into those of the upper or lower extremities; the former are nearly out of the line of criticism, it being generally admitted that in the case of the shoulder, elbow, or wrist, excision should be the rule. It is to the joints of the lower extremity that to-day's discussion has exclusively referred, and no doubt for the reason that here lie our real difficulties; and it is especially to the hip-joint that reference has been made; and it is to this subject that I am about to address my observations. It has occurred to me to perform excision of the hip in seven cases during the past few years. I have followed all of these cases to their termination, and have the disheartening statement to make, that six are dead, and one alone is really completely successful. This result does not condemn the operation, for upon a closer examination we find that two lived for four years, and others for from one to two; one died from extension of the disease to the shaft of the femur, necessitating amputation. The first case, a child, five years of age, recovered so as to go to school, walking a mile and a half daily, and lived in good health for four years, then, after exposure to cold, became dropsical, and died comatose, from amyloid disease of the kidneys, which had been established before the operation. The specimen of the resected hip in this case is now on view in the Congress Museum, it is numbered 517.* It shows a perfect fibrous union between the acetabulum and the cut femur, and shows no sign of return of the disease. The second case has made a complete recovery, the lad, eleven years old at the time of the operation, is now a clerk in an office. As will be seen by these photographs he possesses a perfect support on the side operated upon, as also complete motion, flexion, and extension being perfect. I think the conclusion which we are compelled to come to regarding the time

* It has been presented to the Royal College of Surgeons in Ireland, where the specimen may be examined.

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when excision of the hip should be performed, is, that it must be, as Mr. Croft has said, relatively early, that is, after the case has passed its early stage, in which a good result may be obtained by rest, extension, and general treatment, and before the degenerative disease of the kidneys and liver has advanced so far as to render the ultimate restoration of the patient to health impossible.

Professor KÜSTER, Berlin: I wish to draw attention to the fact that the question of early as opposed to late excision of tuberculous joints cannot receive a simple answer, as this must vary with the particular joint in question. In children, resection of the knee is rarely called for, still less often are those of the feet or upper extremity; but with them the subject of hip-joint excision is the most important. These cases may be arranged in three groups: the first are advanced ones, in which the joint is completely disorganized, and the operation is undertaken with the endeavour to obtain a more useful limb. The second group are those in which the head of the bone has been partly destroyed, but is covered with a cicatricial layer, which has also occupied the portion of the synovial membrane; but the process has lighted up again at some point or other in the bone or the joint; these cases sometimes, but not always, yield a speedy cure. In the third group are the more acute cases, in which I myself have seldom obtained a rapid cure, or one without remaining sinuses. The deduction may seem to be that early excision should be eschewed in all cases; but if that is true, we should condemn a number of children to death or to prolonged suffering. I may lay down the following indication for operation with an easy conscience, as the results of operation are, as far as function is concerned, at least tolerable, and the sinuses eventually do, as a rule, heal:—viz., that excision of the hip should be performed when the general health begins to give way; but, as long as it is well maintained, the surgeon is likely to obtain a more useful limb by waiting.

Mr. T. BRYANT, London: I have been induced to address the Section by listening to the record of Dr. Sayre's interesting case of restoration of the hip-joint after resection; for I have at the present time, in Guy's Hospital, a case which, I believe, illustrates an equal, if not a greater, amount of repair after operation. It occurred in the person of a little girl, aged six, who was admitted under my care at Guy's, in the autumn of 1880, with an acute periosteal abscess about the right femur, and all the constitutional disturbance which is well known to accompany this affection. It was treated at once by free incisions, the evacuation of the pent-up fluid, and subsequent washing out of the abscess cavity with a lotion of iodine and water. Great benefit followed these measures; but the upper two-thirds of the bone became necrotic, and no attempt was made by Nature to reform the bone, except at the junction of its middle with the lower third, where there was an ensheathing periosteal formation of about an inch in length. In the course of a few months the dead bone was removed, and the head, neck, trochanters, and two and a half inches of the shaft of the femur were taken away; the wound was washed out with iodine water, and dressed with terebene and oil (1 part to 4), and the limb was fixed to my double extension splint. In three weeks the operation wound had healed, and in three months new bone had taken the place of that which had been removed, and in another three months every movement of the joint was restored almost to perfection—that is, the limb could be flexed, adducted, abducted, and rotated with perfect freedom, and the general outline of the hip-joint was almost restored, the only difference being that the

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affected limb was almost one inch shorter than the sound. I will not venture to speculate upon the way in which this large portion of the femur and joint has been restored. That it has been restored in a wonderful way there is no doubt, and I am disposed to think that the good result has been brought about by the effusion of plastic matter from the ensheathing periosteal formation which covered the lower end of the sequestrum—for it was at this spot alone that any periosteal formation was to be detected. With regard to excision of the hip, I give my adhesion to most of what Mr. H. Marsh has advanced, and state my opinion that this operation is only called for in cases in which, in spite of great care and attention, no progress towards recovery is visible, and the general condition of the patient is steadily deteriorating. In most others, I believe, a cure by natural processes can be obtained by means of immobility, the relief of tension by subcutaneous puncture of the joint, as alluded to by Mr. Teale, and by free incisions into suppurating cavities when such exist, with or without the removal of dead bone. I may add that the surgeon ought to place unlimited confidence in Nature's power for the cure of joint disease, when well assisted by art, in the way I have suggested. How far the presence of visceral disease should induce the surgeon to interfere, is a question I would put to the surgeons present to find an answer.

Mr. Christopher Heath, London: I regard excision as required in incurable cases, and those in which lardaceous disease is present, but protest against early excision in cases in which general and local treatment are available. I would recall attention to Professor Redfern's remarks, and point out that repair takes place by replacement of the cartilage by fibrous tissue and bone, and that the museums show how common osseous ankylosis is. Excision in private practice is almost unknown, and is not required, because of the good hygienic surroundings.

Mr. C. Macnamara, London: With reference to the remarks that have been made by Professor Ollier and Mr. Bryant, I would briefly call attention to the circumstances of two cases now in the Westminster Hospital: one of these patients is a lad fourteen years of age; in August, 1880, he was brought into my ward suffering from symptoms of what is usually described as acute periostitis. I found, as is the case in most instances of the kind, that the inflammation had extended from the soft-growing tissues at the junction of the shaft and epiphysis of the bone to the periosteum, and probably also to the medulla. On cutting down upon the head of the bone the epiphysis was separated from the diaphysis. Subsequently—that is, in about six weeks from the commencement of his illness—as the patient was evidently sinking from septo-pyæmia, I removed the shaft of his tibia, leaving the upper and lower epiphyses and the periosteum, which, though thickened, could be easily separated from the diseased shaft of the bone. A new tibia has formed from this periosteum, so that the patient has now a useful leg and a perfect knee and ankle-joint. A section of this boy's tibia is to be seen in the Museum of the Congress, No. 19, and it demonstrates the fact that acute inflammation of the epiphysis is apt to involve not only the periosteum but also to cause osteomyelitis of the bone; it was from the absorption of the putrid matter in the shaft of the tibia that septo-pyæmia was induced, which so nearly killed my patient. In the other case, which also commenced in acute epiphysitis of the upper extremity of the right tibia, the disease had existed for three months before I saw the little patient. There was then a thick case of new bone, formed from the periosteum, and surrounding the dead shaft of the tibia. I cut down on this dead bone and

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found the shaft of the tibia had separated from both the upper and lower epiphysis; it was removed, care being taken to separate it from the surrounding soft tissues. In this way I hoped to have left the periosteum, from which a new bone would have been produced, as in the above case; no new bone, however, at the end of six months had been produced because there was no healthy periosteum left in the leg from which it could be formed. We see from this specimen of the bone, recovered from this patient's leg, that the tibia is surrounded with a case of new bone (periosteal) very different from that of the other specimen. After six months no vestige of osseous tissues having appeared in the patient's leg to replace that which I had taken away, and as the limb was perfectly useless, I determined to endeavour to form a new tibia. I therefore took perfectly fresh, very small pieces of bone and periosteum from the foot of a patient's limb I had amputated, and I placed these pieces of bone and periosteum in a groove made in this little girl's leg in the situation of her tibia. The proceeding was conducted on Listerian principles, and no suppuration occurred. From these little bits of bone and periosteum introduced into this child's leg, new osseous tissue has formed. A narrow ridge of bone can be felt along the course of the osseous tissues which I planted in her leg. We shall, of course, watch with great interest the further progress of this case. The wound has entirely healed, and, as I said before, where no bone whatever existed six weeks ago, now a narrow band of osseous tissue has been produced, and those who have watched the case with me have no doubt that this new bone has formed from that which I had planted in the patient's leg, and which we must expect will take some months to grow to any considerable size. With regard to excisions of the hip, or in fact of any other part of the body, no one questions the ease with which these operations can be performed, and that joints having been removed a fairly useful limb may result. There is no great risk either in operations of this kind, provided the patient be young. From the age of twenty years and upwards, in spite of antiseptics, the resections of large joints are dangerous to life; and I am convinced that among young persons by far the majority of joint diseases may be cured in their early stages without resection. This is true even of those cases which commence in tuberculous inflammation of the bone, and much more so in those instances of disease referred to by Mr. Teale, in which the joint affection commences in synovitis. In this latter class of cases I agree with Mr. Teale in the advisability of relieving the tension of the joint either by the method he advocates, or by means of the aspirator; but however the fluid is removed, I think it is necessary immediately after removing it to encase the joint in cotton wool, and surround the whole with an elastic bandage, so as to control the circulation in the foot, and prevent the reaccumulation of fluid in the articulation. Resection of a joint is a most valuable means of treatment, but in my opinion should never be resorted to until all other means have been persistently tried to cure the disease under which our patient is labouring. Surgery can never define exactly how long we are to continue treatment of this kind, but any surgeon of experience knows perfectly well when, if the patient were the child of a relation or friend of his own, he would be able without hesitation to say that resection of the joint was necessary.

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The Causes of Failure in obtaining Primary Union in Operation Wounds, and on the Methods of Treatment best calculated to secure it.

Mr. William Scovell Savory, F.R.S., London.

I rise with reluctance to speak on this question, for a discussion of this kind, with the part I must take in it, is not at all to my taste. The rôle of Cassandra has no attraction for me, and if I could have persuaded myself that silence to-day was consistent with duty I should have been better pleased to listen and to make no sign. In truth, the subject appears to me to have been beaten out in discussion. Much—perhaps a great deal too much—has been already said and written on it. We have reached, if not passed, the point at which what is called argument not only ceases to convince, but acts by strengthening the convictions to which it is opposed. In other words, with many, the subject is less in their heads than in their hearts. It is indeed large enough, and to us and all concerned, vital enough to fire the heart, but it should be remembered that it still has some claim to a place in the head also. And for myself, I may add that I have already spoken freely and fully on the subject, and although this is now two years ago, I have little to add to, and nothing to retract from, what I then said. I have watched attentively all that I have been able to see, and have studied diligently all that I have been able to learn in the interval, and very recently I have added to this the somewhat disagreeable task of reading the Address I gave at Cork, and I venture still to appeal to tnat as the epitome of my creed on the matter. But the question which is set down here for discussion, and on which I have been invited to speak to-day, is, I think, happily chosen in at least one respect. It places the picture upon which all eyes have been so long turned in a somewhat different light, and so the relation of its several parts to each other is changed. It runs thus: "The causes of failure in obtaining primary union in operation wounds, and on the methods of treatment best calculated to secure it." In what circumstances is a wound after operation—that is to say, an incised wound—most likely to heal by primary union? I should say, when the fresh surfaces are brought together in their natural state, and maintained in that position without disturbance. We know too well that, even in these circumstances, they will not always so unite—that, in truth, primary union very often fails throughout the whole or part of a large wound, but I repeat that the conditions I have mentioned are the most favourable to its occurrence; and he who denies this is, I think, bound to tell us what plan he adopts when he has this object chiefly in view—in the operation for hare-lip, for instance, or in autoplastic or other operations where primary union is essential to success. Wounds may be, and undoubtedly are, treated in other ways, for various reasons; but I recur to the question: if these various methods conduce to primary union, why are they not adopted where primary union is the chief object in view? Well, then, to this question before us I should answer that prominent among the causes of failure in obtaining primary union in operation wounds is meddlesome surgery—applications which change the natural surfaces of wounds, which coagulate albumen, and otherwise affect in various degrees the structure and composition of the tissues at the surface, and thus arrest, or hamper, or pervert the normal process of repair; and that the method of treatment best calculated to secure it is to avoid such obstacles, to be content with the part of Nature's humble ally, and not to assume the character of her schoolmaster. Now, I do not say that primary

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union may not occur in spite of such hindrances. The natural forces at work may, perchance, triumph over these and many other difficulties when they are below a certain degree; but this does not prove that they are not, so far as they go, mischievous. That sometimes very rapid union is effected, notwithstanding them, is unquestionable, though I think it may be a question whether this is primary union in the old sense or to be strictly so called. But I do not push this question further, as I suppose it will hardly be contended that wounds are soaked with solutions of chloride of zinc, or carbolic acid, or something of the like kind, for the pure and simple purpose of promoting primary union. But, of course, there is another question—the question of risk to life by blood poisoning during the repair of wounds; and, then, the further question arises, whether the danger from this cause is always in an inverse ratio to the rate at which wounds heal. I am not prepared to give an unqualified answer in the affirmative, nor need I, for a question of more practical importance stands in the way. In many cases, most if you will, is not the chance of obtaining primary union so remote that it is not prudent to incur the risks which the attempt must involve when success is so improbable? We fall back on the question, then, under what conditions, as a rule, do wounds heal most safely and quickly—for, at least, for practical purposes—safety and rapidity may be considered together? And, in the answer to this, opinions of course widely diverge. I should say that wounds heal most safely and quickly when their surfaces are kept at rest in a natural and healthy state, and that, as a rule, in proportion as these surfaces are irritated or separated, so does the process of repair fail or tend to become circuitous. To this end, when healthy surfaces are left after an operation I object to change them by the application of any form of irritant, and I try to dress them in such a way that they may afterwards be disturbed as little as possible. My belief is that the view I have expressed is borne out by facts. I know very well how difficult it is to obtain facts. They seem to have an especial aversion to being taken and registered. But although I despair, at least for the present, of getting satisfactory statistics on the rate of healing of wounds, I think, on the question of safety, we have some facts to which we may appeal. Whether a patient lives or dies within a certain time after an operation, is not in itself a very complicated question. Whether he dies in consequence of the operation, introduces, I am aware, an element of disturbance; but, on the broad fact of living or dying, one can hardly go wrong. I am not about to trouble you with tables of statistics, or even to quote them, for the statistics to which I refer are, and for some time have been, before the profession, and I am content to leave them to the judgment of intelligent and impartial examiners. I make no reference to the publication of single cases, or typical cases, or selected cases. For me, in the question before us, they are worthless—worse than useless—for they tend to mislead. Such fragments of evidence, like *ex parte* statements produce a false impression. We want the truth, and nothing but the truth, and the whole truth. But in this discussion it has come to pass that statistics in any form have been treated with indifference, on the ground that they cannot be trustworthy. Surely much nonsense has been talked about statistics. At one time they are held to be the only sure test of truth; at another they are declared to be worthless—nay, mischievous—as evidence in questions of surgery. Because the simple fact is overlooked that, as a rule, their value is determined by the number of cases upon which they are based. Thus, in the present instance, an appeal to statistics is objected to on the ground that the cases treated are unequal

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in character, that severe injuries and operations are confounded with slight ones. But I submit that in proportion to the number of cases this objection loses weight, until at length it ceases to be of any force whatever. If I compare ten or twenty or thirty cases treated upon one plan at one hospital with an equal number treated upon a different plan at another, any argument drawn from the result would be practically worthless, unless such cases were carefully selected with a view to equality, and, even then, almost insuperable difficulties would present themselves. But if I compare a thousand, or two thousand, or three thousand cases, treated upon one plan, with a like number treated upon another, then I say any objection founded upon their presumed inequality is invalid, unless it can be shown that, in the long run, the kind of cases coming under treatment at one place or under one surgeon are different from those at another place and under another surgeon. I repeat, then, the value of statistics is in proportion to their extent, and if I quote the results of some thousands of cases of injury and operation treated at St. Bartholomew's Hospital, it will not do to assert that these are, as evidence, unworthy of trust, because the cases may be comparatively slight ones, unless it can be shown that, for some particular reason, we have, as the rule, to deal with more trivial cases at St. Bartholomew's Hospital than have other surgeons elsewhere. I venture then, once more to affirm that the statistics I have placed before the profession are trustworthy, for the purpose for which I appealed to them, and furthermore, that no statistics of equal value, in the number of cases and completeness of the tables, and showing a better result, have up to this time been published. You will be glad enough, I dare say, that I cannot now discuss in detail tables of statistics; and, indeed, I have never any inclination to do so. For my part I am quite content to leave this matter as it now stands. Take, on the one hand, such tables as those of St. Bartholomew's, or of the Kilmarnock Infirmary, which have been published by Dr. McVail; and, on the other, the most favourable tables in illustration of any special plan of treatment. Examine them thoroughly, and then say whether the claims to superiority of that particular plan have been established. If these great principles of rest and cleanliness—the maintenance of what is now called an aseptic state—be enforced, if surgery be sufficiently antiseptic, and not too meddlesome, then, no doubt, there may be much variation in detail, and yet withal the best results. So one does this, another that, and a third neither, and their results are, after all, very nearly equal. If you examine the records of surgery in recent years, the fact that most impresses you is the very sudden and prodigious improvement which has taken place in certain quarters. At a single spring, as it were, they have passed from a frightful mortality to a very fair amount of success, and this because the mischief of filth and foulness from putrefaction has been recognised. Surgical wards, not long ago hotbeds of poison, are now made fairly safe for patients. Need I say to whom the chief glory of this reformation is due? But while no doubt some startling novelty of practice was necessary, or at least greatly advantageous to this end, yet I cannot doubt that the same end might have been reached by an adequate improvement in simple sanitary arrangements. What was needed was recognition of the supreme cause of mischief, and the abolition of it by efficient means. And the proof of this lies in the patent fact that equal, nay, yet greater, success is, and for years has been reached in hospitals where no special or peculiar system of dressing has been adopted, but where cleanliness has been held to be of the first importance. This is a fact which shows that we must look to larger causes than the spray or the drainage-tube, or both of them together, to account for such a change. And hence

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the error which certain statistics have been made to foster; old and new figures from the same place have been held up in proof of the transcendent merits of a particular practice, whereas what they show is the advantage of abolishing foulness. The claims of any particular plan of practice to surpassing excellence can be established only when that particular plan can show results better than the best than can be obtained by any other means. And again I declare that this has not yet been done. In almost any other case it would be amusing to notice the instances and observations which are every day published to prove that a particular plan will accomplish results that cannot be otherwise attained. An operation is performed, and the patient recovers. No other method could have achieved this? Why, such a result, and better ones too, have been reached many times before, and will be many times again, by far simpler methods, although, perhaps, never published, certainly not represented as marvellous. A particular form of ligature is applied to an artery by an operation performed with special precautions. Then dead and pickled tissue is reanimated, and henceforth lives happily in communion with the vessel. Then the cry is, "See here! Show this if you can by other means." Well, the same experiment is tried with other means, and the same result ensues, only then it is not painted in such glowing colours. Then we don't talk of restoration to life. We explain it by changes which have long since been described. A word more and I have done. The term antiseptic soon became involved in confusion, and the confusion still prevails. One can only say now that all good surgery is antiseptic. Would that I could say also, that all antiseptic surgery was good! More recently we have heard much of the septic and aseptic course of wounds. But if some of those who employ these terms so lightly were called on to explain what they mean, what difficulties would arise? When is a wound to be considered septic or aseptic? Is pyrexia a sign? Is pus a sign of septicism, or the presence of bacteria, or any other of the lowest visible forms of life; or are we driven back upon the odour, and must we depend upon the sense of smell? But, then, what relation is there between poison and bad odour? Who will define a septic from an aseptic state? Is it then, after all, so far as wounds are concerned, merely a question of degree? For the present I, at least, am inclined to think so.

On the Primary Union of Wounds.

Mr. Sampson Gamgee, Birmingham.

Mr. President and Gentlemen,—In the necessarily restricted time allowed to each speaker, and before such an assembly of the masters of surgical science and art, I shall not attempt systematic or exhaustive treatment of the subject which the Council have proposed for present discussion.

It is to the essential physiological and therapeutic principles of primary wound healing that, with your permission, I shall address myself.

To take, as a first and very simple illustration, an operation for removal of one of the cystic tumours of the scalp. Enucleation effected, through a clean incision, with the least possible bruising, I dry the interior of the wound by a few light touches with a small pad of absorbent gauze and cotton. No water is applied. The edges of the wound are brought together lightly, accurately, yet without tension, and union is maintained by one or more points of suture and strips of dry lint, or of lint soaked in collodion, compound tincture of benzoin, or

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the styptic colloid of my friend Dr. Benjamin Ward Richardson, One angle of the wound, and by preference the most depending one, is not covered by the styptic, to allow the escape of any effused blood. It is well to secure the most perfect dryness of the wound, by passing a probe lightly into the exposed angle, and squeezing through it, by gentle pressure, any blood that may have collected. A pad of absorbent gauze and cotton and a uniform and lightly compressing bandage complete the dressing, which is not to be disturbed for four or five days. At the expiration of that period the sutures may be removed, and another absorbent gauze and cotton pad applied with equable pressure. If there be any discharge, which will be the exception, a small piece of the pad, from the first dressing, may be used to take it up, but no water applied. The second dressing will, in the majority of such cases, be the last, and may be maintained *in situ* for about a week.

Once the surface and edges of the wound have been brought together, infrequent dressing is most conducive to primary union. The merit of establishing this proposition belongs to Cesare Magati, who shed so much lustre on the University of Ferrara at the commencement of the seventeenth century. The first words in the title of his historical essay, "De rara Medicatione Vulnerum," embody the essence of the principle of infrequent dressings, which, half a century later, found so able an exponent in Belloste. One passage in point, from his "Chirurgien d'Hôpital," so accurately conveys my own opinion that I quote it textually :—"Je ne panse que rarement une plaie, convaincu qu'il faut donner à la nature le loisir d'agir, pour qu'elle puisse rétablir les parties blessées dans leur premier état."

Rest is a first essential in the process of primary union, movement one of its greatest obstacles ; and in direct proportion as the former is favoured and the latter avoided by infrequent dressing, this is to be preferred on scientific and practical grounds.

A few words on dry dressing. Experience has proved that cold water irrigation is consistent with rapid and sound wound healing, in virtue of its repressive and sedative action on the local circulation and innervation, and of the perfect drainage which it secures. But stagnant water, with warmth, favours putrefaction, and is opposed to primary wound healing.

Everyone who has attempted the preservation of meat for domestic purposes, knows the importance of keeping it cool and dry in a current of air, wiping off any moisture that exudes on the surface, or taking it up by sprinkling it with flour or other absorbent material ; moistening and putrefaction, drying and preservation go together.

I lately read a letter from a surgeon to one of Her Majesty's ships, stationed on the coast of Peru. He recalled visits to battle-fields in the Bulgarian swamps, where he had seen dead bodies, exposed in a warm and humid atmosphere, rapidly swell and decompose. But when, after the recent feats of arms in South America, he traversed the arid plains, often going many miles without a sight of water, the cadavers, exposed in the dry, burning air, were dry and shrunken.

Take two pieces of fresh meat of equal thickness and weight. Place one between two pads of absorbent gauze and cotton, and wrap the other in lint soaked in water, with a covering of oiled silk or gutta percha. The first piece, as I have often proved by experiment, will continue dry and sweet, long after the meat in water-dressing has become a rotten and stinking mass.

The predication from the operation of physical causes on dead organic matter

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to their influence on living tissues has been productive of so much fallacious reasoning and mischievous practice, that I wish the illustrations just adduced only to be taken as initial proof, other things being equal, of the effect of moisture and dryness on the decomposition of animal tissues.

In the scalp wound on which we are commenting, the dry state of parts was maintained by a combination of influences. Absolute rest, position, and pressure, regulate the blood supply, reduce nerve irritation to a minimum, and proportionately eliminate the causes and lessen the possible source of extravasation. Whatever fluid was poured out was rapidly taken up by the powerfully absorbing gauze and cotton pad, so that the wound was left clean and dry.

In wounds similarly treated the healing process is attended with a shrinking and consolidation of the tissues, which are seen pale and shrivelled when the compressing dry dressing is removed.

When the absorbent pads are not sufficient to ensure the perfect drainage so necessary to primary union, I do not hesitate to employ a drainage-tube. I take care to bring the outer end of the tube through a hole in an absorbent pad, or in the interval between two pads covering the wound, so as to collect the product of discharge through the tube in an absorbent pad placed lightly over its outlet, without interfering with the deeper dressing. This arrangement provides for perfect drainage and cleanliness with absolute repose, rendering possible the frequent removal of discharges, without violating the essential principle of infrequent dressing of the wound itself.

In the case of head wounds, position is favourable to the balance of the circulation and to the maintenance of rest. Not so in the limbs. Their movements are most effectually controlled and immobility secured by close-fitting moulds, enclosing the joints above and below the seat of injury, and uniformly compressing the surface.

For the construction of an immovable apparatus many materials are available. I use in preference light rough millboard, soaked in a concentrated solution of dextrine. Thus prepared, dry gummed strips can be kept any length of time. When required for use a small quantity of hot water poured over the splint softens it in less than two minutes, and it readily moulds under light pressure, and very quickly becomes dry and strong. The process of drying is much quicker when absorbent pads and bandages are used in preference to the calico rollers and cotton wool. The gummed millboard splints once stiffened in use can be remoistened to fit the limb as it shrinks, or to be used in another case.

The efficacy of an immovable apparatus is greatly enhanced by the use of the swing, which preserves parts at rest, in direct proportion to the mobility of the suspending medium.

In an International Congress like the present it is only just to acknowledge that to our brethren across the Channel belongs almost exclusively the merit of establishing the principles and practice of immobilization and hyponarthesis—*vide* the labours of Belloste and Moscati, of Larrey, Seutin, and Velpeau, of Sauter and Mayor.

It is not only in the after-treatment, but during the performance of operations, that suspension is of great value in facilitating manipulation and clean incisions with a minimum of contusion.

The first time I employed suspension for this purpose was in 1865 in amputation at the hip-joint, for ossifying enchondroma of the femur. The limb was so large, weighing within a trifle of 1 cwt., and nearly two-thirds of the whole weight

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of the whole body, measuring] forty-eight inches in circumference, and barely leaving room for the knife to secure cover after disarticulation, that I could not rely on assistants' hands to support and rotate the limb. I therefore suspended it by a double pulley.

I have so frequently referred to compression as one of the most salutary agencies in the treatment of wounds, and generally in the treatment of injuries, that I beg leave to dwell for a few moments, in illustration of a principle which is very far from having obtained the general assent to which it is entitled, on physiological and practical grounds. Compression is not constriction; this strangles and destroys, that soothes and heals. Constriction is hard, violent, and painful; compression elastic, gradual, and comforting. Nothing more soothing to an injured part, nothing more powerfully conducive to the balance of innervation and circulation, to the prevention of sanguineous and inflammatory effusion, and to its removal when it has taken place, than gentle, equable, elastic compression. Its application is greatly facilitated by bandaging evenly, with a rapid spiral and without reverses, over absorbent gauze and cotton pads, the elasticity of which is practically indestructible.

It is very important to have a clear conception of the considerable effect speedily produced by light compression. This is abundantly proved in experiments and surgical practice for the arrest of hæmorrhage. But the effect of pressure in preventing and treating inflammation is less known. Hang the hand down in a basin of hot water, and when you take it out compress the reddened skin lightly, first with the tip of a finger, then with one of these elastic pads; on removing the pressure abruptly you will soon convince yourself under what slight elastic pressure, and how rapidly, the artificially inflamed skin becomes deathly pale; all the more, of course, if you raise the hand vertically, bend the elbow at an acute angle, and simultaneously compress the inflamed surface.

Striking illustration of the value in wound treatment of rest, position, and pressure, with dry and infrequent dressing, may be seen in cases like one in which I was recently consulted, and the main facts of which I shall succinctly relate.

I found a contused wound stretching across the palmar aspect of a man's forearm. It had been inflicted several days previously, and treated with carbolic lotion and afterwards with poultices. The red grey wound gaped and stank, the edges were tumid, the whole limb red and tense and exquisitely tender. Pulse bounding, constitutional disturbance high. I raised the hand vertically above the head, and compressed the brachial artery for a few minutes, when the limb rapidly became pale and soft. I then brought the elbow to the side and bent it at an acute angle, so as to permanently retard the circulation and rest the palm of the hand on the breast, with the tips of the fingers corresponding to the middle of the clavicle. In this position, having previously surrounded the limb with absorbent pads and fitted to it the prepared millboard splints, I fixed it to the side with a uniformly compressing bandage. When the dressings were removed at the end of four days, the patient meanwhile having been perfectly easy, I found the limb pale, shrunken, and painless, the wound healing by granulation, pus small in quantity and odourless. Recovery under similar dressings was uninterrupted.

I venture to express a confident belief that, with the adoption of similar treatment, under analogous circumstances, we shall see no more of the deplorable ravages of diffused cellulitis or phlegmonous erysipelas, under water dressing, poultices, and incision. Many a hot, swollen, and boggy limb has been sacrificed, and many a life lost from alleged blood poisoning, which would have yielded

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readily and painlessly to digital compression of the main artery, equable elastic surface pressure, and undulatory suspension.

When much of the superficial tissue has been destroyed, a little glycerine poured over the pad, before applying it, has the advantage of preventing too close adhesion. Doubtless glycerine, on which Demarquay wrote so ably, is a powerful antiseptic; and this leads me to comment on a class of agents which are of great value as aids to wound treatment, especially under exceptional pathological circumstances. This point conceded, the more the history of the question is studied, the more closely different plans of treatment are compared, all the more must impartial thinkers come to the conclusion that it is an error to attempt to base a reformed system of surgery on antiseptics, to the exclusion of the great physiological principles, on the co-ordination of which depends constitutional and local repair.

Recall, I beg you, the statement of Alanson, of Liverpool, a century ago, in his practical observations on amputations, one of those forgotten surgical classics which I would venture strongly to impress on the attention of my younger brethren. With correct appreciation of the principle of primary union, and of the best means of attaining it, he performed thirty-five amputations, "such as promiscuously occurred at the Liverpool Infirmary, without the loss of a single patient."

Think of Syme's thirty-five ligatures of the femoral artery, with thirty-four successes. Consider, for a moment, all plastic and subcutaneous operations—how rapid and solid their healing. Look at the operation of lithotomy in children—how rare is a fatal result. How safe are the operations of perineoraphy and fistula *in ano*—how rapidly large wounds of the face heal after removal of the jaws. Given a knee or shoulder-joint laid open with an axe, union by the first intention is the rule, under position, rest, pressure, and dry dressing, infrequently changed. So with compound fractures.

Recall, I beg you, the great surgical successes of Larrey with immobility, infrequent dressing, and *eau de vie camphrée;* refresh your memory with the results obtained by such opposite dressings as dry earth and cold water irrigation, exposure to the air and absolute occlusion.

Remember how many times you have seen a vast sloughy ulcer of the leg heal as if by magic, under compressing plasters and bandage, sometimes after the application of nitric acid, nitrate of silver, or a blister.

Having nothing to add to, or retract from, the principles of treatment illustrated by the cases placed on record from my clinique, I beg you to examine with impartiality the results of surgeons who have practised different methods of wound treatment in various parts of the world. Take, for instance, the statistics of Dr. James R. Wood, in his wards of Bellevue Hospital, New York, as recorded by my friend Dr. Frederick S. Dennis; of Bartholomew's Hospital, under the lamented Callender, Mr. Savory, and their colleagues; of the Aberdeen Infirmary, under Professor Pirrie; of the Edinburgh, under Professor Spence; of Kilmarnock, under Dr. Borland and his able coadjutor, Dr. McVail. Having done so, I think you will agree that the germ theory, micrococci, and the spray are a very unstable basis for a revolution in surgical science and practice.

The question is not one of antiseptics or no antiseptics, but of the form and proportion in which they shall be employed in particular conditions. It does not require very deep historical research to prove that those agents have been used by surgeons for generations. Lesne tells us, in the "Discours Préliminaire," with

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which he prefaces his edition of the works of Jean Louis Petit, "Il employait les antiseptiques les plus puissants; il savait qu'on n'arrête pas un mouvement de pourriture avec de l'eau tiède, ou un cataplasme emollient." Louis, of the old Academy of Surgery, recommends balsamic and anti-putrescent substances in treating open abscesses. In 1861, Déclat treated a sloughing sore, on the basis of Pasteur's germ theory, by brushing it over with one part of carbolic acid to ten parts of common oil; and shortly afterwards Maisonneuve extensively used weak carbolic lotions. To Professor Lister's labours in the same direction I need only refer, for they are matters of world-wide renown. Unquestionably, many wound dressings have antiseptic properties, which are of special advantage in exceptional cases. So soon as I ascertained by experiment the value of drainage and compression of absorbent gauze and cotton pads, I asked my friend and colleague, Professor Bostock Hill, to try if they could be medicated with a variety of styptic and antiseptic agents. His answer being affirmative and satisfactory, I placed the matter for practical execution in the hands of Messrs. Southall, Brothers, and Barclay, pharmaceutical chemists, of Birmingham. Here are two absorbent gauze and cotton pads, which they have treated with carbolic and benzoic acid. Twelve days ago I placed one ounce of fresh lean meat between those pads, and have in the interim frequently exposed it to the air by lifting off the top pad for fifteen seconds at a time. The meat is dry, clean, and sweet. Portions of the same meat, of equal freshness and weight, similarly treated with pads of pure gauze and cotton, and with other pads treated with glycerine, borax, and oil of tar, stank several days ago. Here is a piece of meat which was placed at the same time between thymol pads. It resisted longer than that in the pads last mentioned, but it also stinks now, and leaves the carbolic and benzoic pads masters of the situation as anti-putrescents.

With such pads, and with absorbent bandages similarly prepared to take up and disinfect discharges, the principles of dry and infrequent dressing, rest, position, and pressure will admit of very extensive and safe application, even, I trust, in the estimation of those surgeons who are most wedded to antiseptic preparations.

After all, the great antiseptic is life. Rely on the preservative power of living tissues; be physiologists always while you are surgeons, and you will have fewer and fewer opportunities of studying pathological states after surgical injuries and wounds.

When I reflect that such surgeons as Professors Esmarch and Verneuil, Volkmann and Humphry, Trélat and Lund are announced to take part in this debate, which Professor Lister is to close, I am relieved of all anxiety for the imperfections of this sketch, which will be so abundantly compensated for, that we cannot fail to leave this hall better informed, on one of the oldest and yet newest, simplest yet most complex, of surgical questions.

La réunion immédiate, comme toute opération chirurgicale, reconnaît diverses causes qu'on peut ranger sous deux chefs: Exécution défectueuse, application intempestive.

Professor VERNEUIL, Paris.

J'ignore si l'exécution défectueuse est fréquente, mais je sais qu'il est moins aisé que l'on le croit de réunir irréproachablement certaines plaies—que les sutures auto-

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plastiques sont fort délicates et que la moindre négligence peut faire manquer l'adhésion. Toutefois il faudrait ne pas abuser de cet aveu, ne pas attribuer à la difficulté ou à l'impéritie les insuccès dûs à d'autres causes, et surtout ne pas accuser comme on le fait volontiers, de mal pratiquer la réunion les chirurgiens (ceux de Paris, par exemple) qui n'ont tiré de cette opération que de médiocres résultats.

L'application intempestive est malheureusement commune. Envisagée comme moyen thérapeutique, la réunion immédiate, en regard de ses avantages et des indications qui en découlent, présente des inconvénients, des dangers, et par conséquent des contreindications qu'il est imprudent de méconnaître. Les classiques sans doute ont signalé les conditions défavorables, les chances mauvaises, les cas où il faut s'abstenir. Quelques praticiens écoutant les sages préceptes, emploient la réunion quand elle est nécessaire, et que rien ne menace sérieusement sa réussite. Mais d'autres, moins timorés, plus convaincus, recherchent l'adhésion primitive partout et toujours, pourvu qu'elle ne soit pas matériellement impossible. Entre leurs mains, la réunion immédiate échoue souvent et devient un moyen périlleux, quelques succès invraisemblables sont exaltés outre mesure, mais les résultats incomplets et les revers sont beaucoup trop discrètement indiqués. Cependant, comme le vrai finit par se faire jour, la réunion, après les amputations entre autres, reconnue infidèle et funeste par beaucoup de praticiens et dans bien des pays, allait être abandonnée presque généralement dans les grands hôpitaux et dans les grandes villes, quand la méthode antiseptique est venue fort à propos à son secours, et lui a rendu la vogue et la popularité. Depuis lors, les contreindications sont oubliées de nouveau, les inconvénients contestés, les périls regardés comme imaginaires, de sorte que tout en tenant grand compte de la phase nouvelle dans laquelle est entrée la question, il faut recommencer la campagne et défendre la réunion immédiate, fort utile et fort recommandable en soi, contre un regain d'enthousiasme exagéré. Je sais que je marche contre le courant en touchant à cette sorte d'arche sainte, et cela dans le pays où elle est le plus en faveur, mais je suis sûr au moins que mes arguments ne seront nulle part plus qu'ici jugés avec compétence et impartialité.

Je dis qu'on applique intempestivement la réunion immédiate lorsque dans un cas donné on méconnaît les causes qui doivent la faire manquer, ou que, reconnaissant les causes, on passe outre. J'ajoute que les succès obtenus alors sont regrettables, et ne justifient nullement la violation des règles.

L'insuccès de l'adhésion primitive a pour origines :

1°.—Les mauvaises conditions locales de la plaie ; contusion, mortification, inflammation altérations diverses des bords, contamination du foyer par des corps étrangers ou des agents septiques, insuffisance des parties molles, &c.

2°.—L'infection du milieu ambiant par les endémies chirurgicales vulgaires ; érysipèle, pourriture d'hôpital, pyohémie, &c.

3°.—Les maladies générales du patient survenant avant, pendant ou après l'opération.

Force de me restreindre, je ne présenterai sur les deux premiers ordres de causes que de courtes remarques.

Bien que les chirurgiens de profession reconnaissent tous les contreindications tirées des conditions locales mauvaises, on a tellement célébré la bénignité, l'innocuité et surtout l'excellence de la réunion immédiate, que les praticiens ordinaires, qui précisément voient tout d'abord les blessés et leur donnent les premiers soins, réunissent souvent à tort et à travers. Ils suturent sans hésiter

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les plaies contuses de la tête, des faces dorsales de la main et du pied, de la partie antérieure du genou ou postérieure du coude, et renvoient ainsi les blessés. Deux ou trois jours après, ceux-ci arrivent dans les hôpitaux avec des phlegmons, des lymphangites, des érysipèles, du malaise et de la fièvre, quand ils eussent pu si facilement guérir avec le simple repos et quelque compresses imbibées d'eau fraîche. Si, encore une fois, les vrais chirurgiens ne commettent pas ces fautes, ils en portent néanmoins et jusqu'à un certain point la responsabilité en ne protestant pas assez énergiquement contre un abus déplorable, en ne répétant pas assez haut que dans les cas douteux le mieux est de s'abstenir, en ne proclamant pas enfin que pour saisir les véritables indications de la réunion immédiate il faut avoir à la fois de l'expérience et de l'instruction.

L'influence délétère des milieux contaminés sur l'adhésion primitive n'est point douteuse. On se rappelle le temps où la réunion échouait si fréquemment dans les hôpitaux encombrés, dans les salles où régnaient l'érysipèle et la pyohémie. De nos jours la méthode antiseptique a si profondément modifié le milieu nosocomial, que la contreindication tirée des mauvaises qualités de ce dernier n'existe plus qu'à titre d'exception. Si pourtant, de nos jours on se trouvait par aventure privé du secours de l'antisepsie, il faudrait se souvenir que la réunion incomplète telle qu'on la pratique après les grandes opérations n'est pas le meilleur préservatif contre les intoxications venues du dehors.

J'arrive au point que je désire développer. Je veux démontrer que la santé générale du patient exerce une action manifeste sur l'issue de la réunion immédiate, que certains actes morbides font manquer communément, et par là constituent des contreindications formelles à son emploi.

Loin de contester des assertions aussi faciles à contrôler au lit du malade, on devrait m'accuser plutôt d'énoncer pompeusement une banalité. Je répondrai en invoquant simplement ce qui se passe tous les jours sous nos yeux après les grandes amputations, entre autres dans la pratique des partisans résolus de la réunion.

Que le sujet soit adulte, ou très-jeune, ou très-vieux, qu'il soit scrofuleux, tuberculeux, diabétique ou alcoolique, que le thermomètre soit à 36° C. ou à 40° C., que la lésion qui nécessite le sacrifice du membre soit pathologique ou traumatique, ancienne ou récente, quel que soit en un mot l'état organique de l'opéré, la plaie sera réunie dans tous les cas, sans distinction, et sans que jamais l'état général influe sur la détermination.

Cette uniformité dans la conduite est d'autant plus singulière, que les mêmes chirurgiens se comporteraient différemment s'il s'agissait d'une autre application de la réunion primitive, de celle qui par exemple intervient dans les autoplasties. Certes, nul d'entre eux ne pratiquerait la périnéoraphie pendant la fièvre puerpérale, l'uréthroplastie au voisinage de plaques muqueuses récentes, la rhinoplastie sur un ivrogne invétéré, ni, pour tout dire, une restauration organique quelconque chez un cachectique ou un fébricitant.

Pourquoi donc cette contradiction ? En vertu de quoi s'abstient-on dans certains cas et agit-on toujours dans les autres ? Voici sans doute ce que l'on pourrait répondre.

Au point de vue du but à poursuivre et du résultat à obtenir il y a bien évidemment deux réunions immédiates distinctes : l'une obligatoire, fondamentale, constituant à elle seule toute l'entreprise chirurgicale. Sa réussite est nécessaire, son échec est lamentable, car si même tout n'est pas perdu, on n'a rien gagné, et tout est à refaire. C'est la réunion anaplastique, et celle qu'on adjoint à l'ouverture du ventre, à l'extraction de la cataracte, &c.

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L'autre est purement facultative, simplement adjuvante. On l'associe à l'opération principale pour en hâter la guérison et en diminuer la gravité. Sa réussite offre des avantages considérables et son échec, fût-il total, n'empêche pas la nature d'effecteur encore la guérison par un autre mécanisme. Telle est la réunion après l'exérèse des membres ou des tumeurs.

Cette distinction faite, la prétendue contradiction disparaît ou se justifie. D'un côté, on multiplie les précautions et on se montre réservé pour éviter à tout prix l'insuccès de la réunion obligatoire ; de l'autre on tente le plus qu'on peut la réunion facultative qui promet beaucoup sans compromettre le résultat final. Un même moyen remplit des indications différentes ; par la réunion anaplastique, on cherche à rétablir la forme et les fonctions ; par la réunion adjuvante, on compte sauver plus vite et plur sûrement l'opéré.

Il est donc naturel qu'on n'adopte pas une règle uniforme pour parvenir à des fins si dissemblables.

Le raisonnement serait acceptable s'il ne laissait entièrement de côté un élément, inséparable de toute opération chirurgicale, je veux parler du danger intrinsèque. En cas de réunion anaplastique, on ne se préoccupe guère que de l'échec de la suture et l'on ne parle pas du péril encouru. Quant à la réunion adjuvante, on lui a si bénévolement attribué le pouvoir de diminuer la gravité de l'opération principale, qu'on ne l'accuse jamais directement quand les accidents viennent à surgir.

Qu'une amputation ou une ablation de tumeur se termine fatalement, à coup sûr on n'avouera jamais que l'opéré est mort de réunion immédiate, ce qui pourtant est parfois rigoureusement exact. Il faut réparer une aussi grave omission, dût-on formuler à ce propos quelques vérités un peu dures.

La réunion anaplastique, acte unique et indépendant, est dangereuse par elle-même, comme le prouve le martyrologe relativement assez chargé de la chirurgie réparatrice. Mais le danger, qu'on peut d'ailleurs réduire au minimum, doit être non seulement prévu, mais encore accepté, à moins qu'on ne renonce à l'opération plastique.

La réunion adjuvante est également périlleuse au même titre et par les mêmes motifs—seulement on n'est pas forcé d'y avoir recours, et quand on l'associe à une autre opération, il faut bien savoir qu'on additionne deux ordres de dangers—que si, par exemple, les risques de l'amputation égalent 20, avec la réunion immédiate ils atteindront 25.

On protestera vivement, je le sais, contre cette asssertion. Si l'on accepte les chiffres précédents comme représentant les risques de chaque opération, amputation et réunion, prises isolément, on évaluera à 15, je suppose, le dégrèvement des risques de la première par le seul fait que la seconde lui est ajoutée. Je traduis tout ceci en chiffres :—

Risques de l'amputation	20
Risques de la réunion	5
Diminution des risques de l'amputation par le fait de la réunion	15

D'où en fin de compte 20+5 fait 15=10 risques. Résultat excellent, et qui impose au chirurgien le devoir impérieux d'associer les deux actes opératoires.

L'argumentation est spécieuse. Elle serait sans réplique si on n'en découvrait pas le côté faible.

Or, celui-ci réside dans la confusion qu'on s'opiniâtre à faire entre deux procédés

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de réunion qui n'ont de commun que le nom, et qui sous tous les autres rapports diffèrent de la façon la plus radicale.

Dans le premier, réunion totale, le foyer soigneusement débarrassé de tout corps étranger, est hermétiquement clos et soustrait à toutes les influence extérieures.

Métamorphosé en plaie souscutanée, il en présente la bénignité avérée et guérit de même sans suppuration. Cette réunion-là, j'en conviens, fait disparaître presque complètement les dangers de la plaie exposée à laquelle on l'applique. Mais par malheur elle ne convient qu'aux traumas peu étendus, car dans les grandes opérations sanglantes elle est tellement menacée d'insuccès que nul ne la recommande plus.

Le second procédé est cette réunion partielle, incomplète, bâtarde, qui diminue l'étendue du foyer, mais y laisse des corps étrangers : drains et ligatures, puis des conduits étroits, permettant l'entrée libre de l'air et des agents qu'il peut charrier, qui enfin met la blessure dans les conditions des plaies exposées, anfractueuses, et vouées inévitablement à la suppuration. Or cette réunion, à chaque instant mise en usage, peut offrir quelques avantages sans doute, mais ne saurait avoir la prétention de rendre bénignes des blessures qu'elle dispose tout au contraire à la stagnation et à l'altération du pus, à l'inflammation et à toutes ses conséquences. D'où cette conclusion, à laquelle on arrive forcément, que le seul procédé de réunion capable de dégrever le pronostic, est justement celui qu'on ne peut employer que par exception, et que l'autre procédé, qu'on applique tous les jours dans l'espoir d'accroître les chances de guérison, entraîne précisément à sa suite beaucoup d'inconvénients et de dangers. D'où ce conseil aux partisans de la réunion immédiate, de faire à l'avenir la distinction théorique et pratique entre les deux procédés, et de s'efforcer de rendre le premier d'un emploi plus fréquent et plus sûr.

Pour qu'elle ne soit point nuisible, il faut encore que la réunion réussisse. On se trompe en considérant l'insuccès comme un fait indifférent n'ayant d'autre conséquence que de retarder la guérison et de faire recommencer une opération réparatrice. L'échec par lui-même est compromettant pour la vie, et voici comment. L'acte de la réunion totale ou partielle met le foyer de la plaie dans des conditions telles, que s'il renferme des germes infectieux ou devient le siège d'une inflammation, l'intoxication ou la phlegmasie atteigne rapidement une grande intensité. On cherche à arrêter le mal en détruisant l'adhésion et en ouvrant largement la plaie. Cela réussit quelquefois, mais échoue plus souvent encore, et dans tous les cas laisse la blessure dans un fort mauvais état local. On devrait avouer qu'en employant la réunion immédiate facultative on se livre à une véritable partie de hazard. L'enjeu d'un côté est la guérison prompte, de l'autre une série d'accidents qui peuvent aller jusqu' à la mort, inclusivement, et à ce jeu les quatre-vingt-dix-huit coups que je pourrais gagner ne me consoleraient pas des deux que je serais exposé à perdre.

On ne saurait trop le répéter : la réunion immédiate n'a, comme avantages qui lui soient propres, que le rétablissement de la forme et l'accélération de la cure. On ne saurait exiger d'elle rien autre chose, et quiconque lui demande plus, risque d'être déçu.

À la vérité, l'espoir des guérisons rapides séduit et attire certains esprits qu'alarme un peu trop peut-être la perspective des lentes cicatrisations. D'autres, en revanche, préférant les voies plus sûres, fussent-elles les plus longues, envisagent sans effroi les réunions secondaires, qui bien conduites sont d'une extrême bénignité. Il y a lieu de tenir ici compte du tempérament de chaque praticien.

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Mais, dira-t-on, ce réquisitoire suranné ne s'applique plus au temps présent; la méthode antiseptique en purifiant le foyer traumatique et le milieu ambiant a fait disparaître presque toutes les causes d'échec de la réunion immédiate et rendu à celle-ci tous ses avantages. Je l'accorde si bien, qu'adversaire déclaré de la réunion facultative il y a dix ans, je l'emploi aujourd'hui dans maintes circonstances. Mais, si je fais les honneurs de la réunion à l'antisepsie (qui, soit dit en passant, sert tout aussi bien la cause des pansements ouverts) je ne suis pas le partisan assez aveugle de cette admirable pratique pour y voir une panacée et pour ne pas reconnaître de limites à sa puissance.

L'antisepsie peut conjurer sans doute plusieurs influences nuisibles, mais elle ne saurait créer certaines conditions favorables à l'adhésion immédiate, savoir : la bonne qualité du sang, la prolifération suffisante des éléments anatomiques, en un mot, la régularité et la persistance du processus réparateur. Que par suite d'un état morbide de l'opéré ces nécessités fassent défaut, la réunion manque, lors même que l'état de la blessure et la pureté du milieu ne laissent rien à désirer ; et alors on voit apparaître tout à la fois les accidents locaux et les symptômes généraux, d'autant plus redoutables que l'organisme malade est moins capable de réagir énergiquement contre leur invasion.

L'observation clinique confirme ces prévisions. Qui n'a vu la suppuration s'emparer des plaies suturées chez les scrofuleux et les phthisiques ? Quoi de plus douteux que la réunion d'une plaie d'amputation chez un alcoolique, un hépatique, un albuminurique ? Combien de fois cette réunion réussit-elle dans les amputations traumatiques quand la fièvre est intense et que le thermomètre est à 39° ou à 40° ? Enfin, quoi de plus ordinaire que la destruction d'une adhésion commencée en cas de maladie fébrile intercurrente ? C'est dans ces circonstances qu'on observe le phlegmon, l'érysipèle, la lymphangite, l'ostéomyélite, la pyohémie même, dans des hôpitaux et dans des salles que la méthode antiseptique a pourtant rendus très-salubres pour les blessés exempts de maladies générales.

D'après mon expérience, si au lieu de réunir les plaies des sujets diathésiques on les traite par les pansements ouverts, les résultats sont infiniment supérieurs, les accidents moins graves, la guérison plus assurée. Je puis citer entre autres les plaies des diabétiques, qui, pansées à plat et antiseptiquement, guérissent assez bien, et qui, réunies par la suture, provoquent souvent l'explosion soudaine et fatalement envahissante d'inflammations gangréneuses promptement mortelles.

Les autres états constitutionnels n'empêchent pas au même degré la réunion immédiate, mais ils en rendent néanmoins la réussite assez douteuse pour qu'il n'y ait guère lieu de la rechercher dans le problématique espoir de guérir quelques jours plus vite.

Signaler les causes d'insuccès d'un moyen thérapeutique est bien ; indiquer les moyens d'en triompher vaut mieux encore. Or, les ressources dont nous disposons pour combattre les influences fâcheuses dérivant d'une maladie générale sont assez limitées d'une part, et de l'autre parfois inapplicables. L'indication évidente est de combattre l'état général et de rétablir les conditions nécessaires au succès de l'adhésion primitive. Nous n'y manquons point dans les anaplasties quand rien ne nous presse et que nous pouvons choisir le moment opportun. Ainsi nous soumettons à une thérapeutique et à une hygiène appropriées le syphilitique et le scrofuleux. Nous attendons que la phase puerpérale soit loin pour recoudre le périnée ou la fistule vésico-vaginale. Nous prescrivons le sulfate de quinine, l'arsenic, l'hydrothérapie au paludique, que pour plus de

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sûreté, en certains cas, nous faisons encore changer de contrée. Nous exigeons de l'alcoolique un stage de sobriété plus ou moins prolongé, &c.

Malheureusement cet entraînement préliminaire n'est pas toujours possible, parceque son agent principal, le temps, nous fait souvent défaut. Dans les cas d'urgence, il n'y faut pas penser. Quand un ivrogne entre à l'hôpital avec le ventre ouvert ou quelque lésion traumatique qui nécessite le sacrifice immédiat d'un membre, il faut prendre la trousse et non prêcher la tempérance. Dans les cas chroniques, eux mêmes, un moment arrive où la temporisation n'est plus de mise, et où on n'a plus rien à attendre d'une thérapeutique instituée depuis longtemps en vain. Ici encore il faut agir sur un mauvais terrain.

Dans ces circonstances, où l'on se trouve en face de chances d'insuccès qu'on n'a ni le loisir de combattre ni le pouvoir de supprimer, la conduite à suivre est heureusement fort simple.

Puisque la réunion immédiate est facultative, puisque rien n'oblige à y avoir recours, puisqu'elle promet plus d'inconvénients que d'avantages, il faut simplement la considérer comme contreindiquée et prendre le parti de s'en passer.

Conclusions: 1°. La réunion immédiate est tantôt fondamentale, obligatoire; tantôt accessoire, facultative. 2°. Dans les deux cas elle présente des indications trés-différentes, une utilité trés-inégale, mais toujours les mêmes chances d'insuccès et les mêmes dangers. 3°. Il faut accepter ces chances et ces dangers, en cherchant sans doute à les atténuer autant que possible, dans les cas où la réunion constitue toute l'opération, comme dans les anaplasties. 4°. Mais avant d'associer à une opération quelconque la réunion simplement adjuvante et facultative, il faut chercher si le blessé n'est pas atteint de quelque état constitutionnel qui ferait rejeter ou ajourner chez lui une réunion anaplastique. 5°. Si cet état est reconnu, il faut le combattre par les moyens appropriés, mais s'il y a urgence ou si la thérapeutique ne peut plus rien, il faut s'abstenir, ne pas faire une tentative de réunion probablement inutile et dangereuse, et employer un autre pansement qui à défaut de guérison rapide offrira du moin plus de sécurité et d'innocuité.

Je comprendrais qu'on multipliât les efforts pour adapter cette réunion aux cas même les plus défavorables, si comme autrefois on n'avait que deux manières de traiter les blessures et s'il fallait absolument qu'une plaie fut ouverte ou fermée à la façon ancienne. Mais aujourd'hui les procédés abondent, et la méthode antiseptique, en dehors même du pansement de Lister, met à notre disposition des procédés si nombreux et si efficaces: pansement de Guérin, bain antiseptique, pulvérisation permanente, &c., que nous n'avons que l'embarras du choix pour traiter sinon aussi vite, au moins aussi sûrement, les grandes plaies opératoires. A la condition de n'être ni exclusifs, ni impatients, ni pressés, et de pratiquer un éclectisme prudent et raisonné, les chirurgiens en vérité ne sauraient plus aujourd'hui être pris au dépourvu.

DISCUSSION.

Professor HUMPHRY, Cambridge: The treatment of wounds is undoubtedly not merely the first stone but also the corner stone of surgery. By it surgery has attained its greatest triumphs; by it our branch of the profession has conferred its greatest benefits on mankind; by it each individual surgeon may hope to do more good than in any other way. Nevertheless, it has ever been one of the opprobria of surgery; and you have well said, Mr. President, that though it was the first work in which surgeons were engaged, it is, at the present day, one of the chief questions

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of surgery, and I trust it will remain so till it has attained to perfection. Under favourable circumstances, the healing of a wound is so gently and silently conducted that its method escapes our observation and baffles our research; and we are neither required nor able to aid it. It takes place well in proportion as there is little disturbance of the circulatory, the vaso-motor, and the nutritive processes; our chief effort accordingly is to prevent or lessen such disturbance. The wound itself is liable to be a disturbance of these processes. In the case of a clean wound made by a sharp instrument healing goes on well; but in a roughly cut, or lacerated, or contused wound, the vitality of the tissues is lowered and healing is interfered with. Again, in well nourished tissues, where the resisting power to disturbance is greatest, as in vigorous persons, and near the middle of the body, it commonly goes on well; whereas, in very early life, when there is much irritability of tissue, or in old age and in atonic states, when the resistance to disturbance is lowered, as also in parts distant from the centre of the body, and especially when the circulation in these parts is hindered by degeneration of arteries, healing takes place with difficulty. There are, therefore, in each case these two factors:—(1) the nature of the wound as an exciter of disturbance; and (2) the power of the tissues to resist disturbance. The surfaces should be placed and maintained in accurate apposition, not necessarily the corresponding surfaces, though it is best that this should be. Thus skin unites best with skin, less well with fat which may interfere between the edges of the skin. The apposition of skin will be best maintained by sutures so applied as to keep the edges in contact without strangulating or injuring the line of skin included within their grip—thus applied they may be used freely, and will do no harm. All the usual appliances for quiet, &c., should be employed. The great difficulty we have to contend with is the collection of blood and bloody fluid in the wound after it has been closed. This fluid prevents apposition and union. Further, losing its vitality, it may be a foreign and irritating substance in the wound. This is likely to occur in proportion as the vitality of the surrounding tissues is lowered, their power of maintaining the vitality of the effused fluid being also lowered. We have therefore now the three factors: (1) the nature of the wound; (2) the resisting and protective power of the tissues; (3) the fluid in the wound and its liability to death. This last factor may be almost entirely prevented through the careful securing of the bleeding vessels by the clamp, by twisting, by cautery, and, best of all, by ligature. If the vessels be sufficiently and properly ligatured after-bleeding will very rarely occur; and forasmuch as the ligatures now used are composed of material which undergoes absorption, and as the tissues included in the ligatures and strangulated by them do not, as Simpson supposed, become decomposing festering sources of irritation, but undergo quiet dissolution and removal, we need have no hesitation in the free use of ligatures. Next, the wound should be freely sponged, so as thoroughly to clear away all blood clots, and so as to elicit bleeding at the time from those small vessels which may not require ligature, as well as from those which do require it, this being likely to prevent bleeding afterwards; and I have thought that Esmarch's band, which is often followed by free bleeding soon after its removal, tends to prevent recurrent bleeding. Finally, before the last stitch is inserted the wound should be carefully pressed so as to squeeze out any fluid that may have collected; and this should be continued so long as any fluid continues to collect. This will be best and most easily done if a drainage-tube be inserted. Such tube may be left and will allow the exit of any fluid that may after all be effused. I think, however, that

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if the precautions be taken which I have mentioned—careful ligaturing, careful sponging, careful and prolonged squeezing of the wound—such fluid will seldom be effused after the wound is closed, particularly if slight well applied general pressure upon the wound by some soft material is maintained. An additional, or fourth, factor is the presence of air. The deleterious influence of air cannot be doubted when we bear in mind the great difference between a simple and a compound fracture, which difference is entirely dependent upon the presence of air in the wound. The deleterious influences of air are due to two causes. First, what may be called the mechanical effects of the air in separating the particles of the effused fluid and separating them from the surrounding tissues, and so lowering their vitality, just as when blood, flowing from the body, is freely exposed to air it loses its power of coagulation and its other vital properties. Secondly, the deleterious influences of air are further due to certain organic agents, living or not, commonly present in it, and called septic agents, which have the property of promoting decomposition in organic fluids. They seem to do this in accordance with some great general law of the communicability of properties upon which assimilation, nutrition, growth, repair, and pathological processes in great measure depend. It is now generally admitted that upon their presence, rather than upon any other atmospheric agency, decomposition depends. The power of this fifth factor will be to some extent proportionate in any given case, like that of the mechanical effect of the air, upon its quantity and upon the resisting power of the tissues and fluids to its influence. Air may be admitted, and septic matter with it, and no ill result may follow, when what we call the "vital property" of the surrounding tissues is able to resist their agency. Nevertheless, knowing the serious results that are liable to be produced by septic matter, not only in the wound, but also by its passage into the circulatory system, it is an imperative duty to exclude such agents as far as possible; and, further, to prevent their influence if admitted. Every precaution, therefore, should be taken to ensure purity of air; and under air I include sponges, hands, water, and all substances brought into contact with the wound. Thus septic influences may be largely prevented. Still, there will be a residuum in certain cases. We cannot be always quite sure of the elimination of septic agents, and we, therefore, should not decline to have recourse to those substances which possess a recognised power of destroying those agents, or of preventing their evil influence upon the fluids and tissues with which they are brought into contact. That certain substances, as alcohol, carbolic acid, &c., possess this power there can be no doubt in the minds of those who have made experiments of a very simple nature with them; and their introduction into the practice of surgery must be regarded as a decided gain, provided they be employed not as substitutes, but as accessories to other not less important methods, or rather precautions, which I have mentioned. The antiseptic system is to be used in addition to, not as a substitute for, preventive precautionary attention. I suppose no surgeon would willingly cut into a cavity in an impure atmosphere, if circumstances required him to do so, without using such antiseptic agents as he could avail himself of. If this is so, it grants the antiseptic position; and in foreign hospitals, where the need of antiseptic agents may be greater than in the British hospitals, their advantages have been almost universally recognized. That some evils are incidental to the use of an irritating substance like carbolic acid is admitted. In each case the pros and cons for its use must be considered; and we may hope that ere long some agent will be discovered which possesses the antiseptic, without presenting the irritating, qualities of

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that substance. I believed, and still believe, that I made some advance in the treatment of wounds, when, discarding all dressings, wet and dry, I simply approximated the edges by suture, and left the wound exposed to the air. This treatment was certainly an improvement on preceding methods, and was found to be so by others, as well as by myself. Still I was not satisfied. I felt there was something better yet to come, some means of more effectually preventing the collection of effused fluid in the wound, and of preventing the decomposition of such fluid when it does collect. The modern ligatures, the careful sponging and squeezing to which I have referred, go far to attain the former desideratum, and aseptic precautions and antiseptic methods will, I hope, attain the latter.

Professor R. Volkmann, Halle: I should like, in a very few words, to indicate the position which I take in regard to this question, in common, I believe, with all those German surgeons who have fully accepted the antiseptic principle. We know of no suppuration which does not depend on the influence of organic ferments, and we recognize no other cause for suppuration. If we then confine ourselves to the consideration of healing by first intention and suppuration, as has been hitherto done in this discussion, the question is so far answered—viz., that constitutional conditions and individual definitions do not exercise the influence which has been attributed to them, upon the occurrence or non-occurrence of suppuration. In the worst constitutions, and with the most disordered state of health, no suppuration takes place if septic infection is prevented. The expression, "aseptic suppuration," is undoubtedly often employed, but this is nothing more than a conventional indication of a form of suppuration which is so slightly septic, from the action of the bacteria being so weak, or the tissues so unsuitable for their development, that no danger is caused to the individual; no clinical indications of inflammation are present and no signs of decomposition are found in the secretions from the wound. But if we could completely exclude all organic ferments, the largest open wounds and the most extensive loss of tissue would heal without suppuration, either beneath a scab or a blood clot, as in the case of a subcutaneous wound. The only constitutional state, in the meaning applied to the term by previous speakers, which can prevent healing by the first intention, by causing active local inflammation and suppuration, is a general septic infection of the blood. By this means putrefactive organisms may find their way to the wound. Still, this manner of infection is much less common than is usually supposed, though its occasional occurrence cannot be disputed; it usually happens in the case of patients already suffering from a general septic infection, in whom a fresh wound is made through tissues which already contain septic organisms, such, for example, as those of a recent amputation wound. In scrofulous and tuberculous patients, wounds, if treated rigidly antiseptically, heal quite as easily by first intention as they do in healthy people. No doubt we see at times, after resection of joints, or extirpation of glands in such patients, healing interfered with, first intention not attained, or perhaps wounds that had almost or completely healed break out again, or assume the fungating (scrofulous or tuberculous) character; but this simply depends upon the fact that the whole of the diseased tissues were not removed; the wound becomes reinfected from septic or tuberculous granulation material left behind. I differ from those who describe this as the result of a depraved constitutional condition or a scrofulous diathesis; for if, in the case of a tuberculous joint, amputation through sound parts is performed, we do not meet with the unfavourable results which so frequently follow excisions. On the contrary, even amputations of the thigh, in people with well-marked tuberculosis of the lungs

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heal well by first intention if treated antiseptically. And, again, resections of joints in scrofulous children unite by first intention, if the operation be performed before sinuses have formed, if the surgeon succeed in completely removing all the fungous material in the neighbourhood of the abscesses and carry his incisions well into healthy tissues, leaving no diseased parts behind. In the last eight years I have obtained very frequently union by first intention out of nearly 200 resections of the hip; and my results have become better from year to year, because I have been more and more careful to remove all the diseased material. At first I only secured primary union in twenty per cent. of the cases, but of late years the percentage has reached fifty, which certainly could not be the case if constitutional tendencies had anything to do with preventing it. In the case of syphilis, healing by first intention can be obtained, even when gummatous (of course not suppurating) tissue has to be left behind, as must often happen in the removal of gummata from muscles, the scraping out of syphilitic testicles, &c., the syphilitic tissues healing without any difficulty; and it has much less frequently occurred to me to meet with any departure from the normal process of healing, when I have been obliged to perform any great operation, such as an amputation for an accidental injury in a patient affected with syphilis. Professor Esmarch will certainly agree that the brilliant results he has described as obtained by the use of his permanent antiseptic dressings are not interfered with by unfavourable constitutional conditions. It is a much more delicate question to decide in how far these constitutional conditions may render the parts operated upon more liable to the influence of septic organisms, and so render the carrying out of the antiseptic treatment more difficult. Experience in the case of diabetics is here of the greatest value; but I have seen no danger result from attempting to secure primary union and sewing up wounds in such cases, if strict antiseptic precautions are employed; I have only found it essential to adopt extra precautions in such cases in order to prevent septic infection. In cases of senile gangrene, the effects of the antiseptic treatment are extraordinary; and the same influence may perhaps be said to be exerted on operations which have to be performed on tissues which have been subjected to severe injury. In fact, I should maintain that we know next to nothing of any influence exerted by changes in the tissues or fluids of the body which predispose to the occurrence of septic processes; and that much more accurate observations are required in this direction. My personal observation goes to prove that the antiseptic treatment, properly carried out, makes all such differences disappear which otherwise are tangible enough. To obtain primary union two things are essential:—Complete disinfection and absolute apposition, so that the parts may have the chance of growing together. If, therefore, a demand for simplification of the antiseptic methods is called for, it must be met with two objections:—First, that the sources of infection are so ubiquitous and so minute that, unless quite other methods are discovered, the process of excluding them must involve a most painstaking observance of minute details; and, secondly, that when great cavities exist, healing by first intention can only be obtained by the accurate application of dressings which will exert pressure. It cannot, for example, be obtained after excision of the hip unless close contact of the soft parts with the exposed acetabulum be secured, and in this way no cavity be left. In the same way with amputations and the scraping out of large abscesses, when the attempt to obtain primary union and the absolute prevention of suppuration is the highest aim of antiseptic treatment and of the treatment of wounds in general. Disinfect your wounds with the utmost care; sew them up, even in amputations, as carefully as if you were stitching a vesico-vaginal fistula; drain

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them, so that no blood or effusions can possibly accumulate; if the parts cannot otherwise be brought into contact, put on a carefully graduated pressure bandage, and you will be convinced that this end can be obtained even under the worst constitutional conditions.

Prof. ESMARCH, Kiel: I am of opinion that one of the best and most secure methods of avoiding suppuration and producing healing by first intention is that modification of Lister's treatment which I call antiseptic permanent dressing, which has been in use for two years in my practice. In order to show you that this method is not more dangerous than the pure Listerian treatment, I will shortly refer to our statistics of major operations in which this plan was adopted. (1) 146 extirpations of large tumours, including 40 amputations of the breast, with clearing out of the axilla, and 14 castrations. Of these 3 died—viz., 1 from pericarditis and inveterate syphilis, 1 from apoplexy, and 1 from fatty heart. (2) 51 amputations—viz., 18 of the femur, 27 of the leg, 5 of the upper arm, 1 of the fore-arm. Of these 2 died—viz., 1 from shock depending on weakness of the heart after amputation of the thigh, 1 from delirium tremens. (3) 61 resections—viz., 20 of the knee, 8 of the ankle-joint, 7 of the shoulder-joint, 14 of the elbow, 3 of the wrist, 5 of the ribs, 1 of the sternum, 2 of the calcaneus, 1 of the patella. (4) 11 exarticulations—viz., 3 of the hip, 5 of the ankle, 1 of the shoulder, 1 of the knee, which died of exhaustion, the patient suffering from inveterate syphilis (gummata of both testicles were found post mortem) emphysema of the lungs, and fatty heart. (5) 26 removals of sequestra, scooping out of carious bones, abscesses and fungoid growths. (6) 13 nerve stretchings, of which 12 were cured, 1 died from tetanus. This case was done experimentally, the tetanus being already present. (7) 8 herniotomy cases, all of which were cured. (8) 21 cases of opening and clearing out of "cold abscesses." All recovered. (9) 12 considerable wounds and lacerations of the soft parts. All recovered. (10) 49 complicated fractures and other large operation wounds. The proceedings adopted, and which were formerly described by Dr. Neuber, have been recently modified and improved, and the results have become more certain. Our antiseptic dressing at present consists of a small iodoform cushion, soaked, not with carbolic acid, but with a solution of iodoform in absolute alcohol (1 to 10), which is secured with an iodoform gauze bandage. Over this, a large pillow of coarse jute and gauze is placed, and over that again a moistened gauze bandage is firmly applied; lastly, the whole is enveloped by an elastic compress. We no longer employ any protective silk or mackintosh. The escape of secretions is effected through round holes made in the skin by means of a punch-forceps, through which are passed absorbable cartilaginous drainage-tubes. India-rubber tubes are seldom used by us now. The advantages of the antiseptic permanent dressing are:—(1) The absolute rest of the wound until cicatrization is effected; (2) The healing of the majority of the cases *per primam intentionem*, even those which present the most extensive wounds; (3) The avoidance of the danger of infection, by renewal of applications and bandages; (4) The diminished suffering of the patient; (5) The diminished labour of the surgeon; and (6) The diminished expense.

Mr. LUND, Manchester: I have listened with profound interest to Mr. Savory's remarks, and specially to his generous tribute to the scientific value of the laborious researches of Professor Lister, and in proof that I agree with all Mr. Savory has said as to the importance of extreme cleanliness in the treatment of wounds, to a pamphlet I published some years since on antisepticity in

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surgery, I was content to prefix the old motto "Keep the wound clean." In the concluding remarks of Professor Humphry, I detected the admission of one of the most valuable principles in the theory of the treatment of wounds and their consequences which the profession had adopted in recent times—viz., that excessive inflammation and pain in wounded surfaces, ulceration of the edges of a wound, or sloughing of its centre, with those more severe constitutional disturbances, erysipelas, septicæmia, pyæmia and the like, have their primary seat in putrefactive chemical changes in the secretions of the wound itself, and to avert these as ar fas possible, or to prevent them entirely, must be the duty of the surgeon from the earliest moment at which a wounded surface is entrusted to his care. The more simple, yet certain, the means employed, the more likely to receive universal adoption in operative surgery.

Prof. WILLIAM WARREN GREENE, Portland, Maine: Nearly all small wounds unite by first intention if perfectly coaptated. But larger and deeper ones do better with cotton wool or some other dressing that excludes the air. For fifteen years I have used cotton wool in all such cases. Then I found the need of drainage, then came the antiseptic ligature, then came Professor Lister's method. There is no time here for arguing these questions, so that I must content myself with stating my opinions based upon a large experience. First, let me say that I protest against being forced into a false position by the extreme advocates of Listerism. It has come to pass, in America at least, that if a surgeon does not follow all the details of Lister's method, he is classed with those who oppose antiseptic treatment. This is very much like the treatment of the homœopaths, who, because we would not accept the dogmas of Hahnemann, called us allopaths. I believe every member of this Section believes in the most thorough antisepsis and asepsis possible. The only question is as to methods. I for one believe the details of what is called pure Listerism unnecessary and impracticable for a surgeon in large private practice. Again, I do not believe the spray necessary, nor do I believe carbolic acid necessary or safe. You all heard Professor Keith's testimony on this point last week. My own experience and observation correspond with his conclusions. At present, although the question is entirely open, I believe that perfect cleanliness, the use of animal ligatures, thorough drainage, perfect coaptation of the flaps by proper sutures, the use of water for sponging at blood heat (in a bad atmosphere I would add some antiseptic like boracic acid), and an external dressing of cotton wool, give the best chances for union by first intention.

Dr. JAMES HARDIE, Manchester: I would insist upon the necessity of preventing the separation of the wound surfaces by blood or inflammatory discharge. These will certainly collect in the corners of the wound and cellular interspace, unless means be taken to maintain the entire surface in accurate apposition. I secure this object by the application of damp sponges, firmly applied by bandages. The resilience of this material renders it suitable for elastic pressure, while its absorptive property enables it with great readiness to absorb any discharge which may perhaps have taken place. The edges of the wound are accurately brought together, and no drainage is used. The dressing is not changed until the tenth day, when firm union may be expected to have occurred (several cases in point were mentioned). I consider that we have too little recognized the fact that discharge is not necessary in the primary union of wounds, and that our efforts ought to be directed to means of preventing rather than of absorbing it.

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Mr. J. K. Barton, Dublin: The antiseptic method of dressing of wounds has so many special applications that it is more than probable that the special cases to which I am about to refer may be of interest to some members of this Congress. In the hope that this may be so, I venture to take up a few minutes of your time. The special cases to which I refer are those of sinuses remaining after the opening of abscesses, &c., not those connected with diseased bone. The shortest and clearest way to explain my meaning will be to relate the facts of two cases:—(1) That of a young and strumous girl who had a series of abscesses in the right mamma, ending in numerous sinuses which would not heal under ordinary treatment. In this case, after a failure by all other means—for at this time I was not as well aware as I am now of the benefits to be obtained by the antiseptic method of dressing—the following method of dressing was used: the sinuses were first of all thoroughly cleansed by a solution of a chloride of lime, then they were injected by a solution of chloride of zinc (20 gr. to 1 oz.), then under the spray a piece of lint was laid over the wounds, well wetted in a solution of chloride of lime, this was covered with a pad of salicylic tow, the whole covered with carbolic gauze, and kept in position with a gauze bandage and calico bandage, in three days the sinuses were nearly healed; in a week they were healed. (2) A woman was sent from the West of Ireland whose appearance gave the idea that she was the subject of some malignant disease —the only lesion, however, which I could detect was a small abscess in the right thigh; this had been opened, and had been discharging pus for months, but at this time had healed over. There was evidently a little pus under the skin, so I opened this, and a teaspoonful of pus was discharged, a probe introduced ran down twelve inches, outside the fascia. In this case the same kind of dressing was employed—the wound and sinus were first thoroughly cleansed, then syringed with the chloride of zinc solution, then dressed with salicylic tow and carbolic gauze. From the day this dressing was applied suppuration ceased, the sinus was soundly healed in a week, and in three weeks the patient left the hospital, so changed in her appearance as to be unlike the sickly person she was when admitted.

Mr. F. J. Gant, London: All surgeons are, of course, agreed as to the vital importance of preventing septicæmia in wound treatment. There can be no such distinction recognised as antiseptic and septic surgeons. But there may be wide differences of judgment with regard to the methods of attaining the same end in view. Much has been urged in favour of antisepticism by the Listerian method of spray and gauze, and much also, with more apparent reason, in evidence of simpler methods—still involving the influence of some antiseptic agent; but I would briefly advocate the superior advantage of hygienic conditions, notably with regard to free ventilation, as affecting the atmospheric surroundings of wounds. Having closely watched the behaviour of wounds under treatment with the strictest antiseptic dressings, and without any such dressings, my experience for some years has at last impelled me to the conclusion that no appreciable difference can be discerned between the clinical career of wounds under either circumstances—provided only that what I would term preseptic hygienic conditions engage the constant and vigilant attention of the surgeon. Thus, the open treatment of wounds and the dry lint method of dressing have compared most favourably with any antiseptic precautions at the Royal Free Hospital. But this preventive aspect of septicæmia proceeds upon the assumption that the wound is kept free of organic matter, in the form of blood, pus, or sero-lymph effusion. Whether such freedom be effected

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by exposure, in order to induce a dry-glazed state of the wound before the coaptation of its surfaces, according to Liston's procedure after amputation, or whether drainage be employed by tube or position of the part, in either way any collection of organic matter may be prevented, and the local condition essential to primary union is fulfilled. In most parts of the body I prefer position to the introduction of a foreign substance for the drainage of wounds; and I must confess that I have never yet seen any fluid issuing from the aperture of a drainage-tube, although oozing up the sides of the conduit pipe. To illustrate the behaviour of wounds with and without antiseptic precautions, when subject to healthy hygienic conditions, I may cite a few cases now under treatment in the hospital:—(1) Necrosis of the shaft of the tibia, from diffuse periostitis and osteomyelitis, with secondary synovitis and articular destruction of the knee-joint. The patient, a boy, aged 10 years, had a small wound just above the ankle, from whence diffuse inflammation spread up the leg, in the form of lymphatitis, but which assumed a uniform blush redness, accompanied with considerable swelling and deep-seated pain. Periosteal suppuration supervened in the whole extent of the tibia; followed by acute synovitis of the knee-joint, with ulceration of the cartilages. Openings were made both in the leg and in the joint, and drainage-tubes were introduced. These surgical procedures were taken under Listerian antiseptic precautions, with after-dressings accordingly, my house-surgeon having been specially trained by Mr. Lister, and the most approved appliances used. After thus opening the abscesses, the temperature fell to sub-normal, but in two days it rose to 102·6°, and the pulse was 144. The destructive course of the disease continued, with profuse suppurative discharge of a dark green colour, and evidently putrefactive. At the same time the temperature and pulse went up and down in response to the state of the limb. July 30.—A week ago I removed the limb, amputation being performed in the middle third of the thigh. No antiseptic precautions were taken, and the stump was dressed with dry lint and bandage. On the day after operation, the temperature was only 98·4°, rising, however, in the evening two degrees, with a pulse of 100. On the second day, the normal standard was regained, and has since been maintained, with a variation of only one degree of temperature—*i.e.*, 99·4°. The flaps of the amputation-wound are uniting firmly, some pus having been discharged from the lower angle, the stump being placed in position for drainage. This case is eminently suggestive, as showing the course of an amputation wound in favourable contrast with that of the previously opened abscess in the leg and in the knee-joint, when guarded by the strictest Listerian precautions. And the comparison is the more apposite, as being exhibited in the same patient in the same ward of the same hospital. (2) In another bone-case, I removed a sequestrum from the lower third of the femur. No antiseptic precautions were taken during this operation, nor subsequently, yet, under dry lint dressing, the involucrum-cavity has filled up with conspicuously healthy granulations, resulting in a linear cicatrix. Throughout this process of healing, the temperature has remained quite normal—98·2°; except on one day only, when from some temporary cause, it rose two degrees. (3) Excision of the knee-joint in acute traumatic synovitis, with rapid destruction of the articular cartilages. On the 28th of June, about six weeks since, a boy, aged 16, sustained a severe contusion of the right knee, with laceration of the integument in front of the joint; he having been knocked down by a railway train. Abscesses around the joint, with suppurative synovitis, were promptly treated by openings and drainage; a probe could be passed through the articulation in two or three directions, and the ends of bone

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were felt to be denuded of cartilage. All this occurred in the course of a week or ten days. An exhausting discharge continued, and the patient's general health soon succumbed. Having regard, therefore, to this constitutional condition, I resolved to excise the joint, although in a state of acute synovial inflammation. After the operation, the limb was put up with my double splint, and the wound dressed as usual, with dry lint and bandage. This was done on July 30, and in the course of a week the discharge, which had previously amounted to four or five ounces daily, has now become reduced to two or three drachms of perfectly healthy pus. The incision is uniting throughout its extent, except at the angles which close as the oozing ceases. The temperature before operation, 102° to 104°, has fallen to 100°, 99°, and the normal standard. Appetite has returned, diarrhœa has ceased, and altogether the patient's health has undergone a marked improvement. These few cases, from a large assortment, will suffice to show what may be done in surgery without any antiseptic measures, when practised under advantageous circumstances of hygiene. If surgeons be compelled to perform operations in a foul hospital, there can be no doubt whatever that there the high ritual of Listerian antisepticism has yielded some splendid results; and that even more simple precautions than the spray and gauze have been followed by an appreciable diminution of septic blood-poisoning. But these topical safeguards are apt to divert the attention of the surgeon from the higher consideration of the hygienic conditions in which his patient should be placed beyond that of mere wound-dressing. With the advantage of clean air, drainage of organic matter and dry lint protection from external irritation will be all that is necessary for the natural process of healing—the simple means that they should take who have the power and they should keep who can.

Professor LETIÉVANT, Lyons: I should like to make some remarks on the practical results of the employment of the antiseptic treatment at the Hôtel Dieu, at Lyons. In 1868 I made some attempts with this plan of treatment, but did not regularly institute it till July, 1875, since which time purulent infection has disappeared from my wards, erysipelas is less common and less grave, and primary union has become the rule instead of the exception. I am able now to perform with confidence many operations which formerly I would not have undertaken, especially the opening and scraping of joints affected with fungous disease; I am also able to adopt successfully conservative measures in injuries of the gravest kind. But the most marked effect is the diminution of mortality, which has kept pace with the thoroughness and extent of the application of the treatment, till, during a period of five years, it has been reduced by almost a half. This is all the more remarkable, as no similar reduction had occurred previously. During the previous sixteen years the mortality varied from 7·72 to 6·25 per cent., and reached at times a very high percentage. Thus, in 1863, 1866, 1867, and 1873 it was 7·69 per cent.; in 1864, 1865, 1871, 7·14 per cent.; in 1860, 1861, 1862, 1869, 6·66 per cent.; in 1872, 1874, 1875, 6·25. Since 1875 it had diminished, reaching in 1878, 4·96 per cent., and in 1879, the last year for which the statistics are made out, 4·83 per cent. On such results as these I am glad to have the opportunity of congratulating Mr. Lister. Still, I must observe that it has not been demonstrated that the theory that wounds are infected by the air and its micro-organisms, is the true one. The remarkable labours of Pasteur have led surgeons to endeavour to eliminate this cause of infection from their wounds, and when M. A. Guérin introduced in 1870 his method of treatment, based upon this idea, and obtained his brilliant success,

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the truth of the theory seemed confirmed, but I have shown in 1872 that in fact the micro-organisms have the opportunity to develop under this form of dressing, and also that they do, in fact, grow and multiply with extraordinary rapidity, as, indeed, is indicated by the odour exhaled by the wounds, even while the wounds are healing well. Evidently the theory is at fault. Whilst engaged in investigating the ventilation at the Hôtel Dieu, at Lyons, in 1875, I ascertained this remarkable fact, that the Maternity Department of this Institution had remained free from infectious disease, though situated in an atmosphere presumably swarming with aerial germs of disease—*i.e.*, in close proximity to the most unhealthy surgical wards, the chemical theatre, the dissecting rooms, the operating theatre, &c. If the air and its miasmata were amongst the causes of infection in the wounds of lying-in women, this Maternity ought to have been often the seat of frightful epidemics, but as not one had occurred, I cannot assign much weight to the theory of contagion through the atmosphere. The explanation given by me was that this Maternity was, as it were, closed to outsiders. There are no cliniques, no midwives, no frequent examinations Specially trained sisters attend to everything; the physician merely gives his orders, which are carried out by them. If the surgeon is called in, he merely operates, and has no more to do with the patient. It is to this seclusion that I refer the healthiness of this Maternity, which is exceptional amongst the large French hospitals. I apply the same hypothesis to explain the success of the cotton-wool treatment, in which the wounds are untouched for many days, and are thus left free from external contamination; and, therefore, if they have not been affected with the causes of purulent infection at the time of operation, they escape. I must insist on the primary importance of this direct contamination by means of hands, instruments, &c., and maintain, that the excellent results to be obtained from the adoption of Lister's dressing depend rather upon the avoidance of this cause, than upon the exclusion of the sources of purulent infection that are to be found in the atmosphere.

Mr. Lister, in concluding the debate, spoke to the following effect:*—

Mr. President and Gentlemen,—To those of you who know from personal experience what antiseptic measures can do for surgery, it may well seem strange to find some who have taken part in this discussion still assigning them a secondary place in the treatment of wounds. The explanation of this fact is, I believe, to be found chiefly in the success,—nay, the brilliant success,—sometimes obtained without the use of any antiseptic means whatever. This has been conspicuously the case with abdominal surgery, and especially with ovariotomy. Ovariotomy, indeed, has sometimes been spoken of as a touchstone of the efficacy of the antiseptic treatment. The success which has attended antiseptic ovariotomy has been regarded as a signal proof of the truth of the antiseptic principle. Such, however, has never been my own view. Mr. Spencer Wells and Dr. Thomas Keith obtained results which astonished the world before strict antiseptic treatment was thought of; and when, several years ago, Dr. Keith expressed to me an intention of performing ovariotomy antiseptically, I strongly dissuaded him from his purpose. I knew his already brilliant success; I felt that our spray apparatus was as yet inadequate for the production of a cloud sufficiently large to cover the whole field

* In preparing these remarks for the press, Mr. Lister has expanded somewhat the facts and arguments adduced during the necessarily limited time at his disposal in the discussion.

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of operation, and sufficiently fine to avoid needless irritation, and I was also aware that such operations are often both very protracted and very anxious, while in proportion to the duration and the anxiety of an operation is the chance of the neglect of some apparently trivial, yet important, element in the procedure. And if the antiseptic treatment were attempted in ovariotomy, and failed in its immediate object, I felt that it would be not only nugatory, but injurious. It seemed to me that in any case of ovariotomy performed without antiseptic measures, there was a contest between effusion from the wounded surfaces and absorption of the effused serum by the uninjured peritoneum. If absorption kept pace with effusion, there was no time for putrefactive fermentation to take place in the effused liquid; but if absorption lagged behind, and effusion predominated, the serum accumulated in the abdominal cavity, and putrefying, gave rise to septicæmia. Now, supposing an antiseptic like carbolic acid to be employed in the operation, the peritoneal surfaces, as well as those of the wound, would be more or less irritated by it, and in proportion to this irritation would effusion be increased and absorbing power enfeebled, till such time as the temporary effect of the carbolic acid had subsided. And if, in spite of the antiseptic means, active septic matter had been introduced, putrefaction and septicæmia would be the natural result.

At the same time I believed that the day would come when strict antiseptic treatment would prove valuable in ovariotomy. Especially did I anticipate that it would permit early operation for small tumours, instead of the patient being kept waiting in anxiety, as used to be the case, till the tumour should have attained large dimensions. And, in point of fact, I think, judging from the records that have come to us from various quarters, and especially from Germany, that strict antiseptic treatment has rendered very great service to ovariotomy; that it has made the surgeon more independent of healthy surroundings, and also has made less essential the excessively minute care during the operation, and the close attention after it, which we have all so much admired in the practice of Dr. Keith. Thus surgeons generally have been brought, by the use of antiseptic means, much more nearly towards the high level of Wells and Keith. Even Keith, on at length adopting strict antiseptic measures with an improved spray, for a while surpassed himself by an unbroken series of eighty successful cases. Yet, wonderful as this achievement was, it was only a difference in degree from his former experience, and assuredly no absolute proof of the superiority of the new means employed. Of late, I understand, he has abandoned the spray, for reasons which I had not the opportunity of hearing him explain when this subject was discussed at the Congress; and I am informed that he continues to have admirable results, which cannot surprise us when we remember what he had arrived at in his early period without adopting antiseptic measures.*

But how, it may naturally be asked, can the success of ovariotomy, performed without the use of antiseptic means, be reconciled with the truth of the antiseptic principle? The answer is, I believe, to be found partly in certain peculiarities of the abdominal cavity, and partly in circumstances common to wounds in general.

* It is important to bear in mind that Dr. Keith always paid the most scrupulous attention to cleanliness; and among the elements of that cleanliness he included what was a very important antiseptic precaution—viz., the purification of the sponges which he used by boiling them; impure sponges being undoubtedly a very fertile source of septic contamination, while protracted boiling is a most effectual means of destroying septic ferments.

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One great peculiarity of operation-wounds within the abdomen, as compared with those in ordinary situations, is that already referred to—viz., that the plasma from the cut surface is poured out into a large cavity lined with a serous membrane disposed to absorb it as fast as it is effused. Thus, without drainage or any outlet whatever for discharge being provided, the serum is, under favourable circumstances, prevented from accumulating, as it would in ordinary wounds similarly treated, and opportunity is not afforded for putrefaction.

Another favourable result of the disposition of the parts is, that even if some accumulation of fluid does take place, the large size of the cavity, naturally adapted for variations in capacity, prevents the occurrence of tension, which is so common a cause of disturbance in ordinary wounds. More especially if a large tumour has been removed. the part affected is left in a state of the most perfect flaccidity and relaxation.

A further peculiarity in favour of abdominal wounds is due to the high vital power of the peritoneum. I recollect making a post mortem examination in a case of strangulated hernia, where death had taken place within forty-eight hours of the operation, and finding it impossible to discover the site of the incision by inspection from within the abdomen, so completely had the peritoneal wound already cicatrized. This high degree of vital energy operates beneficially in a manner which I shall be better able to explain when I have spoken of some circumstances common to all wounds in their relations to septic agencies.

At the Cambridge meeting of the British Medical Association last year, I brought forward facts which showed that the serum of blood is not at all so favourable a soil for the growth of micro-organisms as I had previously imagined. If we take a glass of uncontaminated milk or urine, so arranged that, if left untouched, it will remain for any length of time free from organisms, and add to it a drop of ordinary water, we are sure to find in a few days evidence of bacteric development in the liquid. Indeed, in the case of milk, which appears to afford pabulum for almost all varieties of micro-organisms, I have shown that if a dozen glasses of the liquid in a state of purity, in vessels suitably arranged to prevent contamination from without, receive each $\frac{1}{100}$ minim of tap water, most of the glasses will develop bacteria, though probably of different species, in the different vessels, showing how numerous and how various are the micro-organisms really present in water.* Even the comparatively crude liquid which we call "Pasteur's Solution" —a mere solution of cane-sugar, tartrate of ammonia, and earthy salts, in which many kinds of bacteria refuse to grow at all—will be pretty sure to produce such organisms in a few days if a drop of tap water is added to it. Now from these analogies, and knowing as we do to our cost that blood serum is but too liable to putrefactive fermentation, I had assumed that ordinary water contained putrefactive bacteria in a form that would develop in serum.

But when, in the course of an experiment to be again referred to, I drew blood with antiseptic precautions from the jugular vein of an ox into a series of purified bottles, about half an ounce into each, and, having allowed the blood to coagulate and the clot to shrink, introduced into them various quantities of tap water to mingle with the expressed serum, I found to my surprise that not only an entire

* See On the Lactic Fermentation, "Transactions of the Pathological Society of London," 1878. The results of such an experiment differ according to the season of the year. In cold wintry weather, when bacteria might be expected to be less numerous, it may happen that few of the inoculated glasses show any change at all.

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minim, but two, four, and even eight minims failed to induce putrefaction, although the bottles were kept in a warm box at the temperature of the body. I have since confirmed this experiment in the ox, and have also extended it to the blood of other animals—the donkey and the dog—with similar results. I even found that putrid blood in full activity, if largely diluted with water purified by boiling, and introduced in small quantity in proportion to the serum, failed to occasion putrefaction, or the development of any organisms that I could discover by ordinary microscopic examination. Yet the same quantities of the same dilutions quickly gave rise to putrefaction in blood of the same animal altered by dilution with an equal part of purified water; showing that they really possessed septic energy, though unable to exert it upon normal serum. Not that the blood of these animals was in its natural state incapable of putrefaction: for inoculation with a mere trace of undiluted putrid blood soon rendered it highly offensive. But the results of these experiments seemed to point to the remarkable conclusion that, after having been widely diffused by means of water, bacteria are incapable of developing in undiluted healthy serum. In this respect, serum is totally different in its behaviour from milk, in which, as I have shown elsewhere,* a single *Bacterium lactis*, detached from others by a similar process of diffusion by means of water, is as sure to produce its kind as are a million taken directly from souring milk.

How it is that the diffusion of the bacteria renders them incapable of developing in serum, I do not profess to understand. It may, perhaps, be that when bacteria are introduced directly from putrid blood, the products of the putrid fermentation adhering to them may induce chemically an alteration in the normal quality of the serum which, when thus impaired, may prove amenable to the nutritive energies of the micro-organisms; while, conversely, copious ablution with water may remove from the bacteria the associated substances which thus act as their pioneers. This view might be otherwise expressed by saying that the bacteria *per se* are unable to grow in normal serum, and can only develop in that liquid when it has been vitiated, whether by the addition of water or by the action of small quantities of the acrid products of putrefaction.

Or, again, it seems to me conceivable that the normal serum may oppose an insuperable obstacle to the nutritive attractions of an individual bacterium, but that this may be overcome by the associated action of several of the organisms in close proximity, after the analogy of the more energetic operation of a concentrated solution of a chemical re-agent. But, whatever be the explanation, the fact remains.

I have also made observations on the effects of exposure of uncontaminated blood to air in different localities; and I have found that even the introduction of considerable quantities of dust has not led to putrefactive change.

Applying this knowledge to the discussion of ovariotomy performed without antiseptic precautions, the question naturally suggests itself whether in many cases any septic organisms have really been introduced into the peritoneal cavity in a condition capable of developing in the effused serum. And thus we have suggested to us a further explanation of the success of such operations.

But the facts which have been elicited by the experiments referred to have a far wider range of application than to the special case of ovariotomy. They seem to indicate that the putrefaction so apt to occur in wounds not treated antiseptically is due rather to septic matter in a concentrated form than to the diffused con-

* *Vide* "Path. Trans.," *loc. cit.*

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dition in which it exists either in water or in air. They suggest the highly important question—Is the spray really necessary? In other words—Is there sufficient chance of the air of an operating theatre or private room containing septic matter which can prove effective in blood serum, to make it needful to regard the question of contamination from the atmosphere at all? If the answer must be given in the affirmative, and the choice must lie between the spray and antiseptic irrigation during the operation, at intervals varying according to the discretion of the surgeon, with syringing of the cavity of the wound after stitching, and syringing also at every dressing, then I should give my voice decidedly in favour of the spray, as being more sure of attaining its object, and involving less irritation of the wound, and also (if carbolic acid be the antiseptic used) much less risk of carbolic poisoning. At the same time it must be distinctly borne in mind that the spray is, beyond all question, the least important of our antiseptic means; and that the circumstance that a surgeon does not happen to have a spray producer at hand is no excuse whatever for his abandoning the attempt to obtain aseptic results. But if the apparatus for the spray is at my disposal, I for my part do not as yet dare to abandon it. By the careful use of our present means, the spray included, we have arrived, I think I may venture to say, at absolute security of attaining the great object in view, provided that we have the two essential conditions complied with, an unbroken skin to start with, and the seat of operation sufficiently distant from any source of putrefaction to admit of adequate overlapping of the surrounding integument by the requisite dressing. I leave it to those who have done me the honour to visit my wards to judge whether I am guilty of exaggeration in making this strong statement. Such being the case, I should not feel justified, except on perfectly established grounds, in omitting any part of the machinery by which results so important to our fellow-creatures have been arrived at.

Nevertheless, I am aware that, concomitantly with the perfecting of the spray there has been an improvement in other parts of our antiseptic arrangements: and I am not prepared to say that our increased uniformity of good results may not be due to the latter rather than to the former. And it may be, for aught I know, that, when the International Medical Congress next meets, I shall be able to speak of results of a still higher order obtained without using the spray at all. For if further investigation should confirm the conclusion to which our recent facts seem to point, and it should indeed be proved that all idea of atmospheric contamination of our wounds during operations may be thrown to the winds, then no one will say with more joy than myself, "Fort mit dem Spray."*

The fact that normal serum is not made to putrefy by the addition of a small quantity of water came out unexpectedly in the course of an experiment designed to illustrate a view which I had long entertained as an inference from circumstances observed in surgical practice—viz., that an undisturbed blood-clot has a special power of preventing the development of septic bacteria. This property I ascribed to the white corpuscles, which are well known to retain their life long after blood has been shed from the body.† I supposed these living elements of the clot to produce this effect in accordance with a principle which I believe I

* This is the title of a recent paper by Professor Bruns, of Tübingen, advocating the substitution of carbolic irrigation for the spray.

† Last autumn I observed amœboid movements in white corpuscles from the buffy coat of a donkey's blood, which had been for two days in a glass vessel.

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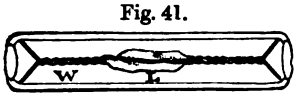
happened to be the first to demonstrate, but which is now generally admitted—viz., that the tissues of a healthy living body have a power of counteracting the energies of bacteria in their vicinity, and preventing their development.

The experiment which I related at Cambridge seemed to confirm this view. I have already described one part of that experiment, consisting of the introduction of small quantities of water into vessels containing uncontaminated blood after coagulation had occurred, and the clot had shrunk so as to press out the serum. In the other part of the experiment the water was introduced before the blood into the purified bottles which were to receive it; so that any organisms which the water contained were diffused in the entire mass of the blood before coagulation, and were presumably retained in the clot when it shrank and pressed out the serum. And unexpectedly great as the resisting power of the serum to the development of the micro-organisms proved to be, that of the coagulum seemed markedly greater. But our knowledge of the remarkable influence of the quantity of the ferment in proportion to the amount of the serum, as indicated by the more recent experiments, makes the evidence on which I relied at the Cambridge meeting far from conclusive:* so that I cannot now regard it as demonstrated for blood outside the body that the coagulum has a greater power than the serum of resisting putrefaction. But what I desire particularly to mention on the present occasion is an experiment with reference to the behaviour of the coagulum under circumstances in which it especially interests us as surgeons—viz., within the living body. The experiment was of a character such as it would have been difficult under existing circumstances to perform in London; so I resorted to the Ecole Vétérinaire of Toulouse, where everything was most liberally placed at my disposal by my friend Professor Toussaint and others in authority at the institution.

Having provided four pieces of glass tube, each about 1½ inch in length and ⅛ inch in diameter, containing two pieces of silver wire (w) twisted together, with a little piece of fine linen cloth (L) fixed between them at their central part, the whole apparatus having been purified by steeping in a strong solution of carbolic acid, and subsequent boiling in water, and drying over a lamp, I applied to each bit of linen minim of septic liquid, which was in the case of one of the tubes undiluted putrid blood, in the second that blood diluted with ten parts of pure (boiled) water, in the third a dilution with a hundred parts of the water, and in the fourth a thousandfold dilution. I then exposed the jugular vein of a donkey in five places with antiseptic precautions, leaving four intervening portions of the vessel intact and covered with the integument, and introduced into each undisturbed portion of vein one of the glass tubes with the septic rag in its interior, strong ligatures of string purified with carbolic solution being applied tightly as each tube was introduced, so as to form four venous compartments or capsules.

Fig. 41.



* If septic liquid is diffused through blood before its coagulation, the bacteria are probably almost all, if not all, entangled in the meshes of the fibrin, and retained by them when the serum is pressed out by the shrinking clot. On the other hand, if the liquid is added after the contraction of the coagulum, the bacteria are, in the first instance, confined to the serum. Hence, supposing the serum and clot alike in their resistance to bacteric development, we should expect, according to our present knowledge, that different results would follow such a comparative experiment, according as the serum was large or small in amount in proportion to the clot. Thus, when the serum is relatively scanty,

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quite distinct from one another, and consisting of vascular tissue healthy at the outset.

After three days the animal was killed and the venous compartments examined. In all four putrefaction had occurred within the glass tube; but the state of the coagulum outside the tube was extremely interesting. I must content myself on the present occasion with referring to the condition of the first compartment, or that which had received the undiluted putrid blood. Immediately around the glass tube the clot was so much softened by putrefaction as to be almost diffluent. But next to the greatly thickened wall of the vein, and forming an adherent lining to it, was a layer of firm coagulum, in some parts about ¼ inch thick, resembling to the naked eye a perfectly recent clot. Before introducing the tubes into the vein I had drawn blood from it, with antiseptic precautions, into a number of purified stoppered bottles containing each the same quantity of septic liquid as the tubes received ($\frac{1}{10}$ minim) of various degrees of dilution, from the unmixed putrid blood to a mixture with 100,000 parts of boiled water. The bottles received each about as much blood as a venous compartment contained, and they were kept at the temperature of the body. Three out of four which had received the 100,000-fold dilution, as well as two kept uninoculated as standards of comparison, remained permanently free from putrefaction;* but those inoculated with larger proportions of the septic material putrefied, and those which received the undiluted putrid blood were, at the time when the vein was examined, in a state of utter decomposition, presenting a very striking contrast with the apparently unchanged layer next the wall of the vein.

But unaltered as this layer appeared to the naked eye, the microscope showed it to be in reality a very different structure from a recent clot. It proved to be teeming with cells of new formation, far more numerous than the white corpuscles of the blood, and differing from them altogether in character, being as a rule larger, and often of great size, with pellucid contents, and having as their nuclei bodies more or less closely resembling pus corpuscles. On the day after the death of the animal and removal of the vein I continued the examination of the first compartment; and I was tearing off a part of the firm coagulum from the lining membrane when I opened into a little cavity in the clot about ¼ inch long and ⅛ inch in other dimensions, containing a thick liquid of a pearly white colour, the cavity having a thin grey lining, separated by a layer of dark clot from the wall of the vein, except at one part. Here it communicated with a small venous branch, whose contents had been entirely converted into this white liquid for a short distance, beyond which the branch was obstructed by clot. On microscopic examination I found this white liquid composed entirely of closely packed corpuscles of new formation, like those above described as infiltrating the clot. Here, however, the new cell development had taken place at the expense of all the original

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as is the case with the blood of the ox, which was the subject of the experiment related at Cambridge, if a very dilute septic liquid were blended with the blood before coagulation, the bacteria, few in proportion to the bulky clot, might fail to develop in it; but if the same quantity of the same liquid were dropped in after the clot had contracted, the bacteria, concentrated in the smaller quantity of serum, might succeed in growing in it. Observations of such a character cannot therefore be regarded as affording evidence of any special resisting power on the part of the coagulum as compared with the serum, unless an estimate is made of the relative amounts of serum and clot in the particular blood which is the subject of experiment, and a judgment formed accordingly.

* The last observation was made nine days after the commencement of the experiment.

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constituents of the coagulum; so that the fibrin had entirely disappeared, and only a stray red corpuscle here and there was to be discovered, and no granular débris was observed. The liquid was in fact neither more nor less than pus, and the cavity a small abscess. The evidence of endogenous cell development was in this liquid extremely striking. Many of the corpuscles resembled those of ordinary pus, though of varying dimensions; but often bodies exactly similar to the free pus corpuscles were seen still included as nuclei within large pellucid cells. In one such cell which I sketched there were four nuclei, three of which resembled the free pus corpuscles. Thus I had the opportunity of repeating observations made in the course of very similar experiments carried on as early as 1864, experiments which, I believe, helped to prepare my mind for applying to surgical practice the conclusions of Pasteur as to the nature of putrefaction. Those observations have never yet been published, as I was compelled to suspend the investigation by the pressure of clinical work in connection with the development of the antiseptic system. They proved, however, in the clearest manner that a blood-clot within a vein is, under septic influence, liable to a genuine suppuration—that is to say, to a change which is no mere result of breaking down of fibrin and accumulation of white corpuscles of the blood, but consists of a growth of living corpuscles multiplying by endogenous cell development at the expense of the original constituents of the coagulum.

As to the source of these newly-formed corpuscles, we must suppose them to have been derived either from white corpuscles of the blood or from proliferation of the elements of the tissues of the wall of the vein. If we conceive them to have originated in white corpuscles of the blood, it is nevertheless quite certain that they were not mere emigrated white corpuscles, but that they had sprung from them as a new and altered progeny. The actual appearances presented favoured the view that the new cells had been derived from the tissues. On examining stained sections* of the wall of the vessel and the adherent coagulum, I found that the corpuscles in the latter were similar in character to those which thronged the interstices of the tissue of the inner part of the vascular wall; and among the latter were some that conveyed the idea of transition from the normal tissue elements. And this view seems confirmed by the fact that I have never succeeded in obtaining any new growth of corpuscles in coagulum outside the body, although the blood has been subjected to very various degrees of septic agency, and has been kept in conditions similar to those within the body as regards temperature and moisture, and indeed in every respect, so far as I can judge, except only the influence of surrounding living tissues.

There was another circumstance observed in the clot within the vein which I must not omit to mention. Even the parts next to the lining membrane, though not putrefying, had a distinct faint putrefactive odour. This is readily explained on physical principles by diffusion of the products of the putrefactive fermentation which was going on within the tube and in its vicinity. Indeed, one of the other venous compartments, on being opened, gave exit to a small amount of fœtid gas, the remains of that which had been evolved from the putrefying part, and which had been only partially taken up by the surrounding parts; but in the other compartments it had been all so disposed of. This diffusion of chemically irritating

* I gladly avail myself of this opportunity of publicly thanking M. Laulanier, the Professor of Anatomy in the Ecole Vétérinaire, for his great kindness in preparing these sections for me.

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products of putrefaction, beyond the limits of the actual septic process, seems to me a matter of much importance; and in the case of the ass's jugular, it appears to explain what otherwise might be difficult to account for, the inflammatory thickening of the coats of the vein and also of the surrounding cellular tissue. For the striking contrast presented between the inflammatory appearances in and around the vessel in this animal, and the entire absence of such appearances which I have found both in the horse and in the calf, where I have tied the carotid artery antiseptically—with silk in the one case, and with catgut in the other—seems to imply that the septic matter introduced within the vein of the donkey was the cause of the disturbance in question; and the mode of its operation would seem to have been that above indicated.

But to return from this apparent digression. I have to add that, although twenty-four hours had passed since the vein was taken from the animal, I saw, in the course of a long investigation of the pus in the little abscess, only a single bacterium. Also I have to mention the important fact that, in the examination of the previous day, when the vein had been just taken from the body immediately after death, whereas in the softened part of the clot, in the vicinity of the glass tube, the red corpuscles were utterly altered and accompanied by abundant bacteria of different kinds, some in active movement, the piece of firm clot from near the wall of the vein showed the red corpuscles often exactly like those of freshly drawn blood,* while if any bacteria were present, they did not declare themselves to ordinary microscopic examination.

Thus the experiment affords very striking proof of the power of an undisturbed healthy coagulum in the vicinity of living tissues to resist the development of putrefactive bacteria, even when present in a highly concentrated form; while we have, as I believe, an explanation of this power in the multitude of new living elements with which the clot was peopled.

Turning now to the application of these pathological facts to ovariotomy, we find that they seem to complete the explanation of the success which has attended that operation performed without antiseptic precautions. Among the points in Dr. Keith's earlier practice which most excited alike my admiration and my astonishment was the way in which he diagnosed an accumulation of fluid in the pouch of the peritoneum termed "Douglas's Space," and, drawing off by puncture *per rectum*, with cannula and trochar, a quantity of putrid liquid, saved his patient from impending death. I greatly admired the skill of this practice; but I was astonished at the pathological facts. How was the septic process limited and prevented from spreading throughout the cavity of the peritoneum? This seems now to admit of interpretation. In such cases, septic matter must have been introduced during the operation in a form capable of developing in the blood; and if we suppose a portion of such effective septic ferment imprisoned in the interior of a coagulum in the space referred to, this clot, situated within the living body, may be conceived to oppose an obstacle to the development of the putrefactive bacteria in its substance, as was the case in the venous compartment of the ass's jugular. And here the high vital energy of the peritoneum would come into play. We have seen how rapidly a wound in the peritoneum may heal; and this rapid

* The only change observed in the red corpuscles of this part of the clot was, that in some cases they had an abnormal purple colour, which I presume was due to products of putrefaction diffused through the clot from the neighbouring putrefying parts.

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healing is, in other words, the rapid peopling of the lymph in the wound with vigorous new living elements. For, whatever view may be taken as to the source of these new corpuscles, certain it is that the organization of lymph proceeds more rapidly in proportion as the wounded tissues are in a more vigorous condition. Thus, a wound on the dorsum of the foot of a feeble old man, though strictly protected from septic influence, heals very languidly; while a cut in the lip of a child unites with great rapidity. This, indeed, would seem a further argument in favour of the tissues being the source of the new corpuscles. But be this as it may, the lymph on the peritoneum becomes very rapidly thronged with new living elements; and the same will be the case with a blood-clot, which differs from recent lymph merely in containing the additional, and probably entirely neutral, element of the red corpuscles. But, in the meantime, as in the ass's jugular the tissues of the vessels were irritated, as it would appear, by the acrid products of putrefaction diffused around the septic parts of the clot, and penetrating to those which were not really septic, so would the parts of the peritoneum surrounding the clot in Douglas's Space be excited to adhesive inflammation, glueing the opposed surfaces together by lymph; and, again, this lymph, becoming rapidly organized through the high vital energy of the peritoneum, would oppose an effectual barrier to the spreading of bacteric development should it extend to the surface of the clot. At the same time, the irritation caused by the putrid products, where most intense, would induce copious effusion of plasma and accumulation of putrid serum. In time, this state of things would lead to limited abscess, such as has been occasionally met with after ovariotomy, the discharge of the putrid pus having, in some instances, been followed by the recovery of the patient.*

But the power of organizing blood-clot or lymph to resist the advancing development of putrefactive bacteria has, like the behaviour of the serum in relation to putrefaction, a far wider range of application than to the explanation of the success of ovariotomy performed without antiseptic treatment. It serves, for example, as I believe, to explain a fact like the following. In Mr. Syme's celebrated case of diffuse aneurism of the axillary artery treated by "the old operation," the aneurismal clots had not only enormously distended the axilla but reached backwards so as to raise the scapula, and upwards high above the clavicle. He entrusted to me the duty of compressing the subclavian artery; and, as a preliminary measure, made a small incision penetrating through the deep cervical fascia, so as to enable my thumb to be thrust through the mass of clots in that situation and reach the first rib, on which the vessel lay. He then laid open the mass in the axilla by a free and rapid incision, and scooped out with his hands enough of the clots to enable him to gain access to the arterial trunk and tie it above and below the place of its communication with the aneurismal cavity.

* It is conceivable that, even without the intervention of a coagulum, a collection of serum in Douglas's Space, undergoing gradual putrefaction through the influence of some effective septic particle introduced at the operation, and being prevented from diffusion through the general peritoneal cavity, in the first instance, by a state of perfect repose of the patient, might, by its irritation, cause such adhesive inflammation as would prove an effectual barrier against the spreading of bacteric development. For the putrescent serum would produce inflammatory disturbance, not merely in the parts on which it acted directly, but also by sympathy (through the nervous system) on parts in the vicinity. The serum of the plasma effused through this agency would be absorbed by neighbouring healthy parts, leaving the lymph to glue the peritoneal surfaces together, and, becoming rapidly organized, to operate as a vital barrier against the advance of putrefaction.

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Making no attempt to remove the rest of the coagula, which indeed would have been quite impracticable, he simply brought the cutaneous margins of the wound together by interrupted sutures, the ends of the silk ligatures being left hanging out of the wound, in accordance with the then universal practice; and, as this was before the days of antiseptic management, no special treatment of that kind was, of course, employed, but a dressing of dry lint for the first few days and water-dressing afterwards. Considering the well-known danger of diffuse suppuration which, without antiseptic treatment, used to attend free incision into a mass of extravasated blood, I was astonished to see both the puncture above the collar-bone and also the main wound heal in the kindest manner, without more suppuration from the latter than must have attended a wound made in healthy tissues as a consequence of the presence of the ligatures. The fact now ceases to be wonderful if we take into account the circumstance that the outlying clots had been for several days among the tissues and had thus naturally-acquired new living elements which would confer upon them the power of fencing themselves against advancing putrefaction.

Our principle seems, however, to find its widest application in aiding to explain the possibility of union by the first intention without the use of any antiseptic means—nay, in spite of the application of septic ones—for such is, in truth, the apparently cleanly water-dressing. As certainly as we remove it from a wound after the lapse of twenty-four hours, do we find that the diluted blood serum which then soaks the lint has a putrid smell, implying that it contains septic ferments such as would assuredly act effectively upon blood outside the body. And yet in the not uncommon case of the occurrence of primary union under such treatment, putrefaction fails to spread into the wound; for if it did so, it would inevitably provoke suppuration.

Long before I entertained the idea of the antiseptic principle I often contemplated with wonder the behaviour of a thin layer of lymph or coagulum between the surfaces of a healthy wound, as contrasted with that which the same material would have exhibited, had it been placed in similar conditions as to temperature and moisture between two sheets of glass or gutta-percha. The absence of putrefaction in the former case, as compared with its occurrence in the latter, was plainly due in some way or other to the influence of the living tissues between which the putrescible lymph or blood-clot lay. Beyond this, however, all was mystery. Afterwards, when the power of healthy living tissues to oppose bacteric development became apparent to me, as exemplified by the inability of bacteria to grow in the mucus of a healthy urethra,* I attributed the absence of putrefaction in the healthy wound to a direct control on the part of its tissues over the septic bacteria. There is, however, a defect in this explanation—viz., that, whatever be the nature of the controlling agency of the tissues on the bacteria in their vicinity, it can hardly be conceived that it is exerted beyond an extremely limited range; so that in order that it should be effective, a more accurate and close apposition of the cut surfaces would seem necessary than actual experience shows to be required. And here our new principle comes to our aid, when we learn that the clot itself, as it becomes organized, acquires the defensive property which remained to be accounted for.

The thinner the layer of the clot, the more rapidly, *cæteris paribus*, will it be densely peopled throughout with the new corpuscles which confer upon it this

* See "Trans. of the Royal Society of Edinburgh," vol. xxvii. p. 315.

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defensive power; and the process will also advance quickly in proportion as the tissues of the cut surfaces are vigorous and active. And thus we have the rationale of the rules of the older surgery in aiming at union by first intention—viz., to perform the operation as much as possible by clean cuts with a sharp knife that shall injure the tissues as little as may be, apply the cut surfaces closely together, at the same time providing an outlet for effused blood and serum, and afterwards endeavour to avoid inflammatory disturbance which, in proportion to the degree in which it exists, enfeebles the vital powers of the part affected.

Yet, sound as these maxims were in the then existing state of surgical science, the best efforts of the surgeon were too often thwarted by "unhealthy actions," as he termed them, of which he did not understand the cause, and which he was consequently powerless to prevent. And we must not allow an exceptional case like ovariotomy to blind our eyes to the truth that the disasters of surgery in the past have been essentially caused by septic agencies.* Simplification of our means of procedure is, no doubt, in itself highly desirable; and I have already indicated one direction in which it may possibly be attained. But the safety of our patients incomparably transcends such a consideration; and it would indeed be a grievous thing if our desire for simplicity should induce us in any degree to relax our efforts to carry out the strict antiseptic principle, the strenuous endeavour so to deal with wounds as to prevent, from first to last, the development in them of pathogenic organisms.† The means by which this object may be most surely and at the same time most conveniently attained, will no doubt vary greatly in the future, in accordance with our ever advancing science; but whatever modifications we may admit in our methods, let us at all events never be satisfied with any that does not yield results at least as good as those which it is now in our power to secure.

If we suffer ourselves to be drawn aside from the strict antiseptic principle, we shall not only subject our patients to the risk of the old disasters, but we shall be compelled to withhold from them the benefit of valuable procedures which strict antiseptic management alone can warrant. Take as a single example the case of a loose cartilage in the knee-joint. To remove it by free incision is the most simple and satisfactory treatment, except for the attendant danger which was formerly so great as to be prohibitory, but of which our present means of carrying out the antiseptic principle have entirely disarmed it. If such a procedure was ever ventured on without antiseptic means, the only chance of success lay in accurate closure of the wound, with a view to primary union. On the other hand, under antiseptic management, I systematically abstain from closing the wound completely, leaving a part unstitched for the introduction of a drainage-tube, so as to guard against the inflammatory disturbance which might otherwise result from accumulation of fluid in the articular cavity. In other words, I abstain from the only means which would have afforded hope of success without antiseptic treatment, and I adopt means which, without antiseptic treatment must infallibly lead to disaster, through septic suppuration of the articulation. Here, then, I conceive we have a true touchstone of the truth of the antiseptic principle. The loose cartilage which I hold in my hand

* Under the term "septic" I, of course, include all unhealthy conditions due to the development of micro-organisms, whether the changes which they occasion in the organic fluids and solids be, or be not, accompanied by putrefactive odour.

† The term "pathogenic bacteria" has been introduced by the German pathologists to signify bacteria which give rise to disease in the animal body by developing within it.

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was removed from the knee of a gentleman a fortnight ago. It lay imprisoned in the angle between the anterior part of the articular surface of the tibia and the femur. I extracted it by free incision: and as the situation was one which did not admit of convenient insertion of a drainage-tube into the joint, I left the wound widely gaping throughout. That patient has not since experienced the slightest uneasiness, nor has there been any disturbance local or constitutional.

And this leads me to make the general remark that, under strict antiseptic treatment, union by first intention has no longer the importance it used to possess. As regards the essential points of avoidance of inflammation and fever, of pain and danger, it is a matter of absolute indifference whether primary union occurs or not. Nay more, as in the case just referred to, if we wish to make doubly sure of preventing all inflammatory disturbance, it is sometimes well to avoid stitching, and the tension which may become associated with it. There is at present in King's College Hospital a man on whom I operated three weeks ago for ununited fracture of the humerus, cutting down on the fragments and uniting them by a suture of thick silver wire, after sawing off their extremities. Now, in a case of this kind some years ago in which I applied stitches to the wound, although a good-sized drainage-tube was used, a certain amount of inflammatory disturbance occurred, which I could only attribute to want of sufficient exit for the abundant sanguineous oozing that took place from the osseous surfaces. This disturbance led not merely to some deep-seated suppuration, but to necrosis of a bit of one of the fragments, which greatly retarded the cure. Hence I have since abstained from anything like close stitching after such operations, and in the patient referred to the wound was left gaping widely. At the next dressing it was found occupied by blood-clot which has subsequently become organized, and at the same time has contracted so much that the cicatrix, already nearly complete, promises to be little more than linear. Meanwhile there has been no uneasiness, redness, or swelling, and no febrile disturbance, and if any imperfect pus whatever has been formed, it has been only from the surface of the organized coagulum as a result of slight unavoidable stimulation by the antiseptic used in the dressing. And here we have, as I believe, another touchstone of the antiseptic principle; showing that it is true, and gives results both new and important. For without effective antiseptic means of some kind or other such a course of such a wound would, I believe, be impossible. The impermeable protective layer applied next the wound under the antiseptic gauze prevented evaporation, and maintained a constant state of moisture of the surface of the clot. Now a moist dressing other than an antiseptic one could not have failed to occasion putrefaction of the exposed coagulum and suppuration of the cavity of the wound.* Yet all who are familiar with antiseptic practice know that it is a common thing to see the organization of the blood-clot proceed under the protection of the superficial layer of coagulum to complete healing without a particle of suppuration, a scar being found when the superficial layer is detached.

Time would not permit me to refer to all that has been communicated by

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* In an animal like the donkey, the reparative energies of the tissues would appear to be greater than in man, as is indicated by the well-known facility with which healing by scabbing is obtained in veterinary practice. Yet in the donkey's jugular it was only the parts of the coagulum near the wall of the vein that had escaped putrefaction. Even in such an animal, therefore, an exposed clot covered with water-dressing would putrefy, with the exception of the parts next the tissues; and these would granulate and suppurate. Under dry dressing, indeed, even in man, healing without suppuration has been

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those who have taken part in this discussion. I cannot, however, forbear making a passing allusion to the extremely remarkable results which have been related by Professor Esmarch as obtained by his permanent dressing: results so surprising that they would be incredible were it not for the perfect trustworthiness of the authority that vouches for them. And I would ask those who advocate mere cleanliness, as distinguished from antiseptic practice, how they can reconcile their views with facts such as these. What can be more dirty in the ordinary acceptation of the term than a wound left covered up with the same dressing for weeks together, the original blood and serum remaining upon it intact under this "Dauer-Verband"? Yet it is surgically clean because it is aseptic. On the other hand the æsthetically cleanly water-dressing is surgically dirty, because it contains elements which give rise to septic changes in wounds.

One other point which has been referred to in connection with this debate is of so much importance that I cannot but notice it. The old objection has been revived that antiseptic treatment leads the surgeon to concentrate his attention upon local measures, to the neglect of general hygienic arrangements and a due consideration for the constitutional state of the patient before subjecting him to operation. I do not think that this charge is at all warranted by facts. For my own part I have from the first used antiseptic treatment even for superficial wounds and sores, not so much for the sake of individual cases (which I knew did well, as a rule, under water-dressing) as for the express purpose of preventing them from contributing elements of general unhealthiness to the hospital ward. And if I have sometimes allowed the beds under my charge to be more crowded than is in accordance with the views of modern hygiene, it has been only after such a condition, originally brought about by accidental circumstances, had proved as a matter of fact consistent with perfect healthiness of the patients. The septic element being suppressed, less space was found to be essential to salubrity.

As to the other part of the charge—viz., that antiseptic treatment leads to a less careful selection of patients for operation than ought to be made—it seems to me to be refuted by the success which confessedly attends the practice. If indeed the charge were well founded, and we did really operate upon patients whose constitutions rendered them unfit subjects for such interference, good general results obtained under circumstances so highly unfavourable would afford most conclusive proof of the value of the local treatment employed. But the truth is that the suppression of the septic element enlarges the capabilities of surgery in the constitutional direction no less than in the local; and enables us to extend the benefits of needed operations to patients whose constitutions are so enfeebled by age or vitiated by disease that without strict antiseptic treatment no prudent surgeon would venture to meddle with them.

I appeal to the logical faculty of this great assembly of eminent men, and beg them to consider carefully in relation to this question the familiar case of a simple fracture or dislocation. Do we feel anxiety regarding the state of the

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sometimes seen in open wounds of considerable extent, especially when the affected part has been kept completely at rest and supported by methodical compression, as insisted on by M. Alphonse Guérin and Mr. Gamgee. Here the dry dressing appears to have an antiseptic influence, by causing an inspissated state of the serum in the dressing; for it has been shown by Naegele that bacteria are unable to develop in concentrated organic solutions. Such results of mere dry dressing cannot, however, be reckoned on with anything like certainty.

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constitution of a patient who has received such an injury? The mischief done is in itself of a worse character than the surgeon ever inflicts. Yet so long as the unbroken skin shields the bruised and lacerated tissues from the access of materials coming from the external world, repair advances safely, no matter what be the constitutional condition; the exceptions being so extremely rare that we practically leave them out of consideration altogether. It therefore surely follows that if we could contrive a treatment of our wounds which would have all the advantages of the unbroken integument, we might operate without anxiety on account of the constitution.

To provide a condition of our operation wounds that shall put them fully on a par with subcutaneous injuries is plainly the ideal of our art. Towards the attainment of this ideal we have already made large progress; and towards its full achievement, so far as it be possible, I would earnestly invite the best efforts of my hearers.

L'Influence de la Diathèse Tuberculeuse Goutteuse, ou autre sur la Syphilis.

Professor VERNEUIL, Paris.

La syphilis atteint les sujets malades aussi bien que les individus sains; elle n'empêche point ses victimes de contracter de nouvelles maladies; aussi peut-on la voir co-exister avec tous les états constitutionnels héréditaires ou acquis: scrofule, tuberculose, arthritisme, herpétisme, diabète, &c., avec toutes les intoxications: alcoolisme, paludisme, septicémie, &c.

Souvent, sans doute, il y a simple coincidence; les deux états évolvent parallèlement sans s'influencer et sans se modifier réciproquement; mais le contraire n'est pas rare, et l'on peut alors constater la réaction d'une maladie sur l'autre et, comme conséquence, la création de formes mixtes très-variées, fort importantes à connaître en pratique, et insuffisamment étudiées jusqu'à ce jour.

Certes, Hunter a entrevu les rapports de la syphilis avec la scrofule; Ricord a édité les expressions pittoresques de scrofulate de vérole et de chancre anophagédénique; on a inscrit parmi les causes du même phagédénisme l'intoxication palustre. Plusieurs dermatologistes ont étudié la syphilis chez les herpétiques et les alcooliques; tels, Ory, Renault, Revillet, Pichard, et je sais de mon savant ami le professeur Fournier que la question est loin d'être épuisée. Quelques auteurs enfin ont avancé que la marche des affections syphilitiques dépend au moins autant du terrain que de la graine, mais ces dernières observations sont restées incomplètes et sans lien, de sorte qu'il n'existe aujourd'hui aucun travail d'ensemble sur les hybrides que forme la vérole avec les autres maladies générales.

Cette lacune doit être comblée, car le peu qu'on en sait prouve déjà que la syphilis compliquante ou compliquée présente dans ses manifestations, sa marche, son diagnostic, son pronostic, et son traitement, des particularités tout-à-fait dignes d'intérêt.

J'ai vu pour ma part la vérole associée à la plupart des diathèses et des maladies générales, mais je n'ai recueilli d'observations détaillées que sur les hybrides scrofulo-syphilitiques et syphilo-cancéreux.

Hybridité Scrofulo-syphilitique.—En générale c'est la scrofule, état antérieur

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qui agit sur la syphilis, état surajouté. La réciproque est plus rare; cependant chez les jeunes sujets la vérole rappelle quelquefois la strume latente depuis l'enfance, de sorte qu'on attribue volontiers à la première les désordres engendrés par la seconde. La scrofule manifeste diversement son influence: d'abord elle semble attirer et fixer la syphilis sur les organes qu'elle affecte elle-même de préférence: peau, ganglions lymphatiques, périoste, &c. Elle y provoque communément le processus suppuratif, rare dans la syphilis ordinaire.

J'ai vu chez des scrofuleux l'adénopathie symptomatique du chancre infectant se terminer par suppuration; j'ai vu les accidents secondaires buccaux et pharyngiens faire apparaître au cou de nombreux ganglions qui plusieurs fois ont suppuré.

J'ai actuellement dans mes salles une femme scrofuleuse dès son enfance, syphilitique depuis huit ans, et atteinte d'accidents tertiaires en plusieurs points du corps. Les ganglions auxillaires, inguinaux, épitrochléens sont volumineux et indurés. Deux d'entre eux sont restés fistuleux après suppuration.

Les syphilides ulcéreuses, recouvertes de croûtes humides, se rencontrent de préférence chez les strumeux.

La scrofule modifie à ce point l'aspect des ulcérations syphilitiques tertiaires que le diagnostic peut être très-difficile, et que le traitement seul parvient à lever les doutes. Chez un vieux scrofuleux presque moribond, miné par une tuberculisation pulmonaire avancée, il existait sur plusieurs points saillants du corps, acromion, coudes, rebord inférieur des fausses côtes, protubérance occipitale externe, des ulcérations atoniques et indolentes avec mortification plus ou moins profonde de la peau, et que je n'avais guère plus de raison de rapporter à la syphilis qu'à la scrofule. Je diagnostiquai un hybride, j'administrai le traitement spécifique, et en moins de deux mois toutes les plaies étaient presque cicatrisées. La phthisie elle-même semblait arrêtée dans sa marche.

Chez la femme scrofulo-syphilitique dont il a été parlé plus haut, le corps est couvert de cicatrices qu'on pouvait également considérer comme syphilitiques ou comme scrofuleuses.

La scrofule, à l'état de dyscrasie simple, ne paraît pas aggraver notablement la syphilis; peut-être en rend-elle les manifestations locales plus tenaces et plus fixes, mais en revanche elle en supprime généralement la douleur. Il est vrai que la plupart des affections véroliques sont indolentes, mais quelques-unes cependant font exception à la règle—telles, l'iritis et la périostite, par exemple. Or j'ai vu des scrofuleux atteints d'iritis, qui n'avaient ni photophobie ni névralgie orbitaire, et se promenaient librement sans protéger leurs yeux. Une strumeuse affectée d'une périostite très-développée du tibia et du sternum, n'accusait aucune souffrance en ces points. J'ai interrogé plusieurs scrofuleux atteints de syphilis secondaire; ils n'avaient ni céphalalgie ni arthralgie, ni exagération quelconque de la sensibilité.

La tuberculose, si elle ne favorise pas l'apparition des lésions graves et rebelles de syphilis, les entretient à coup sur.

Trois fois au moins j'ai pratiqué la rectotomie linéaire palliative pour des rétrécissements syphilitiques du rectum chez des sujets atteints de tuberculisation (les rétrécissements tertiaires de rectum déterminent très-manifestement la phthisie chez les sujets prédisposés). Dans tous les cas la plaie est restée blafarde, a pris l'aspect des ulcérations syphilo-scrofuleuses, et n'a présenté aucune tendance franche à la cicatrisation.

Le traitement de l'hybridité scrofulo-syphilitique est aisé, les mêmes indications se présentant dans le deux diathèses. Cependant à la médication spécifique, mer-

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cure et iodure de potassium, on fera sagement de joindre des agents anti-scrofuleux proprement dits; huile de foie de morue, phosphate de chaux, iodure de fer, &c.

Hybridité syphilo-cancéreuse.—Les cas en sont rares et peu connus. Le néoplasme épithélioma ou cancer, dernier venu, subit l'influence de la syphilis préexistante. J'ai vu un sarcocèle atteindre, sans doute comme *locus minoris resistentiæ*, un testicule guéri de syphilome depuis deux ans. Je ne connais point d'exemple de syphilis postérieure à un néoplasme et l'ayant modifié.

L'association de la vérole et du cancer donne à certains néoplasmes ulcérés une physionomie équivoque qui rend le diagnostic parfois très-malaise. J'ai observé ces cas embarrassants à la langue, à la mamelle, à la vulve, à l'anus, et même en certains points du tégument externe.

On croit d'abord à l'accident tertiaire parce qu'on en constate les principaux caractères objectifs, parce que les symptômes sont relativement bénins, la marche lente, et l'indolence presque complète. Enfin parce que outre les aveux du patient qui reconnaît avoir été jadis infecté, on trouve parfois sur d'autres points du corps des manifestations syphilitiques non douteuses. J'ai vu un malade présentant, avec un syphilo-épithélioma lingual, une large syphilide pustulo-crustacée à la tempe.

Malheureusement on est ramené à l'idée du néoplasme par l'engorgement des ganglions, exceptionnel dans la syphilis ancienne, par les progrès continus du mal, par le généralisation d'autant plus fréquente que la marche est plus lente, par l'insuccès du traitement spécifique qui d'ordinaire aggrave même les symptômes, et enfin pas la terminaison inexorablement fatale.

Le caractère le plus curieux de l'hybridité syphilo-néoplastique est sans contredit l'absence de douleur. J'en ai vu à la langue et à la mamelle des exemples remarquables. Tout récemment un malheureux atteint d'épithélioma lingual très-étendu, avec adénapothie cervicale considérable s'éteignait dans mes salles sans avoir manifesté la moindre souffrance. Avec ce préjugé qui lie étroitement la douleur et le cancer, cette indolence égare fréquemment le praticien.

Si l'on peut prendre un cancer pour un syphilome ulcéré, l'erreur inverse est concevable, et je suis persuadé qu'on a opéré et cru guérir comme épithélioma plus d'un syphilome lingual ou ano-rectal. Trompé par une malade, j'ai été sur le point de commettre une erreur de ce genre.

Le pronostic est malheureusement toujours celui du néoplasme, et dans ce duo morbide le dernier mot reste toujours au cancer. La survie toutefois semble un peu plus longue.

J'ignore si la syphilis a le pouvoir de retarder la récidive en cas d'opération. Dans les cas douteux et pour fixer le diagnostic il faut toujours essayer le traitement spécifique, sauf à le cesser dès qu'il se montre inutile ou nuisible. Lorsqu'il y a réellement épithélioma ou cancer la médication antisyphilitique est naturellement impuissante et d'ordinaire mal tolérée ; cependant elle peut par exception amener une amélioration considérable, qui fait espérer une guérison prochaine. Ce résultat est d'autant plus surprenant qu'on sait combien le mercure et l'iodure de potassium ont peu d'effet en cas de néoplasmes malins ; aussi lorsque l'iodure de potassium administré banalement, modifie favorablement une tumeur sans la guérir, on doit penser à l'hybridité.

Un néoplasme compliqué de syphilis n'est point soustrait pour cela à l'action chirurgicale et peut-être operé, comme une autre; il est utile toutefois de n'agir qu'après avoir tiré parti des spécifiques dont rien d'ailleurs n'empêche de continuer l'emploi après l'opération.

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DISCUSSION.

Mr. Hutchinson, London: I have been requested, Mr. President, to make some remarks on the modifications which syphilis assumes in connection with the diathesis of its subject, whether gouty, tuberculous, or other. But, in truth, I know very little of such modifications, and have but a feeble belief in their occurrence. The most remarkable feature in syphilis appears to me to be its sameness, in spite of diathesis. You cannot tell beforehand who will have it severely and who lightly, any more than you can in the case of small-pox. That it does vary in severity is undoubted, but its differences have always appeared to me to be inexplicable; in other words, assignable only to what we mean to denote when we use the word "idiosyncracy." We see it occur with extreme severity in the most robust persons, whilst it is often very mild in the delicate, scrofulous, and cachectic. There is, so far as I know, no rule. It might have been assumed as probable that in the scrofulous a greater tendency to ulceration would be witnessed, and greater difficulty encountered in inducing inflamed tissues to return to the healthy state; and it may be that this is so to some extent, but not, I think, in any strongly marked degree. Mercury often agrees very well with the scrofulous, and is just as effectual in them as in others in curing syphilis. This leads me to remark, further, that what we call idiosyncracy in respect to the syphilitic virus is perhaps quite as frequently idiosyncracy in reference to its antidotes. The disease is often exceptionally severe because the patient bears mercury and the iodides badly. The idiosyncracy referred to, whether it concern the disease or its remedy, is often revealed in the earliest stages. The conduct of the chancre, its tendency to inflame, or otherwise, will give a clue to the whole course of the malady. If apparent exceptions to this occur, they will, I think, usually be explained by observing that mercury has definitely disagreed and induced a material modification in the patient's state of health. If we admit respecting the diathesis which our forefathers knew as "scrofulous" (not exactly the tuberculous one of the present day) that it is likely to predispose all inflammations to become lasting and to ulcerate, we may, I think, also admit, respecting the gouty diathesis, that it is likely, in all disturbances of general health, syphilis included, to make its subject more prone to bone and joint pains and to inflammation of certain structures in the eye than would otherwise have been the case. Although these are statements rather of what is probable than of the actual results of observation, yet I think I may say they are to some extent supported by the latter. Some of the worst cases of bone pains, during the secondary stage of syphilis, which I can call to mind, certainly did occur to those who were of the arthritic diathesis. If it be inquired respecting the tendency to phagedænic ulceration, whether occurring to the chancre, the secondary eruption, or the tertiary lesion, I am quite unable to associate it with any diathesis. I have seen it occur over and over again in those who had previously appeared to be most robust. When iritis happens from syphilis in an arthritic subject, I think it is more than usually likely to assume the relapsing form. All arthritic iritis, however, is in its very nature liable to relapse and to recur, and it is not without risk of erroneous inference in the cases referred to that we assume that the syphilis takes any share at all. In the remarks which I have made I have been thinking almost exclusively of acquired syphilis. Respecting the inherited form we may fully admit that it seems more probable that it should receive modifications from diathesis, seeing that the two are mixed together from the very beginnings of the individual existence. Even here, however, I suspect that we see but seldom any definite

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proof of mixed results. It is still very remarkable how separate the syphilitic tendencies appear to keep themselves. Syphilis may and does simulate a hundred other maladies, but its offspring are for the most part of pure breed, and show but little evidence of hybridism. They are still curable by specifics, and by these only; and if we have to suggest other measures in order to improve the general health, it is usually rather in order to enable the system to bear well the remedies than in any hope that such treatment will exercise a definite influence on the local lesion. In fact, throughout its course, whether acquired or inherited, syphilis for the most part remains syphilis. If there are any exceptions to this in the inherited form, they occur probably in the following instances, but in each it is my belief that the modification is usually but slight. The interstitial keratitis, formerly known as scrofulous corneitis, is usually the same disease in all variations of the health of its subjects, but it is possible that now and then in the strumous some tendency to ulceration may be witnessed, and also an unusual protraction of the attack. There is a form of ulceration which attacks the nose and other parts, and is often called "lupus," but which is wholly distinct from that disease, much more rapid in its progress, and curable by quite different measures. It is of syphilitic nature from beginning to end, but I am not sure that it may not acquire some of its peculiarity when it happens in the scrofulous. The subjects of inherited taint are also liable to joint affections—the knee especially being prone to synovitis, with effusion. The attacks often last long, but are in the end wholly recovered from. My impression is, that these joint affections are both more frequent and more severe in the children of the gouty. The coincidence of congenital syphilis with rachitis is no doubt very common, and has led to grave errors as regards their relationship. The truth probably is, that both affect the same tissues, and often mix the results, but not, I think, in a manner which we ought to find inextricable. Rachitis leads to defective ossification and to the enlargement of parts consisting of cartilage, or of soft bone, more especially of the epiphyses. Rarely does rickets alone cause acute inflammation, or undue suppuration. But if to rickets be added a taint of syphilis, then the inflammatory tendency is much excited, and cell deposits or suppuration may easily occur. Thus the complicated cases which have led M. Parrot and others to a conclusion which we must regard as a mistake are, I think, easily explained. The last remark which I have to make on this subject is, that I have seen a few cases in which gout seemed to mix itself with inherited syphilis in the production of persisting disease of the eye in gouty adults. In these cases the characteristic keratitis has sometimes passed away, and then relapses, with iritis, and possibly opacities in the vitreous, occur. In some there has been noticed an increase of tension, and the organ is in very serious peril. Whether in these cases the gout or the syphilis takes the larger share it is sometimes difficult to decide. The cases are themselves rare.

Professor E. H. Bennett, Dublin: There is a single point in relation to the modification of syphilis in tuberculous, gouty, and other constitutions to which I would wish to refer, as there is, so far as I know, no exact published statement in reference to it. Does syphilis produce any definite pathological change either in the articular surfaces or the intra-articular structures of the joints? While the pathological features characteristic of syphilis are permanently engraved or embossed on the surfaces of the bones, they strictly avoid encroaching on the joint surfaces; in subjects which exhibit them over every bone of the skeleton they

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constantly avoid the joints. In the scrofulous or tubercular diseases of joints the appearances are quite characteristic, just as they are in gout and chronic rheumatic arthritis in the skeleton of these and in the skeletons affected by rickets; we find syphilis develop its peculiar features on the bones unaltered in its own characters, and strictly avoiding the joints. No modifications of any of these diseases appear to be produced by their mutual presence in the same individual. I do not for a moment deny that syphilis affects the joints; clinically we are familiar with its joint pains, and the very common development of transient effusion into the synovial cavity in secondary syphilis, but I have failed hitherto to find any definite pathological change produced on the joint surfaces by this affection; syphilis appears rather to reserve its permanent pathological products, so far as the skeleton is concerned, for its extra-articular parts.

Dr. L. Henri Petit, Paris: I will first mention a case of syphilis co-existing with malaria. The patient was thirty-nine years of age, and had become so accustomed to the use of quinine, that he had recourse to the drug without consulting a doctor whenever the ague attacked him. In 1879 he found that the quinine had no effect in one of his attacks, which I attributed to the patient having becoming accustomed to the drug. A cathartic and an emetic were ordered, and a gramme of sulphate of quinine prescribed. All the viscera were healthy except the spleen, which was enlarged. In large doses, frequently administered, however, the quinine remained powerless, though the toxic effects of the drug were produced. A pustular eruption appeared on the lower limbs, which was the result of syphilis contracted by the patient three years previously. The quinine was accordingly discontinued, mercurial mixtures were instituted, under the influence of which the pustules rapidly healed, and the ague fits gradually diminished in intensity. Tonics of arsenic and quinine were then given, while the inunctions were continued, and the patient was soon well enough to leave for Italy. I saw him again two years afterwards; he had had several attacks of ague and cutaneous eruption, which he had kept in hand by having recourse to the prescriptions given him; but now his condition was much altered. Two months previously he had been seized by febrile attacks, unaccompanied by cutaneous eruptions, which had not yielded to quinine and mercury; he was pale, cachectic, emaciated, with an anxious expression of countenance, and bright eyes, and was suffering from continuous fever, with an occasional dry cough, but without expectoration. He had night sweatings, which he thought were the remains of his old malady. There was some dulness in the chest and diminution of the respiratory murmur, which was attributed to the existence of gummata. Iodide of potassium, however, afforded no relief. A consultation was held with Dr. Desmos of Paris, who diagnosed the existence of a cavity below the left clavicle. All treatment proved ineffectual, and the patient sank three months afterwards. In this case, whilst the malarial fever existed alone, quinine was effectual in producing a cure; but when the syphilis was superadded, although the former still caused the prominent symptoms, the latter evidently gave its characteristic features to the "duo pathologique," for it was only on exhibiting anti-syphilitic remedies that relief was obtained. The fever in syphilis is denied by many, while it is asserted by others, but an intermittent character has never been attributed to it. Probably in this case the syphilis awoke the malarial fever, which was in a dormant condition. And seeing that fever is undoubtedly present in some syphilitic individuals, and absent in others, may it not be that its presence is accounted for by the pre-

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existence of a febrile diathesis, malarial, arthritic, tuberculous, or what not. This appears to be a question well worthy of consideration. It is also worthy of remark that the fever no longer retained its intermittent character after the viscera were attacked. The actual condition of the lungs was never ascertained; but whether syphilitic or not, the treatment applied remained without effect; which is an illustration of the observation of M. Verneuil, on the inefficacy of therapeutic measures in advanced stages of hybrid conditions.

Dr. R. Drysdale, London: I do not altogether see my way to following M. Verneuil in his opinion as to the frequency of hybridity of scrofula, tuberculosis, gout, rheumatism, and syphilis. I have often been inclined to hold these views for a time when I have seen syphilis act severely on some case of tuberculosis or in rheumatic persons; but further experience has generally caused me to abandon this theory. Doubtless women of consumptive families sometimes suffer extremely from syphilitic ulceration, commencing in the throat and ending in the lower limbs; but persons who are quite healthy also suffer in the same way. There is, indeed, great difficulty in my experience in making a prognosis as to how any given case of syphilis will turn out. Persons in perfect health and with fine constitutions often suffer terribly, whilst delicate women often are scarcely at all damaged by the poison of syphilis. One point seems true to me, that it is dangerous to contract syphilis when in old age. M. Ricord's dictum, "Il ne fait pas bon de prendre la vérole dans la vieillesse," seems true. And, again, there is the question of sex. Women, in my experience, suffer more severely than men from syphilis. They have severe nervous diseases peculiar to them, anæsthesia, peculiar eruptions (pigmentary syphilides), and emaciation and cachexia, are also more common than in men. But neither rheumatism nor gout is, in my experience, liable to be complicated by syphilis. Syphilis, of course, gives rise to severe rheumatic pains and to headache, &c.

De l'Electrolyse dans le traitement de la Tumeur Blanche.

Dr. Aniello d'Ambrosio, Naples.

The author reviewed the past and present methods of treatment, pointing out the unsatisfactory nature of the results often obtained by them. The use of the method of igni-puncture, devised by Richet, led the author to search for a less severe method of obtaining a similar result, and with this idea he has employed electrolysis. After detailing the histories of three cases in which the method was successful, the author offered the following conclusions:—

1.—L'electrolyse, d'après tout ce que j'ai pu récueillir dans les ouvrages de chirurgie, et particulièrement dans les œuvres importantes d'Althaus et de Ciniselli, ne semble pas avoir été jusqu'à ce jour employée dans l'arthrite fongueuse, bien que dans Althaus on trouve qu'il en a été fait usage avec profit dans l'hydarthre donc les phases morbides diffèrent beaucoup de celles de la tumeur blanche.

2.—Un champ expérimental plus vaste est nécessaire afin de mieux apprécier ce traitement, peut être appelé à prendre une place importante dans la chirurgie opérative de la tumeur blanche.

3.—L'electrolyse, ainsi que je l'ai exposé, agit rapidement, et se fait mieux accepter des malades que le feu proprement dit. Son action physico-chimique et

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en même temps caustique produit les métamorphoses de réduction du tissu des granulations indiquées plus haut.

4.—Elle ne semble réussir dans la tumeur blanche qu'à la periode du granulôme dans sa forme clinique la plus classique, et lorsqu'il tend à un passage ultérieur.

5.—Le traitement réussit même quand au granulôme se joint la formation de petites nodosités blanches qu'il est permis de désigner sous le nom de tubercules (Koster)*. Le courant électrique en procurerait les résolutions.

On Apparatus for giving Anæsthetics with precision.

Mr. J. T. Clover, London.

The want of efficient means of quickly augmenting or diminishing the strength of the anæsthetic vapour has been from the first recognized by practical men. The gravity of the subject is apparent when one takes such an anæsthetic as chloroform, and observes the effects of breathing three, five, and ten per cent. of vapour. We find that there is a striking effect on the heart when the proportion rises above five per cent., and that if the proportion is increased, it stops the heart altogether before the respiration has ceased. In my practice I have found it impossible to continue the use of five per cent. of chloroform without causing serious failure of circulation, and in the records of fatal cases we often find that the patient had shown signs of recovery—that chloroform was poured on the cloth, and that within a minute of the application of the cloth to the patient's face he becomes pulseless. The vapour of the liquid poured on a cloth is given off unequally. At first it evaporates very freely, and the vapour, which is four times as heavy as air, does not mix easily with it, but flows in an invisible stream according to the laws of gravity. The patient, finding it strong, holds his breath, during which time it accumulates near his face, and when at last he is forced to breathe he may take in from five to fifteen per cent. of vapour. Fortunately if the vapour is above a certain strength it causes obstruction in the larynx, and so prevents the further supply of chloroform. I have seen the heart of a donkey stop suddenly after some chloroform was poured into the bellows used for artificial respiration, and no mechanical or electrical stimulation would make it contract. Careful experiments show by the curved line of the sphygmograph that the sudden increase of dose is apt to be followed by a dangerous arrest of heart movement. If any anæsthetic is continued too long, even though the dilution be sufficient, of course death will occur, but the respiration stops before the heart, and as we have the means of artificial respiration to meet such a difficulty, it is a far less dangerous event, and, in the hands of experienced men, may be said to be almost free from danger to life.

Dr. Snow's was a great improvement upon previous inhalers, in that he arranged a water-jacket to regulate the temperature of the chloroform, but the vapour becomes very much stronger whenever the patient pauses in breathing, also in long operations it requires replenishing, and then the vapour is given off too freely. Moreover, the stream of chloroformed air is not well mixed, and the valves are liable to get out of order.

My bag inhaler consists of a large bag holding 8,000 cubic inches, which is

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* Arthrite scrofulo-tuberculeuse d' altri scrittori.

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charged with air in the proportion of three to four minims in every 100 inches of air, from which the patient inhales, by means of a face-piece with double valves, so that expired air is not breathed twice over. It is found too cumbrous for general use, and other methods will supersede it. Dr. Junker's apparatus causes a stream of air to be passed through a bottle containing chloroform. But the objection to it is this :—At first the vapour is too strong, and when, by evaporation, the liquid has become cold, the vapour given off is of moderate strength, and very uniform so long as the stream is uniform ; but if the stream is stopped, the liquid rises in temperature, and the vapour is given off too abundantly as soon as the administration is renewed.

I have for the last three or four years adopted a plan of blowing a stream of air charged with vapour into a face-piece held near, not fitting closely to the patient, and I have found it most convenient to use a larger cylinder bellows, holding 2,000 inches. The weight of the top of the bellows is sufficient to keep up a gentle stream for about three minutes. As soon as the bellows have become nearly emptied, the top is raised again. The air passes from the bellows to the face-piece by three ways—firstly, directly ; secondly, by passing over the surface of liquid chloroform ; and thirdly, by passing over liquid ether. The three tubes of india-rubber which are thus charged are arranged one inside another, and near the face piece, where they terminate ; there are three levers like the hands of a watch, arranged against a dial plate, and by their position the quantity of chloroformed or etherized air which is passing is indicated. Connected with the face-piece also is an india-rubber bag, into which the larger portion of the expired air is breathed, whilst a smaller portion passes away between the face and face-piece. The vessels holding the chloroform and ether are surrounded by a large water-jacket furnished with a thermometer. In summer it is not necessary to warm the apparatus, but in winter it is desirable to heat the water as many degrees above 65 as the air of the room is below it—*e.g.*, if the air of the room is 60° the apparatus should be warmed up to 70°.

I now show an apparatus called the Down-draught Dilutor, which is new in principle, inasmuch as it utilizes the weight of the anæsthetic vapour as a moving power, and as a means of keeping up a uniform mixture of the anæsthetic atmosphere, according to the adjustment of the instrument.

I regret that it is not yet so complete that I can at once advise its adoption in practice. I have arranged one—but it is not quite completed—which is carried by the administrator, and requires no table or bed to support it. Until that is satisfactorily accomplished, the instrument will be considered too cumbersome.

It has the advantage of keeping up the supply of air and vapour without the use of a bellows.

The simplest form of this instrument consists of a syphon, the short arm of which is inserted into a bottle half filled with chloroform, the syphon reaching to within a line of the level of the liquid. Air being blown through the bottle, the long arm becomes filled with an atmosphere heavier than air, and a steady down-draught is established. The current and the strength of the mixture gradually diminish as the liquid chloroform becomes cold by evaporation.

After trying other plans, I prefer placing the chloroform in a copper vessel surrounded by a water-jacket. A brass tube, an inch in diameter and six inches long, leads from the centre of the vessel to the regulating stopcock, and a non-collapsing flexible tube leads onwards to the patient's face. The sectional area

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of this tube being greater than that of the stopcock, the heavy chloroformed air has a tendency to draw in air from an opening at the side, just as the light atmosphere inside a chimney whose narrowest place is near the fire will tend to draw in air through an opening like an Arnott's ventilator. By turning the regulator in one direction, the vapour mixture is supplied more and air less; and by turning it the other way, less of the mixture and more of the fresh air. Whilst the regulator is fixed, there is very little alteration perceived in the strength or quantity of the vapour, even after the current has been going on for several minutes. The liquid anæsthetic must be two feet above the patient. It can be either suspended from the top of the bed, or elevated on a pedestal, or placed on the shoulders like a knapsack. Of course the vapour loses in strength in proportion as the level of the liquid falls by evaporation, and the air-current is less close to the surface of the liquid. To prevent this source of inaccuracy, I have made the air pass beneath a cap which rests upon two copper air-cells. As this cap floats, the air-current is always of the same thickness. The stream of chloroformed air, together with the fresh air admitted to dilute it, is discharged from the india-rubber tube near and above the nostrils, through a perforated zinc plate. A towel or handkerchief placed so as to keep off any draught is all that is needed to make the instrument ready for use; but I find it convenient to use a face-piece with supplemental bag, which is soon filled with the mixed air and vapour. The face-piece should not fit close to the face, as that would stop the further supply of chloroformed air as soon as the bag was filled.

I have used this instrument less than fifty times, and from this limited experience I feel warranted in saying that it gives me a command over the anæsthetic which will enable us to obtain all we want from chloroform without danger of that worst kind of accident, cardiac syncope. I believe also that the more manageable kind of danger arising from laryngeal obstruction will be less likely to occur with so quick and reliable a means of altering the strength of vapour.

If I find that it is desirable, I will arrange the instrument so far as to give either chloroform or ether, as in the one last described.

I now show different instruments for keeping up the anæsthesia in operations in the mouth—especially a bag with a nasal tube adjusted and firmly fixed in the nostril by a screw movement; also funnel-shaped india-rubber tubes for conveying the anæsthetic to the back of the mouth during operations on the jaw.

I also show several face-pieces—(1) one of india-rubber for covering the nose and part of the mouth during extraction of teeth; (2) one of a similar shape with a frame of lead wire to make it fit over the nose; (3), an elongated conical face-piece cut obliquely at the bottom. The two latter being useful in giving room to the surgeon in eye cases.

De la Sonde Œsophagienne à demeure.

Dr. Krishaber, Paris.

Il est des cas de rétrécissement extrême de l'œsophage dans lesquels le chirurgien est obligé de renoncer au cathétérisme graduel. S'il parvient, après bien des difficultés, à introduire une bougie ou une sonde, il prévoit avec appréhension la nécessité de la replacer le lendemain. Cette situation assez embarrassante en face d'un rétrécissement cicatriciel, devient tout-à-fait redoubtable lorsque la sténose

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est due à une tumeur de l'œsophage. Il arrive bien souvent, en effet, que la sonde s'égare au milieu des tissus morbides, et qu'une perforation se produit, donnant lieu aux accidents rapidement mortels du pneumothorax. En outre, la tumeur peut être touchée de telle sorte qu'un fragment soit repoussé au devant de l'extrémité de la sonde qui se crée elle-même un obstacle insurmontable. L'opérateur se trouve alors placé dans cette alternative de pratiquer, soit l'œsophagotomie, soit la gastrotomie, qui, en raison du mauvais état général du malade, ne présentent presque aucune chance de succès ; ou bien de renoncer à toute tentative, ce qui équivaudrait à une condamnation à mort par inanition.

Depuis un temps relativement court, je me suis trouvé cinq fois en présence de rétrécissements œsophagiens tellement serrés que, ne voulant plus, après quelques séances de cathétérisme répété, exposer mes malades aux dangers du cathétérisme répété, et prévoyant du reste, à courte échéancé, l'impossibilité de le continuer, je résolus de le pratiquer une dernière fois et de laisser la sonde à demeure. Il est important de dire que voulant ainsi fixer la sonde pour un temps indéfini, je me garde bien de la passer par la bouche, le maintien prolongé d'un corps étranger dans cette cavité devant nécessairement amener des complications sérieuses. J'introduis donc la sonde par l'une des narines : dès qu'elle est parvenue dans l'estomac, je la fixe et je ferme son extrémité libre. Dans mes deux premiers cas, je fixai la sonde en plantant une aiguille à quelques centimètres de l'extrémité libre, l'aiguille ainsi placée figurant la branche horizontale d'une croix dont la sonde formait la branche verticale. Autour de l'aiguille est enroulée une bande que j'attache au front de mes malades. Dans les cas où il devient impossible d'introduire une sonde molle de caoutchouc, la fermeture de l'extrémité libre de la sonde se pratique par un système d'emboîtement de bouchons en os que j'ai fait faire chez Charrière-Collin. Un système analogue peut être appliqué aux sondes en gomme. Cette dernière n'en reste pas moins plus difficile à fixer que celle en caoutchouc, dont l'extrémité munie de la fermeture en question disparaît, pour ainsi dire, dans la narine. Dès lors, l'alimentation se fait sans difficulté ; à la sonde on adapte le bout d'un irrigateur et cinq ou six fois par jour on injecte du lait, des œufs crus et même de la viande hachée et délayée dans du bouillon ou dans du vin ; les médicaments sont administrés par le même procédé. Un fait spécial mérite d'être noté : lorsque la quantité d'aliments injectés en une fois excède la mesure, les malades en rejettent une partie par la sonde et paraissent soulagés.

La sonde œsophagienne laissée à demeure pendant 305 *jours.*

Observation I.—X., âgée de 50 ans, atteinte d'une tumeur maligne de l'entrée de l'œsophage et du larynx. Cette femme présente un rétrécissement tellement serré et chaque cathétérisme est si mal supporté, qu'après deux ou trois séances, je renonce à le pratiquer et je place une sonde à demeure (le 2 Juin, 1879).

Dans les premiers jours qui suivirent l'introduction de l'instrument, il survint un certain degré de coryza avec rougeur du nez et douleur s'irradiant vers la tête ; mais peu à peu la tolérance fut obtenue et bientôt la malade n'accusa plus de gêne. L'alimentation se fit normalement et sans encombre. Il est à peine utile d'ajouter que la tumeur cancéreuse qui avait nécessité le cathétérisme, suivit sa marche progressive. La malade succomba 306 jours après la mise en place de sa sonde sans que celle-ci fut jamais déplacée.

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La sonde œsophagienne est restée à demeure quarante-six jours.

Observation II.—X., âgée de 55 ans, atteint de goître suffocant, pour leque j'avais pratiqué, quelque temps auparavant, la laryngotomie inter-crico-thyroïdienne au dessus de la tumeur du cou qui eût rendu la trachéotomie presque impossible. Le goître s'étant enflammé et abcédé, l'œsophage se trouvait comprimé en même temps que la trachée, et le passage des aliments solides et liquides fut intercepté. C'est dans ces conditions que je parvins, après bien des difficultés, à franchir l'obstacle œsophagien avec une sonde mince mais suffisante toutefois pour permettre l'injection d'aliments délayés. La sonde avait été introduite par l'une des narines et ne fut plus retirée. Cet état de choses dura quarante-six jours. La tumeur s'abcéda et s'ouvrit ; le quarante-cinquième jour, il survint un frisson précurseur de l'infection purulente, et le malade succomba le lendemain.

A l'autopsie, je pus constater l'existence de deux tumeurs parties du corps thyroïde ; l'une, antérieure, comprimait la trachée ; la seconde, postérieure, aplatissait l'œsophage contre la colonne vertébrale. Il est évident que dans ce cas la mort fut arrivée par inanition dès les premiers jours qui suivirent la laryngotomie sans le secours de la sonde œsophagienne à demeure.

La sonde œsophagienne est placée le 17 *Février*, 1881, *est encore à demeure lors de la lecture de ce mémoire, le* 3 *Août*, 1881, *soit depuis* 167 *jours.*

Observation III.—M., 68 ans, marchand de charbon, est atteint depuis dix-huit mois d'un cancer de l'œsophage se révélant par une dysphagie qui n'a fait que s'accroître malgré les traitements les plus divers. En Décembre 1880, il vient me consulter pour la première fois. La déglutition des aliments solides est impossible depuis plusieurs mois déjà ; les aliments liquides mêmes passent avec la plus grande difficulté. Je pratique, séance tenante, le cathétérisme de l'œsophage à l'aide d'une sonde ordinaire dont l'introduction est trés-difficile et très-douloureuse. Pendant deux mois environ, le cathétérisme fut répété deux fois par jour à l'aide d'une sonde en gomme, de façon à permettre l'alimentation. Chaque séance est accompagnée de douleurs extrêmement vives ; le malade rejette des mucosités teintes de sang, et ce n'est qu'après bien des tâtonnements qu'on parvient à franchir l'obstacle. Malgré ces cathétérismes répétés, la coarctation ne fait qu'augmenter ; enfin, le 2 Mars, je me décide à laisser une sonde à demeure. A partir de ce moment, le malade jouit d'un bien-être relatif, son état général, très-mauvais, semble s'améliorer pendant quelque temps. Malgré le petit calibre de la sonde introduite, on parvient à faire absorber au malade, par jour, deux litres de lait et six œufs crus délayés dans ce liquide. Le 15 Mai, dans la nuit, M., est pris de délire et rétire sa sonde. Appelé le lendemain, je ne pus la réintroduire qu'avec beaucoup de peine.

A partir de ce jour l'état général du malade devient de plus en plus mauvais ; une salivation très-abondante se montre, des crachats de sang pur sont rendus par expuition, la face prend une teinte pâle, indice d'une cachexie avancée. Néanmoins le 3 Août, 1881, c'est-a-dire, seize jours après l'introduction de la sonde à demeure, le malade vit toujours et semble devoir résister encore quelque temps.

La sonde œsophagienne placée le 31 *Mars*, 1881, *est encore à demeure, lors de la lecture de ce mémoire, le* 3 *Août*, 1881, *soit depuis* 126 *jours.*

Observation IV.—D., 51 ans, huissier, éprouve, depuis un an environ, une gêne sans cesse croissante de la déglutition, en même temps que des douleurs assez vives à la partie antérieure du cou. Lorsqu'il vint me consulter pour la première fois, en

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Mars, 1881, les aliments solides ne pouvaient être déglutis, les liquides seuls passerent avec peine. Le cathétérisme de l'œsophage permet de constater un obstacle siégeant à 22 centimètres de l'orifice supérieur de l'œsophage. Des olives d'une très-petit calibre pouvaient seules être introduites; après le troisième cathétérisme, le malade refusait de se soumettre encore, tant cette opération lui était pénible.

Le rétrécissement ne faisant que s'accentuer de jour en jour, je me décide le 31 Mars, à introduire une sonde en gomme par l'une des narines, et à la laisser à demeure. Le 19 Avril, je la rétire pour la remplacer par une autre de calibre plus considérable, et je constate qu'elle n'est altéré dans aucun point. Le 17 Mai je substitue à la sonde en gomme une sonde en caoutchouc rouge, d'un centimètre et demi de diamètre; cette sonde n'est plus changée. L'alimentation se fait d'une manière régulière; le malade, dont l'état général est bon, vaque à ses occupations et ne se plaint que de sa salivation abondante, et de la fétidité de son haleine Il y a aujourd'hui 126 jours que la première sonde à demeure a été introduite.

La nutrition peut se faire à côté de la sonde. Il arrive dans ce cas, ce qui arrive pour l'urètre lorsque la sonde reste à demeure, la micturition peut s'effectuer à côté d'elle; chez mes malades, de même, les aliments peuvent passer à côté de la sonde, et grâce à cette circonstance, ils peuvent avaler des liquides par la bouche, tandis qu'avant le cathétérisme les liquides mêmes ne passaient pas.

Dans les cas de rétrécissement extrême de l'œsophage, alors que l'œsophagotomie est devenue indispensable, la sonde à demeure trouve encore son application, car le point rétréci de l'œsophage une fois incisé, la sonde peut maintenir l'œsophage dilaté, et permettre que la cicatrisation s'effectue sans nouvelle coarctation.

Indépendamment des cas de rétrécissement de l'œsophage, l'introduction des sondes œsophagiennes à demeure peut rendre de grands services dans les opérations qui se pratiquent sur les lèvres, le voile du palais et les parois de la cavité pharyngo-nasale, alors que pour le succès de l'opération il importe d'éviter au patient les mouvements de déglutition, et qu'il n'est pas moins nécessaire d'assurer une alimentation suffisante, très-difficile quelquefois à obtenir après ces opérations-

L'alimentation au moyen de a sonde œsophagienne a, d'autre part, l'avantage d'éviter le passage des aliments sur la plaie, avantage considérable, surtout à la suite de l'ablation de la mâchoire ou de l'amputation de la langue qui occasionnent presque toujours un œdème inflammatoire avec dysphagie.

Il sera nécessaire de placer la sonde quelque temps avant l'opération lorsqu'il s'agit de staphyloraphie ou d'uranoplastie, parce que sa présence produit un coryza passager qui, pendant plusieurs jours, provoque des éternuements et des secousses qu'il faut éviter après ces opérations. Comme resultat de ces quatre observation. j'offre les conclusions suivantes.

1.—L'œsophage tolère indéfiniment une sonde à demeure.

2.—Cette sonde doit être introduite par l'une des narines, et non par la bouche.

3.—La présence permanente de la sonde amène la dilatation du rétrécissement au point de rendre possible l'introduction de sondes de plus en plus grosses exactement comme cela se pratique pour l'urèthre.

4.—La présence prolongée d'une sonde en gomme permet ensuite l'introduction d'une sonde en caoutchouc rouge plus molle, plus flexible, et plus facile à supporter.

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5.—L'alimentation est absolument assurée et l'on évite les fausses routes du cathétérisme intermittent.

6.—La sonde œsophagienne à demeure est d'une incontestable utilité pour les grandes opérations qui se pratiquent sur la langue, la face, la bouche, et la cavité pharyngo-nasale.

7.—Dans ces derniers cas, la sonde œsophagienne sera placée plusieurs jours avant l'opération afin d'en assurer la tolérance pendant et après l'opération.

8.—Dans les opérations alors qu'il n'existe pas de rétrécissement, la sonde en gomme est inutile; on peut d'emblée se servir d'une sonde on caoutchouc.

Double Irrigation and Drainage Tubes.

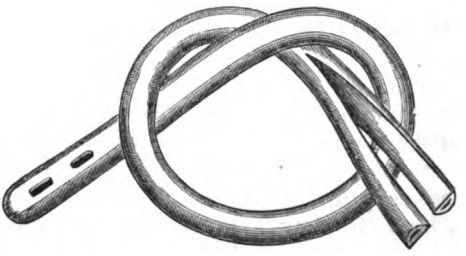
Dr. Henry O. Marcy, Boston, Mass.

There is no novelty in double tubes, the value of which has long been appreciated by the profession, but so far as has come under my observation, they have hitherto been constructed of metal or other stiff material, and on this account their utility has been greatly lessened. Since the manufacturing of rubber in glass moulds, which gives such perfection of finish and durability to its surface, the rubber catheter has in a large measure, for most excellent reasons, supplanted the use of the older as well as more expensive instruments.

It seemed to me possible to have made a tube of various dimensions, double through its entire length and of equal beauty of finish. After experimentation in this direction, Mr. Lockwood, of the Davidson's Rubber Co., of Boston, has succeeded in preparing for me the specimens here shown. These tubes vary in diameter, French scale, Nos. 18, 22, 25, 27, 35, 40, and in length from fourteen to twenty-four inches.

The name suggested has been "double irrigation, injection, and drainage tubes." The thought which more especially prompted their usage was to obtain in any cavity, not only a thorough cleansing from its contents, but the local application of any therapeutic agent; and especially to give to its surfaces the benefit attained by the secondary effects of heat and moisture. In the diseases of the bladder, the No. 18 is sufficiently small to traverse the urethra unless con-

Fig. 42.



Double Current Rubber Catheter.

stricted by disease. Even with a considerably enlarged prostate it enters quite easily, and the benefit of the continuous currents of hot water is early pronounced. The relief comes not only from the washing out of mucus and decomposing urine, but continued, as has been my habit for one half hour or more, night and morning, at the temperature of about 100°, the congestive thickening and vascular

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supply of the mucous membrane are greatly diminished, the prostatic irritability largely held in control, and permanent improvement ensues.

In vesical hæmorrhage from ulceration or other process the results are even of greater value. In acute as well as in chronic cystitis my results have been very gratifying.

In the very large class of cases where the uterus has become the receptacle of septic and decomposing materials, the double tubes have proved of very great value. After delivery I have found the larger sizes the most advantageous and convenient. Their introduction as a rule is easier, and the débris washes away with greater readiness.

In strictures of the rectum or inflammatory conditions of the pelvic organs the relief and benefit of the continued rectal douche has been very marked.

In these cases the tube enters the colon, and the hot water is syphoned in without strain upon the intestinal wall at a point above the constricted or inflamed part. Not only is the large intestine emptied of its gas and contents, and the tenesmus and pain removed, but the prolonged effect of the continuous current is very efficacious in reducing the congestive and inflammatory processes.

In nutritive enemas I have felt that a very considerable gain has followed by the introduction of the fluids thus high in the transverse colon, and can but recommend their use for further trial.

The larger and longer tubes are of easy application for the evacuation of the stomach. Said a professional critic, "They are not introduced, they are swallowed." Certainly almost equally easily do they enter the stomach, in marked contrast with the ordinary stiff tube of the stomach-pump. A tube of this kind and an ordinary rubber syringe are all that are required for the speedy and effective emptying of the stomach; artificial feeding is by this means easily accomplished. This I have had recourse to in a number of instances, with children as well as adults. For the secondary effects, the syphon is the simplest and most convenient method from a height sufficient to ensure a slow but continuous stream.

It is usually well to lengthen the efferent end, or both, by connections with rubber tubing, as thought most convenient.

A Case of Prostatotomy, in which a Silver Tube was worn in the Perineum for fifteen months, with great benefit.

Mr. E. Lund, Manchester.

Any attempts to overcome the painful consequences of enlargement of the prostate gland and the inability to void urine so generally attendant upon that condition are very rare, and especially that method which I am about to explain, which is purely mechanical in its principle.

The case which I have to relate is that of a gentleman, W. D., who first came to consult me on the 8th of August, 1875; and the history he gave me was somewhat to the following effect: He stated that he was unmarried, and up to the last four years had enjoyed good health. He was engaged in business, chiefly connected with speculations in the cotton market in Liverpool and Manchester. When he first began to suffer from a difficulty in passing urine, he went to see Sir Henry Thompson, and this was about four years before coming to me. Sir Henry examined him, and said he had no stricture, but only early prostatic disease. He

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had also been a patient of Mr. Southam, of Manchester, and about eight months previously to my seeing him, a catheter was passed into the bladder. A flow of blood followed the first use of the instrument, and this recurred on two subsequent occasions, but he has never passed any grit or sand, or had any of those special symptoms indicative of the presence of stone in the bladder. He has also been under the care of Dr. William Roberts, of Manchester; and whilst at Matlock, in Derbyshire, he was under treatment by Dr. Hunter. After this, he went to Mr. Gamgee, of Birmingham, and now he is brought to me by Dr. Rains, of Chorlton-cum-Hardy, for my opinion and advice.

The patient told me he could hold his water for four, and very often six, hours, in the night; but in the day time, whilst walking, the irritability of the bladder was very great, and the calls to pass urine most urgent and unbearable.

He was of a very costive habit; he had a somewhat sallow, unhealthy look, and admitted that in early life he had had gonorrhœa on more than one occasion. The urine, when I examined him, was faintly acid, almost approaching alkaline in its reaction. No albumen was detectable, or, if so, only the faintest trace of it. He told me he had been moderately temperate, and certainly never, habitually, a spirit-drinker.

I passed an ordinary silver catheter with prostatic curve, for he told me he was in the habit of using an elastic catheter himself, but that very frequently he had difficulty in its introduction, and this I ascertained was due to the clumsy way in which he handled it, driving it straight down towards the perineum, or lower floor of the urethra, where in front of the prostate he had made a pouch-like dilatation.

By the rectum I did not discover any very great enlargement of the prostate; there was general fulness, and it was somewhat painful on pressure. I advised him to refrain from passing the catheter, except when threatened with overflow from accumulation of urine, and to do this only very occasionally, so as to test his capacity for emptying the bladder.

I prescribed a mixture of dilute nitric acid, sulphate of magnesia, and quinine, with the object of securing as empty a state as possible of the venous plexuses around the rectum and neck of bladder.

I heard nothing of him for about a month after this date, and then at the urgent request of his friends, who were respectably connected, he was made inpatient of the Manchester Royal Infirmary in one of my wards. Then it was that I discovered how much he suffered from the condition of the bladder, which I have endeavoured to explain, for I have recently received a letter from Mr. Charles Smith, of Stretford, who was at that time house-surgeon at the infirmary, and who says he has indelibly impressed upon his memory the extreme amount of trouble which this gentleman gave him by his great irritability, and the intensely agonizing pain which he seemed to experience in his attempts to void his urine, or at the time of passing the catheter. In fact it is almost impossible to describe in words the agony he was in on these occasions; so much so, that taking into account his somewhat cachectic condition, I could not disabuse my mind entirely of the idea that, possibly, we had in this case to deal with some malignant growth deeply placed within the pelvis, and interfering with the functions of the bladder. Yet it was very rarely that blood was found with the urine, and then only when it could be traced to the passage of the catheter.

The neurosis of which he was the subject was almost the most intense that I had ever seen, it yielded to the continued use of opiates, but so soon as these were

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suspended, the agonizing pain returned, yet it never had the character of the pain commonly attendant on the presence of stone in the bladder.

Soon after his admission to the infirmary, I placed him under chloroform and sounded the bladder. There seemed to be a rough gritty condition of its coats, but the most careful examination did not show any loose calculus within it.

The distress which he experienced and the screams he uttered when the pain came on were so great as to disturb the patients not only in the same ward, but in distant wards of the hospital; and he had not been with us many days before I received an urgent message from the medical side of the building, saying that they could, even at that distance, hear his screams and cries during the night.

After due consideration, therefore, I felt it right to propose to him the operation of cystotomy, or it might be said of lithotomy, with the chance of discovering some loose calculous matter in the bladder. To this he gladly assented, saying that his life was intolerable, so he would submit to any chance of improvement.

Accordingly, in the first week of October, 1875, I performed exactly such an operation as I should have done for the removal of stone by lateral lithotomy. There was no difficulty in it, except that in piercing the prostate gland, I felt the knife passing through a layer of gritty matter. Introducing my finger in the usual way, it seemed as if all around the neck of the bladder nearly as far as the finger could reach, phosphatic deposit had taken place, giving to the mucous membrane a rough and scabrous feel.

I syringed the bladder with warm water, and afterwards with a very dilute solution of nitric acid; then I placed the usual lithotomy tube in the wound and fixed it in the ordinary way. I also succeeded in scraping off a small quantity of the phosphatic deposit. He was put to bed, and after the effects of the anæsthetic had passed off, he expressed himself greatly relieved. The urine flowed through the tube very copiously, and he appeared to have little or no pain.

On the third day, when I thought I had allowed the tube to remain as long as was prudent, I removed it; the end was slightly covered with phosphatic deposit, and it was my intention not to have re-introduced it, but at the suggestion of the patient himself, who dreaded a return of his old symptoms, I cleansed it with a weak solution of phosphoric acid, and re-inserted it. It was then left in for four or five days, and the same proceeding repeated each time. I began to think it would be right to try whether on the removal of the tube a better state of things might not result. But while the patient hesitated, I consented to its continued use, and at the end of nearly a fortnight when it became very difficult to re-introduce the tube owing to the contraction of the wound, I had made a curved silver tube very similar to the one I now show you. I had two such tubes made, and I am not certain whether this is No. 1 or No. 2. You will see that it is about 3¾ inches long, and has a curve corresponding to the segment of a circle 28 inches in diameter. One end is made round and smooth, and to the other is attached a shield in which are two small slit-like openings for tapes, &c., with which to fix the tube in the perineum. It answered admirably; urine at first flowed, not only through the tube, but by the side of it, by reason of the size of the wound. In a few days, however, the prostatic tissue itself contracted so closely round the tube that all the urine escaped through the interior of the tube.

I endeavoured to get the tube exactly of such a length that, when the shield pressed flat upon the perineum, not more than half an inch of the tube should

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project into the bladder. This I arranged by withdrawing and replacing the tube, at different lengths, by way of experiment.

Before six weeks were over, he was able to sit up and move about the ward, and then I noticed this fact: that, in spite of all my efforts to keep the tube with the concavity of its curve towards the pubes and the convexity towards the rectum, it would turn round in an opposite direction, so that, after many attempts to fix it, I came to the conclusion that the best way was to allow the tube to be so directed, the point of it dipping into any pool of urine which might exist above and behind the prostate.

He left the infirmary in December, 1875, and passed under the care and observation of Mr. George Stevenson, of Stretford, who has recently written me a note, saying that, to the best of his recollection, the tube was worn by this patient from December, 1875, to April, 1877, when, unfortunately, it was removed, and either allowed to remain out too long, or there was some difficulty in its re-introduction. In the course of a few days the sinus closed so rapidly that the instrument could not be introduced, for I ought to have said that during the whole of this time (fifteen months) he could, besides drawing off the greater quantity of his water by the tube, expel some occasionally in the natural way, *per urethram.* When, at last, by the unfortunate occurrence I have mentioned, the exit of the urine by artificial means could no longer be maintained, he was thrown back into his old condition, and he died rather suddenly, with all the symptoms of acute uræmia.

I have a note of his coming to see me on the 27th of October, 1876, about one year after the operation, and the following is the memorandum I then made:—"He comes to-day, at 3.45 P.M.; he has not passed urine since 11.45 A.M., and he draws off six fluid ounces by the tube with great ease, and it is just neutral in its reaction, becoming opaque on boiling, but clearing with nitric acid."

I also heard from him that he had been able to come to business, and attend the Exchange, very rarely suffering any inconvenience from the presence of the tube, so long as it was allowed to roll over, with its convexity to the pubes, rather than being kept fixed by the tapes in the opposite direction. I was much struck with the fact that no urine escaped by the side of the tube, and I can only account for it by assuming that the somewhat elastic tissue of the prostate gland gripped or pinched the tube as an elastic collar.

There was some difficulty in getting a suitable plug for the tube, but, after trying many substances, a small piece of boxwood, made nearly cylindrical as a very long cone, carefully fitted to the orifice of it, was the most successful. It is hardly necessary for me to remark upon this case, as the general history of it is so simple and intelligible. It must be regarded as an operation of expediency, and doubtless only applicable to a few rare cases.

The means employed for relief in confirmed obstructive disease of the prostate gland have thus far not been very encouraging. For one case where the bladder has been tapped above the pubes, and the cannula kept permanently in the line of puncture, nine cases have been followed with unfavourable results, and the same I believe may be said of other operations performed through the perineum by the side of the urethra with a similar object.

But here, fortunately, the healing process of the lithotomy wound went on so slowly, yet so completely, that the presence of the metallic tube did not bring about any dangerous irritation.

I think we shall all agree that, in prostatic obstruction, it is not always easy to

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determine what part of the prostate gland is most at fault, for we find cases in which, by a rectal examination, it is evident that the whole volume of the prostate is enlarged, and yet micturition is not seriously obstructed, and others where the actual size of the gland is not much beyond the normal standard, and great difficulty exists in the passing of the urine.

And, again, there is another form of disease of the prostate which seems hardly to be due so much to deformity as to some peculiar nervous condition where the continued feeling of weight in the perineum, the aching pain, after voiding urine, the irritable state of the bladder, by which the act has to be incessantly repeated, are so confirmed, and so little referable to mechanical causes, that we must consider there is either a permanent congested state of the gland itself, or the disease is from first to last true neurosis.

I think that this method of prostatotomy, with the after use of a tube, could only be attempted in cases where there is enlargement of the prostate gland, for it would be less likely in such cases that the irritation of the sinus should be propagated to the surrounding parts, than if the gland were small, for then the surrounding plexuses would come more closely into contact with the tube, and ulceration might result, for the tube is a foreign body lying in close contact with the highly sensitive surface of the urethral canal.

I have thus placed on record this case, unique in its character and interesting and encouraging in its result, with the hope that, should a similar case occur to any surgeon now present, he will not hesitate to give his patient the chance of benefit from a course of procedure so simple in its nature, and so likely to be followed by temporary, if not permanent, benefit.

On the Results obtained from the Excision of the Primary Syphilitic Sore.

Dr. Louis Jullien, Paris.

Is it possible to suppress or modify the syphilitic poison by acting in some way or another on the primary lesion, the chancre? Such is the question which is at present occupying the minds of syphilographers. By many writers, such as Auspitz, Kölliker, Folinea, and Unna, it is considered as answered in the affirmative, many are still doubting, while others, guided by theoretical ideas, object to such attempts, and will not even discuss the question, which they regard as already answered in the negative.

The author of these few remarks, although formerly on the side of those who regarded the suppressive treatment of the primary sore as useless, has now somewhat modified his views, and wishes to make his *confrères* acquainted with the results he has obtained from the excision of the primary sore.

Six patients submitted to the operation, but the cases of five alone have been completely followed out. Among these five cases, one alone has remained absolutely free from all syphilitic manifestations. Nine months have now elapsed since the first appearance of the primitive sore, and no secondary accident has shown itself. The chancre was small and evidently syphilitic, and on the nineteenth day was thoroughly extirpated by excision. The wound at first had an unhealthy aspect, but soon was covered with healthy granulations. The inflammatory induration which followed the operation soon disappeared.

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Scarcely any traces are now to be discovered, and the patient is in perfect health. This, then, may be called the abortion of syphilis, or, at any rate, of its manifestations. One objection only can be brought forward. Was the chancre syphilitic or not? As regards this point I am able to call Dr. Besnier, of St. Louis, as a witness. He saw the patient and made an examination of the piece that was removed, and without hesitation pronounced it to be a syphilitic chancre.

In two other cases the excision was performed on the twentieth and fifteenth day respectively, and in each case was followed by general syphilitic symptoms. The syphilides, however, were indecisive, and the other symptoms were either *nil* or of the mildest possible character. Such results might be considered as favourable to the new method of treatment. I know, however, too well how common it is for the primary sore to be followed by mild secondary symptoms. These two cases, however, scarcely differ from the most ordinary cases, and there are no distinguishing peculiarities. Nothing proves that they were modified by my treatment, and I therefore prefer to exclude them from the list.

It is not so, however, with the two other cases I am now going to recount. After the operation, each of them showed a minimum of infection as far as the secondary symptoms are concerned. To these two cases I therefore call the particular attention of my colleagues. First, Mr. D. was operated upon for a preputial chancre five days after its first appearance. The wound healed by first intention. On the twenty-sixth day cephalalgia appeared, along with a few papulæ on the abdomen and three herpetiform spots on the dorsum of the penis. At the same time there was swelling of the tonsils along with cervical poly-adenitis. No other accident appeared until the 1st of August, the hundred-and-ninetieth day of the infection. Secondly, I excised from Mr. M. two preputial chancres. The cicatrization took place rather slowly. On the hundred-and-third day there was cephalalgia, slight ganglionary intumescence, and cervical poly-adenitis. No other symptom appeared until the two-hundred-and-fifth day of the infection.

A syphilis developing itself in this way is so unusual that the reproduction of the same signs, in consequence of the same treatment, must undoubtedly imply something more than a mere coincidence. For my part, I must confess that I have not seen such a form of syphilis, in which, while there was no affection of the skin or mucous surfaces, the lymphatic system alone testified to the presence of the syphilitic poison. I have thus every reason to believe that the excision of the primary sore must account for this modification in the symptoms. I do not think, therefore, that I overstep the bounds of fair inductive reasoning if I call this a case of syphilitic abortion. If, then, these two patients remain free from all further accidents, as I consider they most certainly will, they will no longer be exposed to syphilitic inoculation, and may be actually said to have been vaccinated. This is the point I wished to arrive at, and I desire to point out what an hypothesis it suggests and what a future may be in store for us.

If we do not yet know the means of diminishing the severity of the syphilitic virus, as Pasteur has done with the malignant pustule, it seems probable that we may be able to graduate its effects, by allowing it to develop for a longer or shorter period at the seat of the inoculation. An enfeebled virus may thus be developed, and so act as a preservative against further infection.

I confine myself to these conjectures, as I do not think it would be prudent, on account of the limited number of my cases and my time of observation to develop them further. I conclude, however, by asking my colleagues to consider carefully the following conclusions:—

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1.—The excision of the chancre causes no local disorder and hastens the disappearance of the ulceration.

2.—Under certain circumstances, with which we are not yet acquainted, it suppresses all manifestations of syphilis.

3.—Under other conditions it diminshes to such a degree the effects of the poison that it seems to give the patient the benefit of a real vaccination.

Differences of Opinion as to the Nature of the Primary Lesions of Syphilis existing in London and Paris.

Dr. Charles R. Drysdale, London.

Notwithstanding the many and important discoveries that have been recently made in pathology, it is very remarkable that in the domain of syphilis there still exist a great number of points wherein a good deal of dissonance is heard between the opinions of various authorities in London and Paris.

At the very threshold of the subject, indeed, that divergence of opinion is noticeable; for, whilst a certain number of leaders of medical opinion in London are in favour of the view that there exists some close relationship between the two sores which are seen so commonly on the generative organs, others, no less well instructed in the matter, in Paris, allege that there is not the least tie of causation between the two.

I unhesitatingly range myself among the ranks of those who believe that the soft sore is in no way related to syphilis; and think that, whatever be the truth in this matter, it is high time that the question were fairly discussed and, if possible, set at rest. A meeting of medical men from all parts of the civilized globe is, naturally, the fittest place to have such questions heard and decided upon.

Since the able work written by M. Bassereau, of Paris, became known to readers of syphilography, it has been recognized that persons who have contracted true syphilis, either in the form of the primary lesion or, in case that is not visible, of some secondary lesion, have received the contagion from a person suffering either from primary or secondary syphilis.

M. Bassereau took the trouble to confront the patient with the person to whom his disease was due, and always noted that the soft sore was derived from a person with soft sore, whilst syphilitic, or so-called hard sores, were derived from patients who were evidently syphilitic. He also alleged, with a great deal of plausibility, that the simple chancre was an affection that had existed as long as there were any records of the healing art, and in all states, whereas syphilis—and I have no doubt of this latter allegation—had never been known among the Greeks or Romans, or indeed prior to the fifteenth century of our era.

Historically, those who are familiar with this question will remember that, one by one, the able writers who composed the school of the Southern Hospital of Paris (Hôpital du Midi), in company with the illustrious leader of the school, M. Ricord, assented to this view promulgated by Dr. Bassereau, and, in numerous publications, carried on the theory of so-called dualism of the chancre to ts present perfection.

In London we have had, as partisans of the dualistic theory, Mr. Henry Lee, Mr. De Meric, Mr. B. Hill, and several other able writers; whilst, on the other

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hand, we must cite among its opponents Mr. Jonathan Hutchinson, Mr. James Lane, and probably also Sir James Paget and the army surgeons.

In Paris, with one or two rare exceptions, the whole of the writers on syphilis have ranged themselves on the side of dualism. Such are MM. A. Fournier, Diday, Rollet, Mauriac, Verneuil, and others; in short, we may say that in Paris the question now has reached the peaceful stage of a settled scientific dogma, and that it is no longer discussed.

In London, however, at the well-known discussion which took place some years ago in the Pathological Society, a gentleman whose transcendent abilities have placed him at the very summit of fame in such inquiries made use of the expression, "Dualism is dead." Mr. Hutchinson by this meant, I believe, to urge that the soft sore was merely one of the ways in which syphilis affected the system, and referred to the experiments of the late respected Professor Boeck, of Christiania, who alleged that, by the irritation of so-called hard sores, soft sores could be produced.

But let us see what objections have been made to the unity view of the two sores. In the first place, then, Dr. Danielssen, of Christiania, many years ago had an idea that, by inoculating frequently patients with leprosy with soft sore pus, that incurable disease might perhaps be benefited. He consequently made a series of experiments similar to those of his colleague, Professor Boeck, who thought to cure true syphilis by what he called syphilization. Danielssen inoculated these leper patients hundreds of times with the pus from soft sores, and produced on them nothing more than a local ulceration, until, on one occasion, the product of a hard sore was taken by misadventure, and this was followed on the patient by a similar adhesive non-suppurating sore, followed by true syphilis.

The so-called syphilization of Boeck was, in my opinion, a complete misnomer. The patients recovered from syphilis, when inoculated with soft sores, in precisely the same way as they recover when altogether untreated. And I need not at the present hour say that the natural history of syphilis is very often a very mild and uneventful one. As to the sores produced by the auto-inoculation of the hard sore, when that is made inoculable by being irritated by savine ointment, they bear a certain resemblance to, but are easily enough differentiated from, the soft sore.

The character of the simple chancre is that it is a suppurative ulceration, which seems stamped out of the tissues, with a greyish base, and that it is very contagious, so that usually there are more than one of such sores, whilst the sore produced on the patient himself by inoculation from an irritated hard sore is not ulcerative, but rather raised above the surface; and, furthermore, it is not very contagious, and cannot often be re-inoculated, a property which the soft sore possesses, for hundreds of times on the same person. Then, as to the diagnosis between the soft sore and the hard, or primary lesion of syphilis, that seldom presents any difficulty. Those who have practised inoculations on a large scale, or seen them practised, are well aware that the soft chancre appears in the course of some twenty-four to forty-eight hours after inoculation, as a small eminence which gradually turns into a pustule and ulcerates—*i.e.*, the soft sore has absolutely no incubation.

The hard sore when it has been purposely inoculated in a way similar to this, has always presented a rather long period of incubation, from fourteen up to thirty days, or even longer, and if in our clinical observations this cannot always be noted, it is because in such questions there is so little reliance to be placed upon the recitals of those who have contracted such a contagion.

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The aspect of the two sores is so dissimilar that they need rarely be confounded. The soft sore secreting pus, and greyish ; the hard sore secreting little, and ham-coloured ; the soft sore rarely accompanied by bubo, but when so accompanied the bubo being always suppurative, whilst the hard sore is inevitably, at least in the male, the cause of multiple enlargements of the glands in the vicinity of the part in which it is situated.

Again, whilst the soft sore is multiple, the hard sore is generally solitary. Whilst the former is painful, the latter is rarely so.

These, and a certain number of other characters, are quite sufficient to make us easily able to say to a patient who comes to us with a sore, "You will, or will not. have syphilis." Who is there that has had much experience who denies this ?

A further instance of the non-relationship of the two sores is seen in the fact that, by hygienic care, the soft sore may be very much diminished in frequency, whilst at the very same time the hard sore and syphilis may be on the increase. If one of these sores depended on the other, it would naturally follow that, when one decreased, the other would. But in Paris, according to Dr. C. Mauriac,* "la réglementation de la prostitution à Paris à eu l'effet du diminuer quelquefois tellement le chancre simple, qu'on ne voit que peutêtre 50 cas à la clinique de son hôpital (l'Hôpital du Midi)," at the very time when the hard sore, or initial lesion of syphilis, has so increased in that city as to make syphilis at his hospital increase to such a degree from 1861, when it was 570, up to 1875, when it was 1,554.

It must be remembered that Mr. James Lane, whose experience in these matters has been most extensive, and who is quite cognizant of all that has been written on the side of dualism, yet remains on the side of unity. His arguments are, however, not convincing to me. He admits, what no one will now question, that a sore which is attended with indolent painless induration and similar multiple enlargement of the neighbouring glands will almost invariably be of the infecting character. Here he is entirely with the dualists ; but in the next sentence he asks, "Is it equally certain that the soft suppurating sore is never followed by a like result ?'

I think that no dualist has ever gone so far as to say that a patient with soft sore should not be infected at the same time with syphilis, which will appear after an incubation of some weeks. It is true, too, as quoted by Mr. Lane, that twenty-nine experienced witnesses, examined before the 1864 Committee at the Admiralty, gave evidence that sores, both soft and hard, may be followed by every variety of syphilitic eruption. But almost all these witnesses are now dead, and had lived and practised long before the discovery of Bassereau and the subsequent discussions.

Mr. James Lane admits, then, that hard and soft sores have an intimate relation to each other, and are, in fact, the product of one and the same virus. He maintains that the action of the virus may be modified in the case of the soft sore, so that its infecting property is materially lessened ; but it still retains that property to some extent, and, under favourable circumstances and on a suitable soil, may produce constitutional disease.

Mr. Hutchinson seems to think that the soft sore results simply from pus contagion, this pus being more or less peculiar, its peculiarities being due to its having originated in syphilitic inflammation. He insists that this pus may or

* "Rareté actuelle de Chancre Simple." Paris : 1876.

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may not contain the germ matter of syphilis, and that the soft sore therefore will sometimes prove infecting.

For my part, as I read the mass of accumulated evidence on this point, I cannot hesitate to conclude that the soft sore differs as much from the hard sore as variola does from scarlatina, or as wheat does from oats. The grain is different; and as no variety of soil or climate that we are acquainted with will convert one such species of grain into the other, so, in the same way, I hold it as made out that no variety of human constitution will convert variola into scarlatina, or make syphilis originate in a soft ulcerating sore inoculable an infinite number of times on the patient himself.

The question is fundamental. The doctrine of syphilis depends on it. It influences the whole statistics of syphilis to such a degree as to render our Army Medical Reports valueless, if dualism be the true doctrine I believe it to be. It is impossible, indeed, to conceive of any advance being made in the description of syphilis until this point is settled. At this moment chaos reigns supreme, to the satisfaction of those alone who like to carry the doctrine of political compromise into a territory not suited to it—that is, into the domain of natural science, whose motto is "Thorough.'

DISCUSSION.

Mr. Hutchinson, London: I am much obliged, Mr. President, for Dr. Drysdale's clear and explicit statement of the doctrines of dualism. When once an error is definitely avowed and expounded it becomes comparatively easy to refute to it. Not that I have any right to assume that Dr. Drysdale is in error. Indeed, I think he greatly exaggerates the importance of the difference between the schools of unicists and dualists. Respecting the clinical aspects of the facts, we are really much at one, and our difference concerns a question of the speculative interpretation of them. We all agree that there is but one lesion. In this sense we are all unicists. We all agree also that there are sores frequently contracted in the venereal act, contagious, just as the true chancre is, but which are local from beginning to end, and never induce constitutional symptoms. We all agree also that these local sores may be very much prevented by attention to cleanliness and careful inspection of those who are likely to be exposed to the risk of conveying them. However much we may differ as to the genesis and genealogy of these non-infecting sores, we all admit their existence and agree as to their chief clinical features. But between such a disease as syphilis—an affair of the whole organization, often a life-long disease—and a little trumpery local ulcer like the "soft sore," the difference is so vast that really I see no good reason to proclaim dualism respecting them. Excepting that they are both caught in sexual intercourse and that often for short periods they present features and resemblance which make diagnosis difficult, their courses are in no way parallel. It is as if we should place a little boy with a popgun side by side with a prize rifle shooter, and then say we had paired them. There was a time when it was believed that different kinds of sores were followed by different classes of constitutional symptoms, and then the word dualism meant something. Now, I submit, it has become almost meaningless. Looking at the facts from this side, I again repeat, if I may without offence, what I said some years ago, that "dualism is dead." But I must treat Dr. Drysdale and others whose opinions he expresses fairly in reference to the only question on which there is any difference between us. They believe that these non-infecting

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sores have from time immemorial been wholly distinct from syphilis that they originated in a peculiar and wholly separate virus, and that the two forms of virus exist side by side without ever mixing, and display their different effects as occasion offers. It is this speculative proposition, a matter of the merest theory, which I am concerned to doubt. It appears to me far more probable that soft, by which I mean non-indurated, sores, result from abortive inoculation with the secretions of those due to true syphilis. I believe firmly in the doctrine of a particular virus for syphilis, and, this being admitted, it is easy to see that the secretion from a true sore if inflamed might contain only modified inflammatory products and not the elements of the true poison. Such products would have an irritating local effect only, and could not produce the constitutional disease. A non-indurated sore is then for me the result of inoculation with syphilitic pus only, with a fluid which either does not contain any of the particular germs of syphilis, or contains them only in a dead or dying state. The soft sore is therefore a wholly non-specific affair. This theory seems to me much more simple and more easily credible than that which Dr. Drysdale has advocated, and it seems to well explain all the facts and to fit with them far better than his does. For instance, I cannot quite agree with him in thinking that the sore which will infect and that which will not are always easy to distinguish with certainty. I have very often known secondary symptoms to follow sores which good observers had pronounced to be "soft," and have been exceedingly cautious in pronouncing any opinion on this point. If characteristic induration is present, it is a most important symptom, and implies syphilis with certainty; but its absence goes for little or nothing. If I am correct in this belief of the present difficulty in diagnosis, the fact is, I think, much in favour of the soft sore being merely an offshoot of true syphilis. At any rate it proves that infecting sores may often be attended by inflammatory secretion, and thus that it is possible that pus contagion may take place from them. The further fact that soft and hard sores prevail under similar conditions and on the same class of persons also tends in the same direction, whilst there is, as I have already explained, nothing whatever against it in the fact mentioned by Dr. Drysdale, that it is possible to partially suppress the one by hygienic measures, whilst the other is much less easily restrained. In brief, nothing that Dr. Drysdale has told us in the least shakes my creed that the non-indurated sore stands in the same relationship to the indurated that an unsuccessful vaccination does to a successful one, and I still can see no evidence whatever in support of the theory that there exists a distinct form of particular virus for the soft sore.

Appareil à action continue destiné à redresser la direction des pieds dans les malformations de ces organes.

Dr. Th. Anger, Paris.

Cet appareil se compose essentiellement d'un ressort d'acier enveloppé dans un tube de caoutchouc qui inférieurement se fixe au soulier ou à la bottine, et de là monte en s'enroulant en spirale autour de la jambe et de la cuisse pour se terminer et se fixer à une ceinture en cuir.

S'il s'agit d'un pied-bot varus, avec déviation de la pointe du pied en dedans, le ressort d'acier se fixe au côté externe de la bottine et monte en s'enroulant en spirale autour de la jambe et de la cuisse pour aller se terminer et se fixer à la

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ceinture, près de la crête iliaque. Lorsque le pied-bot est double, les tiges d'acier après avoir été enroulées en spirale autour du membre, vont se rejoindre et se fixer en arrière de la région lombaire où ils sont maintenus par une ceinture abdominale.

Les avantages de cet appareil sont nombreux et si évidents par eux-mêmes que vous les devinez aisément.

Le premier de tous ces avantages, c'est la douceur et la continuité de son action. C'est pour ainsi dire un principe chirurgical que, pour vaincre la résistance constante des muscles et des malformations articulaires, il faut lui opposer une force également constante et permanente qui agisse en sens contraire. Or dans le pied-bot la résistance qui provient des muscles, des ligaments, des malformations articulaires étant continue, il faut pour la vaincre employer une force également continue, qui est ici représentée par ce ressort d'acier. Le second avantage de cet appareil est sa simplicité, et par suite la modicité de son prix.

La force de l'appareil peut être graduée à volonté. Si une seule bande d'acier ne suffit pas, on en accole deux ou trois, suivant l'âge du malade et la résistance que l'on éprouve à diriger le pied en dehors. Il est même préférable d'employer plusieurs tiges d'acier qu'une seule, parce que l'appareil devient moins cassant.

Avec cet appareil les mouvements du membre conservent leur liberté absolue: l'extension, la flexion de toutes les articulations restent libres. L'enfant marche avec une grande facilité. Mais tout en marchant, il est constamment sollicité par l'action rotative de l'appareil à diriger la pointe du pied soit en dehors, s'il s'agit d'un pied-bot varus, soit en dedans, s'il s'agit d'un valgus.

L'appareil est applicable à tous les âges. Je l'emploie dès la naissance pour corriger l'attitude vicieuse du pied, et plus tard lorsque l'enfant marche, il réussit admirablement à imprimer aux pieds une bonne direction.

De la déviation en dehors du gros orteil (halux valgus, vulg. "oignon," "bunions," "Ballen") et de son traitement chirurgical.

Professor Jaques Reverdin, Geneva.

L'affection dont j'ai l'intention de vous entretenir pendant quelques instants n'a jusqu'ici été l'objet que d'un petit nombre de recherches et paraît au premier abord plutôt du ressort du pédicure que du chirurgien; il s'agit de la déviation du gros orteil en dehors que les anglais appellent vulgairement *bunions*, les allemands *Ballen*, et les français "oignons," à laquelle Hueter a donné le nom plus scientifique de *hallux valgus*. L'oignon est une affection très-fréquente, il occasionne souvent de très-vives douleurs, il entrave toujours plus ou moins le fonctionnement du pied, gêne la marche; il entraîne parfois de graves complications, suppurations, fistules, arthrite et ostéite; voilà bien des raisons pour ne point en négliger l'étude et pour chercher au contraire à en perfectionner la thérapeutique. Deux mots d'abord sur les lésions et sur leur pathogénie probable, puis j'arriverai au traitement chirurgical; objet principal de cette communication. Je n'insisterai pas sur les lésions osseuses et articulaires de l'oignon; sur l'exostose qui se développe au côté intérieur de la tête du métatarsien, sur les franges vascularisées de la synoviale articulaire, sur l'usure et l'érosion des surfaces cartilagineuses au niveau de fausses sésamoïdiennes du

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métatarsien, sur les altérations de la forme et des rapports des os sésamoïdes, sur les changements de direction des tendons ; tout cela a été décrit par Broca, Volkmann, Prompt, &c., et d'ailleurs m'entraînerait trop loin. Je veux seulement vous signaler deux faits qui me paraissent intéressants—1°, la communication constante de la bourse muqueuse avec l'articulation ; 2°, les rapports et les altérations du nerf collatéral interne du gros orteil.

1.—Au niveau de la saillie formée par le premier métatarsien de l'exostose, on trouve immédiatement sous la peau une large bourse muqueuse ; sa paroi est constituée par des feuillets superposés ; or, dans tous les cas que j'ai examinés, cette bourse muqueuse communiquait librement et largement avec la synoviale articulaire, je m'en suis assuré, soit par injection, soit directement par les dissections. Ceci explique les dangers que peuvent entraîner l'inflammation et l'ouverture spontanée de cette bourse muqueuse ; l'orifice persistant ordinairement à l'état de fistule peut alors donner libre accès à l'air, à ses germes, au pus jusque dans la cavité articulaire, d'où des arthrites, des ostéites suppureés ; c'est dans des cas de ce genre que les altérations osseuses ont par nécessité, soit une résection articulaire, soit une amputation partielle.

2.—Si l'on suit le nerf collatéral interne dorsal du gros orteil, on le voit au niveau de la bourse muqueuse pénétrer dans l'épaisseur même de la paroi interne, de son feuillet sous-cutané ; du point où il en sort ou un peu plus loin, ce nerf paraît plus épais et en même temps plus étalé ; dans un cas j'ai rencontré une autre disposition : le nerf traversait librement la cavité même de la bourse muqueuse, il baignait de toutes parts dans la synovie et dans ce cas encore le nerf présentait le même épaisissement. Il est probable que l'enveloppe du nerf se trouve doublée de couches de tissu fibreux, mais je n'ai pas encore eu l'occasion et le loisir de faire l'examen histologique du nerf ainsi attiré. Cette constatation anatomique ne doit-elle pas expliquer, au moins en partie, la violence des douleurs que ressentent parfois les gens affectés d'oignons, les irradiations de ces douleurs et leurs exacerbations provoquées par la réplétion de la bourse muqueuse ? Sous l'influence d'une marche un peu prolongée, du port d'une chaussure neuve, la bourse s'enflamme, secrète du liquide et le nerf se trouve soumis à une pression hydrostatique plus ou moins forte. Je vais être le premier à attirer l'attention sur cet état du nerf et ses conséquences.

Deux théories principales ont été émises sur la pathogénie de l'oignon ; la première est due à Volkmann (1856), et a été adoptée par Pitha ; je ne crois pas devoir m'y ranger. D'après cette théorie l'articulation serait le siége d'altérations analogues, semblables à celle du "morbus coxa senilis," de certaines arthrites déformantes ; contrairement à l'assertion de Volkmann, je me suis assuré que cette maladie ne se développe point seulement vers l'âge de 40 ans et au-delà ; j'en ai vu plusieurs exemples chez des sujets beaucoup moins âgés, un jeune homme de 19 ans m'en a offert un exemple type.

Je suis au contraire fermement convaincu que dans la majorité des cas les lésions de l'oignon sont dues à une cause purement mécanique, que la forme vicieuse des chaussures doit en être probablement seule accusée. Actuellement j'arrive du nord de l'Allemagne où généralement les chaussures ont une forme relativement bonne : l'oignon y est peu fréquent ; à Londres au contraire, où l'on porte des souliers pointus qui ne répondent point à la forme normale du pied, j'ai pu voir en peu de jours plusieurs exemples d'oignon. Je me range donc à la théorie défendue par Meyer, Nyström, Hueter, &c. Mais il faut cependant faire quelques réserves : en premier lieu certaines déviations paraissent se rattacher

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directement à une affection générale, rhumatisme, goutte, &c ; en second lieu il n'est pas impossible que les altérations mécaniques dues aux chaussures ne se trouvent facilitées par quelque altération primordiale des os et des tissus articulaires ; cela est-il la règle ou l'exception, cela est-il nécessaire ou fortuit, je ne saurais le dire.

Quoique l'oignon soit fréquent la chirurgie a peu fait pour son traitement ; quelques auteurs, comme Camper, Meyer, Günther, Nyström, ont insisté sur sa prophylaxie ; d'autres, comme Pitha, Lothrop, ont imaginé des appareils redresseurs, enfin Hueter a conseillé la résection articulaire dans les cas graves.

Pour ma part j'ai mis en œuvre trois moyens ; un appareil et deux procédés opératoires. L'appareil se trouve être presque identique à celui de Pitha que je ne connaissais pas quand je l'ai fait construire : je l'ai employé soit comme seul moyen de traitement dans les cas peu avancés, soit comme moyen adjuvant après l'opération. Les procédés opératoires sont les deux suivants : — Le premier consiste dans l'ablation de l'exostose, dans un simple nivellement du bord interne du pied ; la déviation de l'orteil n'est point corrigée, l'application de l'appareil redresseur est alors nécessaire. Le second procédé répond aux deux indications : supprimer la saillie anormale de l'exostose et rétablir la direction normale du gros orteil.

1er Procédé.—L'année dernière dans deux cas d'oignons compliqués de fistule je me suis borné à inciser la bourse muqueuse, à enlever ses couches internes avec la cuiller Volkmann et à abraser l'exostose d'un coup de ciseau : la cicatrisation obtenue j'ai fait porter l'appareil redresseur, malheureusement je n'ai pu suivre ces deux opérés, mais je n'avais pas été très-satisfait du résultat obtenu et je pensai que l'on pouvait faire beaucoup mieux.

D'un autre côté j'étais peu tenté d'adopter, sauf dans certains cas, le procédé de Hueter. La résection articulaire a en effet le grave inconvénient de supprimer la saillie du premier métatarsien, ce que l'on a appelé le talon antérieur ; cette suppression doit évidemment gêner à un plus ou moins grave degré le fonctionnement du pied dans la marche. J'ai donc cherché à redresser l'orteil tout en conservant intacte l'articulation metatarso-phalangienne, et voici le procédé que j'ai suivi dans les six opérations que j'ai pratiquées jusqu'ici.

2e Procédé.—Une incision est faite sur la face dorsale du premier métatarsien à un centimètre en dedans du tendon de l'extenseur propre ; d'abord vertiligne et parallèle au tendon, cette incision arrivée au niveau de la tête du métatarsien se recourbe en dedans ; elle vient se terminer à la limite de la face dorsale et du bord interne du pied, dans le sillon qui sépare l'exostose de la partie articulaire de la tête métatarsienne ; le périoste qui a été incisé du même coup est alors détaché de l'os avec la rugine ; l'exostose est ainsi complétement mise à nu par la préparation de cette sorte de lambeau cutané et périostique. Le tranchant d'un ciseau est appliqué dans le sillon de séparation de la tête osseuse et de l'exostose, et d'un coup de marteau cette dernière est complétement détachée ; le bord interne du métatarsien est de cette façon en quelque sorte nivelé. Il s'agit maintenant de corriger la direction vicieuse de l'orteil, et cela est très-facilement obtenu au moyen d'une petite ostéotomie cunéiforme ; la base du coin osseux qu'il s'agit de tailler dans le métatarsien répond à la surface osseuse mise à nu par l'ablation de l'exostose ; le trait antérieur de cette base sera placé à un centimètre et demi ou deux centimètres de la surface articulaire ; la largeur de la base du coin et la direction à donner au ciseau sont naturellement calculées suivant le degré de la déviation de l'orteil. Dans mes six opérations ce coin osseux a pu être très-facilement

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taillé, l'os était d'une dureté moyenne, et d'ailleurs le ciseau pénétrant d'emblée sur une surface avivée de l'os n'avait pas à surmonter la résistance de la couche corticale ; le coin osseux fut donc rapidement enlevé.

L'orteil peut alors être absolument redressé, à moins que les tendons n'y fassent obstacle ; dans ce cas il est facile de sectionner l'extenseur propre comme cela m'est deux fois arrivé. La section peut se faire à travers la plaie primitive, ou au besoin à travers une seconde incision ; la ténotomie n'a pas eu de conséquences fâcheuses ; elle a permis un redressement complet et la continuité du tendon s'est parfaitement rétablie par la suite.

Dans mes six opérations j'ai pratiqué la suture des surfaces osseuses au moyen d'un simple point de catgut ; il y aurait peut-être avantage à assurer une coaptation plus exacte par une suture métallique perdue comme celle qu'emploie le professeur Lister dans l'ostéotomie. Je n'ai pas parlé de la bourse muqueuse ; un simple lavage avec la solution d'acide phénique, un raclage avec la cuiller tranchante, ou l'excision avec les ciseaux pourront être adoptés suivant les cas. Il va sans dire que toutes les précautions antiseptiques ont été prises avant, et pendant l'opération, et qu'après c'est au pansement de Lister que nous recourons.

C'est grâce à la sécurité que nous donne la méthode antiseptique que l'on peut entreprendre cette opération : il faut remarquer que non seulement l'os est mis à nu et coupé, mais que de plus l'articulation est forcément ouverte directement et indirectement par l'intermédiaire de la bourse muqueuse ; avant la méthode antiseptique on n'aurait pas eu le droit, si l'on peut dire, de pratiquer une opération certainement hasardeuse sans son secours, pour remédier à une affection gênante, douloureuse souvent, mais qui après tout ne pouvait compromettre la vie que dans des cas assez exceptionnels. Aujourd'hui les conditions sont tout-à-fait différentes. Voici du reste les résultats que j'ai obtenus. Les suites de l'opération ont été en général très-simples ; dans deux cas cependant la réunion par premiere intention n'a pas été obtenue, la plaie a suppuré, quelques petites séquestres ont été éliminées, mais la guérison n'en a pas moins eu lieu sans aucun accident grave. Dans ces deux cas, il faut le noter, la bourse muqueuse avait suppuré et un orifice fistuleux existait au moment de l'opération ; c'est évidemment une moins favorable condition ; mais c'est surtout moi, l'opérateur, que j'accuse de cette suppuration ; il est probable que je n'ai pas dans ces cas suffisamment désinfecté le foyer opératoire.

Au point de vue de la forme je vous montre ici le résultat obtenu, et j'ajoute qu'avec moins de timidité on pourra certainement obtenir mieux en général que dans la figure que je montre.

Au point de vue fonctionnel mes deux premiers opérés marchent facilement, normalement, sans douleur et se déclarent parfaitement satisfaits. Le dernier a été opéré trop récemment pour que je puisse vous donner le résultat à ce point de vue.

J'ai fait porter à mes opérés, une fois la consolidation osseuse terminée, un petit appareil pendant la nuit et leur ai fait faire pour la marche des chaussures spéciales, dont la semelle présente une sorte de loge pour le gros orteil.

Il me semble que l'opération que je viens de vous décrire présente une valeur réelle, car, je le répète, l'articulation reste ainsi intacte ; les mouvements passifs aussi bien que les mouvements actifs s'y font comme dans un pied sain ; c'est, à n'en pas douter, un résultat bien supérieur à celui que peut donner une résection métatarso-phalangienne. Cette dernière opération, la résection articulaire devra être reservée, je pense, pour les cas où l'articulation étant déjà envahie par la

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suppuration, ou les surfaces articulaires étant profondément altérées, les tissus en un mot infectés et septiques, nous ne pouvons faire autrement que de les retrancher.

Si nous mettons à part ces cas graves, si nous laissons de côté les cas peu avancés pour lesquels les appareils peuvent probablement suffire, il reste une catégorie nombreuse de cas moyens qui sont, je le crois, curables par l'opération que j'ai pratiquée. Les malades seront soulagés de leurs douleurs, mais aussi, et surtout mis à l'abri des complications parfois fort graves qu'amènent l'inflammation, la suppuration, la formation de fistules, l'arthrite, l'ostéite et qui peuvent nécessiter une résection ou une amputation.

On the Etiology of Congenital Club-foot (Talipes Varus Congenitus).

Dr. W. J. Little, London.

The theory of primary malformation of bones is considered by many to be sufficient to explain the production of congenital club-foot. Whilst acknowledging the fact of comparatively unimportant alteration in the form of some bones, nothing is more certain, in our opinion, than that the development of the bones is apt to be modified by their relation to, or by the behaviour of their neighbouring parts. We believe the alteration to be secondary, and not primary. In the fœtal normal foot, the several bones remaining in their natural places present a barrier to any preternatural growth of any single bone. But when, from any cause, the front row of the tarsal bones is drawn or carried inwards, the round head of the astragalus, for example, becoming uncovered and unsupported, may grow exuberantly when the *nisus formativus* is still active within it.

Prof. C. Hueter, Mr. W. Adams, and Prof. Volkmann assert, for example, that it originates in malformation of the separate bones, and that no force acting on the ankle-joint could have gradually drawn the previously well-formed limb into the morbid position, but that the foot wrongly developed itself in consequence of unequal growth of the bones. This opinion of unequal growth is accompanied by no explanation of the causes of its existence. By it is probably meant that congenital varus is always hereditarily predetermined. We regard as a pure assumption the opinion, that no force outside the ankle-joint or outside the bones is capable of altering the form of the astragalus, or drawing the fœtal foot into the abnormal position in which it is found at birth. More important than the form of the bones is the fact of great displacement of the navicular and the tarsal bones, which, together with the metatarsus and toes, are drawn or carried inwards by some disturbing cause. This disturbing cause can only be operative from within the embryo—for example, muscular contraction; or from without, for example, uterine pressure, or pressure of neighbouring organs. It is not sufficient to study the etiology of fœtal varus, which goes on in the dark, as it were, merely by the light of the proved existence of some bone malformation in it. It should be studied by the help of the facts we see unrolled before our eyes. We stated many years ago that, despite the bone alterations, when the contracted tendons of the child's club-foot are severed within a few days after birth, the natural form of the foot is recovered in a few days or a month, with a rapidity

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which would astonish those surgeons who believed in the importance of the bone changes.

Nevertheless, like all facts of morbid anatomy, the knowledge of the minutest changes has its value. From the author's point of view they are valuable, as they appear to be precisely the changes which might proceed from the dragging inwards of the navicular and other anterior bones of the tarsus.

The author and others, before the writings of these surgeons, described, amongst other congenital changes, the formation of a joint between the navicular and the inner malleolus, and the morbid posterior position of the external malleolus. It remains for the author to show* that, in a non-congenital equino-varus the navicular may articulate with the internal malleolus, and that the external malleolus assumes the morbid posterior position; both changes being identical with those met with in severe congenital cases.

These two changes of relation, in addition to the actual changes of the form of the bones, which we have shown to be unimportant as regards cure, are somewhat dogmatically asserted by Volkmann in support of the primary bone malformation theory. Nevertheless, Volkmann says† that the majority of the causes which operate during later life may occasionally be operative during fœtal existence. We are of opinion that no anatomist can study congenital varus side by side with extreme non-congenital varus without being convinced that anatomical conditions so similar can only have had a similar primary origin. The author presumes that the writers named have never witnessed the origin of the most complete varus during adolescence. There can be no question of primary malformation of bone in a case of spasmodic or hysterical varus springing up gradually in the adult before our eyes. The intra-uterine morbid phenomenon in question unfolds itself in a few weeks or months, because in that period the whole outline of the future man is built up. We have witnessed varus from cerebro-spinal disease developing itself in adolescents with comparative suddenness, or taking several years to attain full development, presenting ultimately as much deformity as any congenital case, the great toes in both feet touching the inner side of the tibia. If such, instead of becoming cured, had been the subject of anatomical post-mortem inspection, the essential changes of relation and form would have been found the same as those described of congenital club-foot.‡

The author apprehends little difficulty in the ultimate general acceptance of the above explanation of the similarity of the primary causes of non-congenital and of congenital T. varus, in cases where an hereditary cause is not discovered. Congenital varus, when of a comparatively slight grade, may be readily supposed to have originated during the second half of utero-gestation, during which the bones and muscles of the fœtus may be supposed to be so far advanced in their distinctive features as that the bones may be acted upon by causes working through the nerves and muscles, in a manner similar to that which obtains after birth. Sufficient facts in human embryology, in comparative anatomy, and in the

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* "On Little Club-foot," f. 8, p. 12.

† R. Volkmann, "Handbuch der Allgemeinen und speciellen Chirurgie," Erlangen, 1872, p. 690.

‡ The most extreme non-congenital case referred to was seen by Trousseau and Nélaton, who recommended amputation. It was seen by me in consultation with four of the most eminent physicians and surgeons of England. The case was cured, aided by tenotomy and gentle gradual mechanical treatment. The patient remains well and active.

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pathology of deformities are now known to justify an hypothesis to explain in what manner, allowance being made for differences in the condition of the embryo during the first or second third of uterine existence, a varus at that early period may be brought about.*

The limbs are said to appear as buds at about the commencement of the fourth week, at about the fifth or sixth the skeletal matrix is evident in a state of chondrification, some ossification being visible about the fifth, the outlines of muscles being discoverable about the sixth or seventh week, the cerebro-spinal axis and the nervous structure being apparent at the end of three months; the mass of the embryo at this period being soft in texture, comparable to the shell-less mollusc rather than to the formed skin, flesh, and bones of the advanced fœtus.

As soon as the triple circuit of cerebro-spinal axis, nerves (nerve elements, molecules, or cells), and muscles is formed in the embryo, all the materials for direct or reflex muscular action and its consequences exist upon the application of a given stimulus to action along this circuit. As it is proved that even extreme varus may be produced in the adult and adolescent, little effort is necessary to believe that in a previously well-formed foot during the later months of gestation when the muscles and bones of the foot are as well differentiated as they are found to be in sixth and seventh months' fœtuses, a varus may be produced in the later months of gestation from causes similar to those that act after birth.

Nevertheless, equally strong proofs may be required to show that that which is doubtless true of a six or seven months' child is applicable to the embryo of three or four months. The experience afforded by the study of large numbers of congenital varus cases immediately after birth enables us to judge approximately the age of the embryo and fœtus at which the club-foot originated.

A slight case differs little in appearance from a natural foot; a slight pressure with the hand suffices to put it into a right shape, from which it relapses when the hand is withdrawn. A worse case may resemble the natural foot turned inwards, the inner margin being unduly raised and the great toe pointing upwards and inwards, requiring firmer handling in attempts to straighten it, and foiling the surgeon's attempts to effect more than partial replacement. We believe that in such still comparatively slight cases, the less the distortion, the nearer to the close of gestation it arose. We name severe cases, not simply those in which to the inexperienced eye deformity appears very evident, but those in which the surgeon is unable wholly to replace the parts with his hands, and in which he judges that resistance is offered, not merely by two or three tendons and muscles, but by all the cutaneous, fascial, muscular, and ligamentous structures on the resisting side of the limb; in which he judges that the muscles usually contracted are individually less defined, and appear as if they were bound down with unusual tightness to the subjacent bones, and sometimes as if the integuments, fasciæ, and muscles were in parts fused together, presenting deep clefts in the skin behind the heel, over the internal malleolus, and in the sole, denoting to him the greater length of time the

* It should be remembered that the intra-uterine morbid muscular action may, like the same morbid action when originating in after-life, have been only transient or temporary, so that the surgeon may, when consulted, have to treat, not the primary morbid muscular action, but simply the resulting structural adapted shortening. Thus, when the patient is examined, he may exhibit neither paralysis nor spasm, but only contraction or shortening of muscles and other structures.

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distortion had existed *in utero*. These worst cases often present knee contraction and deficiency of one or two toes.

We attribute the production of the worst cases to embryonic life before "quickening," if not to the earliest period after the cerebro-spinal axis, nerves, and muscles were first developed, when all these parts existed only in the molecular state, aggregated into separate groups representing the future nerve centre, the nerves, and muscles, before they were differentiated into distinct nerve and muscle structures.

When we endeavour to explain the production of varus in the embryo of three months by analogy with the observations we are enabled to make upon the gradual changes in form and structure of the adolescent, the child, and fœtus, the difference in the kind of organization of the embryo compared with that of later existence is so great, and so little is positively known as to the mode of production of pathological changes (during the first third of uterine life), that we are forced to resort to comparative physiology, the occasional handmaid to human pathology, for aid in this obscure department of knowledge. We turn to groups of animal life of a lower scale of organization, in which the visible structures are all soft and more nearly resemble the temporary condition of the similar structures in the human embryo, the play of whose organs we can watch, in search of facts which may possibly enlighten us as to the processes of diseased organization during the earlier stages of human dependent existence.

In the protozoon, composed of almost transparent structures, we observe the individual parts move upon each other, or the entire animal moves as stimulated to act apparently by pleasure or discomfort, from which is inferred the existence of nervous and muscular molecules, although distinct nerves and muscular bands are undistinguishable.

An interesting illustration of the spontaneous action of a lower form of animal life in convulsively twisting its limbs and even destroying them is afforded by the so-called brittle star-fish, common on the west coast of Ireland. The dredger may bring up from the deep a hundred or more of this star-fish and find not one perfect one amongst the lot, the animals in their death struggles to escape capture having broken off their limbs. But if, before death, the whole mass dredged be at once plunged into fresh water they die suddenly and have no time to break their limbs.

In higher organisms, say the mollusc, in which ganglions, nerves, and muscles can be distinguished, there are found more elaborately developed local moving organs, such as a heart, an alimentary canal, as well as movements of particular parts of the body of the animal upon each other.

It will be conceded that whenever there exists, as in the embryo, a nerve centre, be it a ganglion or spinal cord, nerves, and muscles, there will, as a consequence, co-exist the potentiality of action or motion liable to affect the relation of the moved with the adjacent parts.

We believe that the aggregated molecules of the embryo can be drawn hither or thither, analogous to the direction after birth of either T. valgus or T. varus, as the muscles may determine, like those of the protozoon stimulated to move by causes acting from within the developing embryo; or from without—*i.e.*, from influences conveyed from the mother; or from disturbing, irritating external causes and that it is not unlikely that direct or reflex movements so set up in the embryonic matter may affect the form and functions of the future organs, even to the extent of causing not only displacement of them, but fusion and even destruction of them.

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In the embryo a particular part, such as the foot, is more susceptible of disturbance, just as in the completed fœtus and in the individual in after-life a particular muscle such as the gastrocnemius, or, as stated by Mr. Hutchinson, the levator palpebræ superioris, is more susceptible of morbid (hereditary) contraction than other muscles.

We believe also that internal nervo-muscular disturbance of the embryo, excited through the system of the mother, and mechanical disturbance propagated from the uterus and neighbouring maternal organs may set up disorder in the nervous and muscular particles of the embryo and interfere with its development.

Thus far we have avoided as much as possible allusion to hereditary club-foot, whether upon the paternal or maternal side, or their respective ancestors. We have striven in what has been here written to endeavour to express our conviction that congenital club-foot springs from similar disordered action in the embryo and fœtus to that which affects adolescents and young adults as a consequence of distinct cerebro-spinal, nervo-muscular disturbance. We have urged that the degrees of the distortion, the symptoms, and the changes of relation being identical, an extreme probability exists that the pathology of the two conditions is identical.

Let us now inquire whether the explanation we have ventured to offer of the mode of origin of non-hereditary congenital club-foot can be applied to explain the occurrence of the undoubtedly hereditary distortion.

We consider that hereditary distortion must be either predetermined in the germ, or that it is the result of an hereditary disposition taking effect during uterine life, instead of being delayed until after life.

Regarding distortion of congenital club-foot, however, as the consequence of disordered action, we believe rather that the predisposition to disorder of the nervo-muscular system and distortion is communicated to the germ, than that the germ shall have acquired the certainty of becoming developed in an unhealthy manner. Many acknowledged facts in special and general pathology point to propagation of the tendency to diseased action, both in intra-uterine and post-natal life, rather than to the direct imprinting of a particular disorder or distortion *ab initio*. A striking fact in this direction appears to be the tendency to increase of the intensity of the disease or distortion in each succeeding generation, rather than a tendency to diminished intensity.

If only a predisposition to distortion be handed down we can understand that many embryoes may escape distortion when disturbing or exciting causes during intra-uterine life are not applied, and the explanation here offered of the production of non-hereditary congenital club-foot may also be applicable to that of the hereditary distortion.

For want of time the consideration of the manner in which the relations of the fœtus to the mother may induce disorder or distortion is postponed.

Die Reduction veralteter Humerus-Luxationen.

Professor Kocher, Bern.

Die Leichtigkeit, mit welcher gelegentlich die Luxatio humeri zu Stande kommt und die geringen Symptome, welche dieselbe in einzelnen Fällen anfänglich macht, erklären es, dass stets wieder veraltete Luxationen zur Beobachtung kommen. Es ist bekannt genug, dass mit der Zeit eine relativ ordentliche Brauchbarkeit des Arms, besonders des Vorderarms und der Finger eintritt, und es ist

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deshalb gewiss im höchstem Masse zu bedauern, wenn stets noch Repositionsmethoden Anwendung finden, welche ernste Consequenzen, ja sogar den Tod nach sich ziehen können. Es scheinen in England nach der Zahl der Publicationen zu urtheilen die forcirten Repositionsmethoden sich noch grösserer Beliebtheit zu erfreuen, als anderswo, und es möchte deshalb am Platze sein, auf einem Congress in London zu erörtern, welches die geeignetste Methode sei. Wenn man bei frischen Luxationen der Spannung und Contraction der Muskeln stets noch eine grosse Bedeutung zuschreibt von gewissen Seiten als Repositionshinderniss, so hat man dazu ein Recht in der Erfahrung, dass durch Chloroformnarkose häufig die Reduction, welche vorher nicht zu Wege zu bringen war, sofort gelingt. Es lässt sich aber ebenso bestimmt erweisen, dass die Reposition sich sicher und leicht bewerkstelligen lässt, wenn man unbekümmert um die Spannung der Muskeln, nur auf Kapselriss und speziell Spannung der unzerrissenen Kapseltheile Rücksicht nimmt.

Ich habe im Jahre 1870 ein neues Verfahren* beschrieben, welches darauf hinzielt, den Kapselriss zum Klaffen zu bringen und die unzerrissenen Kapselparthien zu erschlaffen, um ohne Gewaltanwendung die Reposition zu bewirken. Bei der gewöhnlichen Humerusluxation unter dem Rabenschnabelfortsatz ist die hintere Kapselwand, und die sie bedeckenden und verstärkenden Sehnen der hinteren Scapulamuskeln erhalten, und gespannt über die Pfanne gezogen. Sehr gespannt ist auch die untere Kapselwand, welche den untern Rand des Kapselrisses bildet. Die stärkste Spannung aber bietet die obere Kapselwand dar, speziell derjenige Antheil, welcher zwischen der langen Bicepssehne und dem obern Rande des musc. subscapularis liegt, und welcher durch die Fasern des lig. coraco-humerale verstärkt wird. Dieser Kapselantheil wird bei der Luxatio subcoracoidea gedreht und zu einem festen Strange angespannt. Dreht man nun von der gegebenen Luxationsstellung aus den Oberarm nach aussen, so dass der rechtwinklig gebeugte Vorderarm direkt nach aussen gerichtet ist, so wird der letzterwähnte Strang mit dem Humeruskopf nach aussen gedreht und dadurch die hintere Kapselwand von der Pfanne kräftig abgehoben und somit der Kapselriss zum Klaffen gebracht. Allein der Humeruskopf ist noch am vordern Pfannenrande festgepresst, weil der Strang der oberen Kapselwand durch die Bewegung durchaus nicht erschlafft wird, ebensowenig wie die untere Kapselwand.

Erst wenn man von der Auswärtsrotation aus, den Oberarm in der Sagittalebene des Körpers nach vorn erhebt, wird die obere Kapselwand erschlafft und Dank der Spannung der untern Kapselwand, welche verhindert, dass der Kopf nach vorne abgleitet, rutscht nun der Humeruskopf unter die abgehobene, hintere Kapselwand rückwärts in die Pfanne hinein. Eine Einwärtsrotation vollendet die Reposition.

Diese Methode, welche ich als Rotationselevationsmethode bezeichnet habe, hat sich überall, wo sie versucht worden ist, völlige Anerkennung erworben. Es giebt kein Verfahren, welches so sicher und leicht die Reposition der Luxatio subcoracoidea bewerkstelligt. Der Erfolg der Methode ist aber davon abhängig, dass wirklich die Kapsel in der beschriebenen Ausdehnung erhalten sei, da ihre Spannung zur Reposition benutzt wird. Ist die Kapsel ausgedehnter abgerissen vom Humeruskopf, letzterer stärker nach einwärts dislocirt, wie bei der Luxatio intra-coracoidea oder gewissen Formen der axillaris, so dreht sich bei unserm Verfahren der Kopf um seine eigene Axe, statt um einen Stützpunkt am vordern Pfan-

* *Berliner Klinische Wochenschrift*, No. 9.

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nenrande und die Reposition misslingt. Gerade für diesen Fall passt aber die Methode der direkten Auswärtsschiebung des Kopfes durch Druck vortrefflich, und die zuverlässigste Methode zur Ausführung dieser Absicht ist diejenige A. Cooper's, bei welcher die Ferse in die Axilla gestemmt und am Arme angezogen wird. Der Arm muss dabei auswärts rotirt werden. Wir haben alle Fälle, bei welchen unsere Methode nicht zum Ziele führte, in den letzten Jahren nach Cooper eingerichtet.

Bei unserer ersten Publication glaubten wir aussprechen zu sollen, dass die Rotationselevationsmethode für veraltete Luxationen nicht passend sei. Wir sind durch weitere Erfahrungen anderer Meinung geworden. Seit dieser Zeit habe ich 15 veraltete Luxationen in Beobachtung bekommen. Bei 2 wurde meine Methode nicht versucht, sondern die Einrichtung durch die Abductionsmethode nach Mothe zu Wege gebracht. In 13 Fällen wurde die Rotationselevationsmethode in Anwendung gezogen, und führte 12 Mal zum Ziele, zum Theil sogar ohne Narkose, ohne Anwendung irgend einer ausserordentlichen Gewalt, ohne weitere Assistenz. In einem Falle wurde gleichzeitig mittelst eines Bindenzügels um das obere Ende des Oberarms ein Zug nach aussen in Anwendung gebracht. Mehrfach zeigte es sich förderlich, den Ellbogen nicht nur an den Leib herangedrückt zu erhalten, sondern ihn etwas rückwärts hinter den Körper zu bringen.

Das Alter der Luxation betrug in den Fällen, wo meine Methode Erfolg hatte, 1 mal 3 Wochen, 2 mal 5 Wochen, 3 mal 7 Wochen, 4 mal 3 Monate, und 2 mal 4 Monate. Ich habe bei Anwendung der Methode einen einzigen Misserfolg zu verzeichnen. Bei einer 70-jährigen Frau, welche sich vor 8 Wochen eine Luxation zugezogen hatte, und welche mir von einem Collegen auf einer Eisenbahnstation kurz vor Abgang des Zuges vorgestellt wurde, brach der Oberarm unter der Mitte entzwei. Die Frau heilte in 6 Wochen; ein erneuter Versuch hatte nich bessern Erfolg; die Luxation blieb bestehen. Ob in diesem Falle die Eile der Ausführung grössere Schuld hatte an dem Unfall, will ich dahingestellt sein lassen, jedenfalls mahnt die Erfahrung zur Vorsicht bei sehr alten "gebrechlichen" Leuten.

Wenn ich mich nun frage, warum bei veralteten Luxationen die Rotationselevationsmethode noch sicherer zum Ziele führe als bei frischen Luxationen, so finde ich die Erklärung darin, dass bei jenen der Kopf durch Adhäsionen am vordern Pfannenrande sehr sicher fixirt ist und deshalb für die Drehung ein guter Stützpunkt gewonnen ist. Die Drehung hebt auch hier bei der Wendung des Kopfes nach aussen mit grosser Kraft die vielleicht zum Theil durch Adhäsionen auf der Pfanne fixirte, hintere Kapselwand ab, öffnet den Kapselriss und erzwingt bei der Elevation das Rückwärtsgleiten des Kopfes durch die Spannung der untern Kapselwand.

Fälle von Zerreissung der Hauptgefässe oder anderer wichtiger Theile, welche wie in einem unlängst publicirten Falle von Bellamy zur Aneurysmabildung führen und in der Folge tödtlichen Ausgang bedingen, können bei unserer Methode absolut nicht vorkommen. Eine fractura humeri, welche bei brüchigen Knochen alter oder kranker Leute möglich ist, wird man nicht als ein grosses Unglück betrachten. Ich habe von mehreren Collegen Mittheilung gelungener Reposition veralteter Luxationen erhalten nach unserer Methode, nachdem alle möglichen anderen, selbst sehr gewaltsame Versuche fehlgeschlagen waren, so von Dr. von Muralt, und von Dr. Kaufmann in Zürich.

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Sur la Hernie Etranglée.

Dr. Daniel Mollière, Lyons.

Le sac herniaire contient-il toujours la même anse intestinale?

Il est un détail encore peu connu dans l'histoire des hernies, c'est l'influence que peut avoir au point de vue du pronostic le niveau du tube digestif auquel siège l'étranglement.

Etant donné un sujet porteur d'une hernie, est-ce toujours la même portion d'intestin que renfermera le sac? Cette question que nous avons le premier soulevée en 1877 dans notre travail sur l'occlusion du péritoine après kélotomie a croyons-nous son importance.

La solution nous permettrait en effet d'éclairer quelque peu l'histoire des mouvements intestinaux. Ce qu'il m'a été donné d'observer me porterait à penser que ces mouvements sont extrêmement étendus, et que le pronostic des hernies étranglées est d'autant plus grave que le siége habituel dans l'abdomen de l'anse étranglée est plus éloigné du sac herniaire; c'est ce qui nous explique pourquoi on a pu voir dans un nombre de cas relativement considérable des anses intestinales, manifestement perforées, impunément réduites. Les matières stercorales s'écoulent par la plaie, tandis que, dans d'autres cas, la réduction d'anses très-légèrement érodées a suffi pour amener des péritonites mortelles.

Suivant que son siége habituel est plus ou moins éloigné, le coude formé par l'intestin est plus ou moins aigu, son calibre plus ou moins affaissé. C'est pour cette raison, que dans des cas, qui semblent du reste absolument identiques, on voit l'ouverture d'une anse gangrenée ne donner issue à aucun écoulement, tandis que, dans d'autres cas les matières jaillissent dès la première ponction. N'est-ce pas pour la même raison que, dans nombre de cas, la réduction des hernies est si difficile, même après le débridement du collet et de l'anneau, tandis que, dans d'autres circonstances, on voit l'anse disparaître dès que l'étranglement a été levé, et cela même avant que le chirurgien ait eu le temps de constater si elle est suffisamment saine pour être réduite? Cette même notion de physiologie pathologique nous explique encore pourquoi la manœuvre de l'inversion, qui consiste, comme on le sait, à suspendre le malade par les membres inférieurs, réussit si bien dans certains cas, et échoue absolument dans d'autres. Et pourtant les conditions semblent être strictement les mêmes.

Dans le premier cas, l'anse étranglée a son siége habituel très-loin, elle est ordinairement cachée dans le petit bassin, ou au voisinage de la colonne vertébrale. La corde formée par le mésentère est alors extrêmement tendue. Le poids intestinal agit alors, de dedans en dehors, comme une main qui, introduite dans l'abdomen, tirerait sur l'anse herniée. Au contraire, dans les cas où l'on échoue, c'est, peut-être, parce que l'on a affaire à une anse ordinairement en rapport avec l'orifice par lequel elle s'est échappée. En pareil cas, le rôle du poids de la masse intestinale est nul.

Mais avant de continuer cette discussion théorique, citons quelques faits.

Observation.—Hernie inguino-scrotale étranglée; choléra herniaire; kélotomie; réduction; mort; autopsie; l'anse réduite est profondément cachée dans l'abdomen. —Le nommé Claude B. est admis, le 7 Septembre, 1879, à l'Hôtel Dieu, Salle St. Louis, No. 7, service de M. Daniel Mollière. Cet homme, qui jouit ordinairement d'une très-bonne santé, est porteur, depuis vingt-sept ans, d'une hernie

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inguino-scrotale double. Jamais ces hernies ne l'avaient fait souffrir. Il n'avait jamais éprouvé que quelques troubles digestifs passagers. Toujours ses hernies étaient rentrées facilement. Trois jours avant son entrée à l'hôpital, et sans cause appréciable, sa hernie cessa d'être réductible. Quelques heures plus tard survinrent des vomissements verdâtres tout d'abord, puis féculents, et ces vomissements ont persisté jusqu'à l'entrée du malade à l'hôpital. Opération; anesthésie par l'éther. On pratique immédiatement la kélotomie après avoir fait quelques manœuvres de taxis pour assurer le diagnostic. Le sac ayant été mis à nu, on débride l'anneau autour du sac. L'anse intestinale est enflammée, mais elle n'est pas friable. En aucun point on ne trouve de menace de perforation. Il n'y a aucun symptôme de gangrène. Réduction facile. Occlusion du péritoine à l'aide d'un fil élastique, étreignant le collet du sac très-fortement, et arrêté à l'aide d'un anneau de plomb écrasé sur lui. Traitement antiseptique. 8 Septembre.—Fièvre peu intense. Les vomissements depuis la veille ont cessé. Dans la soirée selle diarrhéique spontanée. Les matières ont une coloration foncée, et une odeur très-fétide. Anxiété; subdélirium; à onze heures du soir délire violent, fièvre ardente. On est obligé de maintenir le patient avec une camisole de force. L'abdomen, cependant, ne paraît pas douloureux à la pression. Il n'y a pas le moindre ballonnement. Le malade succombe le 9 Septembre au matin. Autopsie pratiquée 30 heures après. Le cadavre est déjà dans un état avancé. A l'ouverture de l'abdomen, on ne trouve tout d'abord aucune altération. La masse intestinale semble normale, mais en soulevant les anses, au niveau de la région du cæcum, on trouve, profondément caché dans le petit bassin, l'iléon très-fortement enflammé sur une longueur de 15 centimètres. Les tuniques intestinales offrent une coloration brune très-foncée, et à leur surface l'on trouve des fausses-membranes nombreuses. On retrouve facilement le point qui a été étranglé. Il n'y a pas de perforation intestinale. Mais l'anse malade contient un liquide d'une coloration brunâtre, répandant une odeur extrêmement fétide. Le mésentère est, dans toute l'étendue correspondante à l'anse étranglée, très-fortement congestionné. Notons que les ganglions sont fortement engorgés dans son épaisseur, et signalons à l'attention cette complication peu connue, l'adéno-lymphangite mésentérique. La cavité péritonéale renfermait environ cent grammes d'un liquide jaune-brun, tenant en suspension de petits flocons fibrineux.

Observation.—Hernie inguinale étranglée; réduction par le malade lui-même; gangrène intestinale; mort; autopsie; intestin très-éloigné de l'anneau.—Le nommé X. B., âgé de 73 ans, entre dans mon service le 22 Décembre, 1876. Le malade est porteur d'une hernie inguinale qui d'ordinaire rentre sans difficulté. Trois jours auparavant, dans la nuit du mardi au mercredi, cet homme après un copieux repas est tombé. La hernie est sortie brusquement, il n'a pu la réduire. Vomissements, douleurs, ballonnement du ventre. Le malade désespéré, après trois jours de souffrance, exécute lui-même une séance de taxis si énergique que la tumeur se réduit. Mais tous les accidents persistent. On le transporte à l'hôpital où mon collègue Fochier lui pratique la kélotomie le vendredi soir. Il tombe sur un sac petit, graisseux et contenant seulement quelques fausses-membranes et une sérosité louche. Aucune tumeur au voisinage du collet. Pas d'intestin, pas d'épiploon dans le sac. Dès le lendemain on vit s'écouler par la plaie des matières fécales entraînées par une sérosité abondante. Péritonite aiguë. Le 25 au soir, mort. A l'autopsie, péritonite aiguë généralisée avec épanchement d'une quantité considérable de matières stercorales dans le péritoine. En aucun point, au voisinage de la plaie, je ne pus trouver l'intestin malade.

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L'anse gangrenée et perforée gisait au fond du petit bassin. Elle était noirâtre et friable, nettement limitée par un sillon ; la perforation siégeant au point opposé à l'insertion mésentérique était large et irrégulière, l'intestin dans ce cas était donc retiré très-loin de la région où il s'était étranglé.*

Observation.—Hernie inguinale double ; phénomènes d'étranglement ; double kélotomie ; ligature élastique à droite ; fistule stercorale à gauche ; guérison. Ce fait est absolument l'inverse de celui que nous venons de rapporter. Il n'en est pas moins démonstratif.—Le sieur S. P. entre le 28 Mai, 1877, dans mon service. Cet homme, âgé de 64 ans, est porteur depuis une trentaine d'années d'une hernie inguinale double. Le matin de son entrée, il a senti, à la suite d'un effort, de vives douleurs au niveau de ses hernies. Coliques violentes, vomissements ; pouls petit, facies altéré. Comme les hernies semblent réduites, on prescrit un purgatif qui reste sans effet. Le 29 au matin, les phénomènes constatés la veille par l'interne de service s'étant aggravés, et les vomissements persistant, le malade fut éthérisé et je pratiquai la kélotomie. A droite sac moyen, graisseux, contenant un peu d'épiploon ; le sac fut cerné dans une ligature élastique et excisé. A gauche sac vide, ne contenant qu'un peu de sérosité. J'étalai alors ce sac en tirant sur ses bords avec des pinces afin d'apercevoir l'orifice de son collet. Cette manœuvre suffit pour ouvrir ce collet que la pression intra-abdominale tenait fermé, et aussitôt un flot de matières fécales verdâtres s'échappa par son calibre. J'étais donc en présence d'un intestin perforé. Une sonde en gomme de petit calibre, introduite par l'orifice étroit du collet pénétra sans difficulté à environ 20 centimètres de profondeur. Il s'écoule par son calibre une quantité considérable de matières fécales. L'intestin perforé était donc en rapport avec la paroi abdominale, et la perforation en rapport avec l'orifice du sac. Le sac fut fixé à la peau à l'aide de quelques points de suture métallique. Opium à l'intérieur. Pas de pansement. Le soir même selle copieuse. Le 30, abdomen souple. Quelques matières s'écoulent encore par la plaie, mais en petite quantité. Le 31, abdomen souple, selle copieuse. Dès le quatrième jour les matières cessèrent de s'écouler par la plaie, et les selles redevinrent normales et copieuses quotidiennes. La ligature élastique placée sur la hernie droite tomba le quatrième jour. Le 28 Juin, le malade qui se lève depuis plusieurs jours n'a que deux petites plaies superficielles dans les aines qui étaient complétement cicatrisées lorsqu'il a quitté l'hôpital. Il est évident que dans ce cas le patient avait lui même réduit un intestin gangrené, mais enflammé, et que des adhérences se sont établies sous l'influence de cette inflammation. La kélotomie, ou plutôt la simple ouverture du sac a dans ce cas prévenu un épanchement stercoral dans le péritoine. Mais en somme ce qui est certain, c'est qu'après la réduction l'intestin est resté en rapport avec le collet du sac. Et cela parce que l'anse étranglée avait son siége habituel près de l'anneau.† C'est pour la même raison que l'on voit assez souvent guérir des malades dont la hernie s'est spontanément ouverte à l'extérieur. J'ai déjà plusieurs fois assisté à cette singulière terminaison naturelle de l'étranglement herniaire. Voici l'un de ces faits.

Observation.—Anus contre nature ; repos ; guérison spontanée.—La nommée

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* Cette observation a été rapportée dans mon travail sur l'occlusion du péritoine après la kélotomie, p. 10. 1877.

† Ce fait a déjà été cité dans mon mémoire sur l'occlusion du péritoine après la kélotomie.

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Marie B., née à St. Privat, âgée de 62 ans, entre le 22 Janvier, 1880, au No. 11 de la Salle St. Paul, Hôtel-Dieu de Lyon, dans le service de M. D. Mollière. Cette femme portait depuis neuf ans une hernie crurale droite, parfaitement réductible. Il y a une quinzaine de jours, elle s'aperçut que la tumeur s'enflammait, qu'elle devenait rouge, dure, douloureuse. La marche devint très-pénible. La hernie cessa de rentrer. La malade se mit au lit et fit de nombreuses applications de cataplasmes. Enfin elle se décide à entrer à l'Hôtel-Dieu. Aucun symptôme du côté du tube digestif. Dès son entrée on trouve la peau ulcérée et au fond d'un orifice anfractueux on aperçoit un sac épiploïque sphacèle. Il y a beaucoup de suppuration. Toujours absence absolue de tout symptôme d'étranglement. On se borne à faire des lotions désinfectantes et à appliquer un pansement conformément aux règles de la méthode antiseptique. Le septième jour après l'entrée de la malade à l'Hôtel-Dieu, on vit s'échapper par la plaie, qui était restée béante, une quantité considérable de matières fécales. Pas le moindre symptôme grave du côté du péritoine. On prescrit une immobilité absolue. Pendant les jours suivants, on voit les matières s'écouler par l'orifice morbide, mais la plus grande partie des excréments sont cependant expulsés par l'anus. Au bout d'une dizaine de jours, tout écoulement stercoral avait cessé. La cuisse avait été maintenue en demi-flexion sur le bassin afin de faciliter l'oblitération de cette fistule stercorale. Quand la malade quitta l'hôpital, le 29 Février, la cicatrisation était absolument complète, et la santé de la malade on ne peut plus satisfaisante. Il est évident que, chez cette femme, l'anse qui habitait ordinairement le sac herniaire, était une anse normalement voisine de ce sac, c'est pour cela qu'il a pu y avoir perforation intestinale, sans épanchement stercoral dans le péritoine. Ce sont sans nul doute des faits de ce genre qui ont engagé, il y a quelque temps, un chirurgien déjà connu pour d'ingénieuses créations opératoires, M. Bourguet d'Aix, à tenter une pratique qui nous paraît mériter la plus sérieuse attention. M. Bourguet, en présence d'une anse intestinale étranglée et frappée d'une gangrène limitée, au lieu d'établir un anus contre nature ou d'abandonner l'anse intestinale dans la plaie, en opère la réduction. Mais il a soin de fixer cet intestin malade à la paroi abdominale à l'aide de fils à suture. Ces fils sont placés de manière à ce que le point gangrené soit en rapport direct avec l'orifice herniaire, c'est à dire avec le collet du sac. Si lorsque l'escharre se détache, les matières fécales tendent à s'écouler à l'extérieur, au lieu de se répandre dans le péritoine, elles s'échappent à travers le collet. Grâce à cette ingénieuse pratique, que je me propose de suivre, le cas échéant, M. Bourguet a obtenu en moins d'un mois la guérison, dans trois circonstances, où, selon les anciens errements, on aurait sans nul doute établi un anus contre nature. Nous allons résumer ici en quelques lignes ces trois faits ; mais tout en félicitant le chirurgien d'Aix, nous lui demanderions volontiers pourquoi il n'a pas opéré plus tôt ces malades, pourquoi, avant de se décider à l'intervention opératoire, il a tenté tous ces moyens de douceur (belladone, antiphlogistiques, &c.), dont, mieux que nous, il connaît l'absolue inefficacité. Voici du reste en quelques mots l'histoire de ces malades.

Observation 1.—*Hernie inguinale congénitale étranglée ; gangrène de l'intestin ; adhérences anciennes de l'anse intestinale au sac et à l'épiploon ; dissection de ces adhérences, rentrée de l'intestin dans le ventre, avec fixation en face de l'ouverture herniaire ; fistule stercorale consécutive ; guérison.*—Cette observation a trait à un malade de 24 ans. La hernie s'étrangle le 22 Mai, et il n'est opéré que le 25 au soir après avoir sub plusieurs tentatives de taxis ; après avoir été purgé et lavementé. "Les diverses adhérences qui unissent l'intestin et l'épiploon entr'eux et avec la surface interne

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du sac sont détruites avec précaution au moyen du doigt, du bistouri et des ciseaux." 'Quand le malade avait vingt ans, l'existence de la hernie et son irréductibilité avaient été constatées par le conseil de révision, qui pour ce motif l'avait exempté du service militaire." (Il s'agit donc bien dans ce cas d'une hernie ancienne et d'un sac habité toujours par la même anse intestinale.)

Observation 2.—Hernie crurale étranglée depuis trois jours, gangrène circonscrite de l'intestin; kélotomie, avec fixation de la portion gangrenée à la face profonde de la plaie; fistule stercorale; guérison.—L'opération a été pratiquée soixante-dix heures après le début des accidents d'étranglement. Ici encore il y a eu plusieurs tentatives de taxis même tardivement pratiqué. (Il n'est pas question dans l'observation d'adhérences entre l'intestin et le sac, mais on ne parle pas non plus de la corde mésentérique, perceptible à travers la paroi abdominale. Or, je crois que ce symptôme prouve que l'anse étranglée siège ordinairement très-loin de l'anneau.

Observation 3.—Hernie crurale entéro-épiploïque étranglée depuis soixante-quatre heures, adhérences anciennes d'épiploon à la face interne du sac; destruction de ces adhérences; débridement avec réduction et fixation de l'anse intestinale; guérison sans fistules stercorales.—Le seul titre de cette observation en dit assez, croyons-nous, au point de vue de la thèse que nous soutenons. Il s'agissait dans ce cas d'une anse intestinale présentant des adhérences anciennes, c'est-à-dire d'une anse intestinale habitant ordinairement le sac où elle a été étranglée.*

Or, pourquoi dans ces trois circonstances, M. Bourguet a-t-il sauvé ses malades? C'est parce qu'il a eu affaire à des anses intestinales habitant depuis longtemps les sacs qu'il a dû ouvrir. Les adhérences manifestement anciennes qu'il a eues à détruire avec les ciseaux et les doigts sont là pour le prouver.

Que serait-il arrivé s'il avait eu affaire à une anse intestinale située ordinairement dans le petit bassin ou vers le foie? Il est probable que les deux fils de soie auxquels il a cru pouvoir se fier, n'auraient pas résisté au poids de la masse intestinale. L'anse perforée aurait fui, il y aurait eu épanchement stercoral.

Des adhérences inflammatoires épaisses se peuvent rompre même plusieurs jours après l'ouverture d'un sac gangreneux, s'il s'agit d'une anse ordinairement éloignée du sac. Il faudrait donc fixer solidement, avec plusieurs points de suture, l'anse douteuse, si l'on voulait suivre la méthode de M. Bourguet. En tout cas ses trois intéressantes observations sont venues démontrer toute l'importance pratique du fait clinique nouveau que nous donnons comme conclusion de ce chapitre:—L'étranglement herniaire est d'autant plus dangereux que l'anse étranglée a son siége habituel plus loin de l'anneau constricteur.

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* De l'immobilisation de l'anse intestinale dans quelques opérations graves d'hernie étranglée, par M. le Dr. Bourguet d'Aix: *Archives Générales de Médicine*, Novembre, 1879, p. 336.

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De la Laparotomie et de la Cystorraphie dans les plaies perforantes intra-péritonéales de la vessie.

Nouvelles recherches expérimentales et déductions cliniques.

Dr. E. Vincent, Lyons.

Avant de communiquer au Congrès les résultats de mes dernières expériences sur les perforations de la vessie, je crois devoir rappeler brièvement mes travaux antérieurs sur le même sujet.

Le 28 Mars, 1881, j'ai eu l'honneur de présenter à la Société Nationale de Médecine de Lyon les vessies perforées, suturées et guéries des chiens que j'avais soumis à une expérience remontant à six semaines en moyenne.

Voici ces pièces anatomiques. Le 5 Avril, 1881, j'ai fait soutenir à la Faculté de Médecine de Lyon, par l'un de mes élèves, une thèse inaugurale ayant pour titre, "Contribution à l'étude des traumatismes de la vessie."

Cette monographie importante contient le résumé de mes expériences (pp. 16 à 36) et un recueil assez complet des observations publiées de traumatisme proprement dit du réservoir urinaire.

Au Congrès d'Alger (Association Française pour l'Avancement des Sciences; Congrès d'Alger du 14 au 21 Avril, 1881) j'ai adressé une note intitulée, "Recherches expérimentales sur la laparotomie et la cystorraphie dans les plaies pénétrantes intra-péritonéales de la vessie, avec applications à la taille hypogastrique."*

Cette note renfermait les principales conclusions d'un mémoire écrit en Février, 1881, et que la *Revue de Chirurgie* d'Ollier et de Verneuil n'a pu insérer qu'en Juin et Juillet, 1881. Ce mémoire a pour titre: "Plaies pénétrantes intra-péritonéales de la vessie. Expériences relatives à leur traitement par la laparotomie et la cystorraphie."†

Ce travail se divise en deux parties: la première est consacrée à l'historique du traitement des perforations de la vessie. J'y relate notamment les observations de Walter de Pittsburg, de Willett et de Heath. La seconde partie contient quelques types de mes expériences de suture immédiate ou retardée dans les divers genres de traumatismes, le manuel opératoire, et enfin un paragraphe de conclusions et déductions. Si le temps le permettait, je lirais les conclusions que mes dernières expériences n'ont fait que corroborer. Elles peuvent, du reste, se résumer en quelques mots:

(1) La suture vésicale immédiate bien faite réussit presque toujours, même dans les perforations par armes à feu. (2) La laparotomie et la suture retardées réussissent le plus souvent dans les dix premières heures. (3) Il faut pratiquer la laparotomie et la suture vésicale le plus tôt possible dans les cas de perforation de la vessie suivie d'épanchement d'urine dans la cavité péritonéale. (4) Avec les garanties données par la méthode antiseptique et notre procédé de suture à double pilon avec adossement très-large, n'est-il pas permis de recourir plus souvent à la taille sus-pubienne? Celle-ci ne devrait-elle pas être la taille de choix?

Telles sont en substance les conclusions auxquelles m'avaient conduit mes premières expériences (sur les chiens). Dans la seconde série d'expériences dont j'apporte les résultats au Congrès de Londres j'ai voulu répondre à l'objection qui

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* Compte-rendu du Congrès d'Alger, 1881; et *Gazette hebdomadaire.*

† *Revue de Chirurgie* d'Ollier et Verneuil, 1881, Juin, pp. 449–464; Juillet, pp. 556–580.

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m'a été faite d'avoir choisi des animaux à péritoine peu sensible. Cette sensibilité varie avec les races et, dans une même race, suivant l'alimentation et les conditions hygiéniques. Chez des lapins bien nourris, bien soignés, j'ai trouvé une résistance infiniment plus grande que chez les chiens placés dans des conditions différentes. Ce fait important ressort avec évidence de la comparaison de mes deux séries d'expériences.

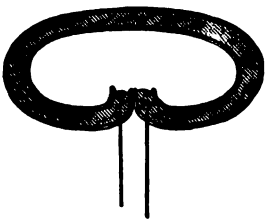
Voici mes nouvelles expériences sur les lapins. Pour le manuel opératoire et la suture, je renvoie au mémoire précité.

EXPÉRIENCES.—A. *Suture immédiate, perforation compliquée de la vessie.*

La suture immédiate réussissant presque toujours, je n'en rapporte pas d'exemple. Dans deux expériences j'ai compliqué la perforation vésicale d'autres traumatismes viscéraux graves.

Expérience 12.—*Laparotomie, cystotomie, colotomie et hystérotomie; suture immédiate de toutes ces solutions de continuité; guérison.*—Vieille lapine grise à oreilles déchiquetées; 24 Juin, 1881, 2 h. soir, lavage préalable phéniqué du ventre; laparotomie sus-pubienne; extraction de la vessie, dont un fragment de la dimension d'une pièce de 2 francs est enlevé sur la paroi antérieure; large perforation du colon et de l'utérus. La vessie pleine a déversé une grande partie de son contenu dans la cavité abdominale. Les solutions de continuité de la vessie, de l'utérus et du gros intestin ont été suturées en l'espace d'une heure, pendant laquelle ces organes sont restés dehors et baignant dans l'urine, le sang et des matières fécales. Au moyen d'une éponge phéniquée, nettoyage de toutes les impuretés. Dans cette expérience, comme dans toutes celles qui suivent, je n'ai fait qu'un plan de suture à la manière de Lembert, en me servant d'un fil de fer très-fin (No. 36), dont chaque anse est coupée en ras du nœud et abandonnée dans le ventre. Suture métallique de la boutonnière abdominale. Le 4 Juillet, sacrifié par piqûre du bulbe. A l'autopsie, nous trouvons les trois organes réunis entr'eux par des adhérences. Il y a du pus enkysté entre le gros intestin et la vessie. Celle-ci qui a exercé la fonction sans interruption, est pleine d'urine. On a de la peine à retrouver la suture dont les fils sont recouverts et dissimulés par une couche d'exsudats. Même succès de la suture du colon et de l'utérus.

Fig. 43.



Suture après Lembert. Modifié.

Expérience 13.—*Laparotomie; perforation large—de la vessie; extirpation du rein gauche; suture immédiate de la vessie; mort de péritonite.*—Vieille lapine grise; le 27 Juin, 1881, lavage phéniqué préalable du ventre; le rein a été extirpé par la boutonnière sus-pubienne; j'ai lié son pédicule avec du fil métallique et l'ai sectionné en suite en deçà; pas une goutte de sang; épanchement d'urine, hernie d'intestin. Nettoyage, suture, réduction comme ci-dessus. Après l'ablation du rein, l'animal a éprouvé des secousses générales comme s'il eut été violemment électrisé. Prostration très-grande pendant cinq à six heures. Le 1er Juillet, il a crevé dans la nuit. A l'autopsie je trouvai la suture vésicale bien réunie, mais néanmoins tous les signes d'une péritonite, dont le point de départ a été le siége du rein enlevé.

* *Revue de Chirurgie*, Juillet, 1881.

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B. *Suture retardée.*

Dans nos expériences sur les chiens, nous avons vu que l'opération n'a pas été suivie de guérison au delà de 8½ h. (Exp. 18). Savoir que la suture immédiate réussit à coup sûr est déjà un résultat expérimental important, mais il est d'un intérêt pratique plus grand encore de savoir pendant combien de temps on a chance de réussir en recourant à l'opération de Walter et Willett. Nous nous sommes appliqués à déterminer ce temps dans les expériences qui suivent; expériences que nous rapportons d'une façon très-abrégée. On verra que nos résultats tendraient à faire admettre que le lapin a un péritoine beaucoup plus tolérant que celui du chien. J'admettrais plus volontiers l'heureuse influence d'une bonne alimentation et d'un milieu sain. Mes chiens étaient dans les chenils provisoires du laboratoire de physiologie de la Faculté, tandis que mes lapins étaient dans une pièce dépendante de mon appartement.

Expérience 1.—Gros lapin; 8 Juin, 1881, à 5 h. du soir, laparotomie sus-pubienne; excision d'un fragment de 2 cent. de long sur 1 de large à la face antérieure de la vessie. Réduction de celle-ci non suturée; suture temporaire de la boutonnière ventrale. A 8 h. du soir, c'est à dire trois heures après la perforation, le ventre est rouvert, la vessie retirée et suturée. Expression de l'urine épanchée dans le ventre; étanchement à l'aide d'une éponge. Suture métallique de la boutonnière ventrale. Le 18 Juin, l'animal qui a toujours été bien, est sacrifié. Nous lui trouvons la vessie pleine, point de péritonite, une adhérence de la vessie à une anse d'intestin.

Expérience 2.—Vieux lapin, très-gras; 11 Juin, 1881, 2 h. du soir, laparotomie; excision de la paroi antérieure de la vessie sur la largeur d'une pièce de 2 francs, piqûres multiples du gros intestin; excision d'une portion de vésicule séminale (grave); réduction de tous les viscères herniés, pas de suture, beaucoup d'urine et de sang épanchés dans la cavité abdominale. A 8 h. du soir, c'est à dire six heures après la perforation compliquée, réouverture du ventre; expression des liquides épanchés en grande quantité; suture de la perforation vésicale béante, réduction, &c. Le 12 Juin, soir, crevé. A l'autopsie je trouve des matières fécales s'échappant par les piqûres du gros intestin; pas de liquide dans le ventre; la suture de la vessie tient parfaitement.

Expérience 3.—Gros lapin, d'âge moyen; 11 Juin, 1881, 2½ h. du soir, laparotomie, cystectomie (1 franc), sur la paroi antérieure de la vessie, hernie d'anses intestinales; réduction sans suture, suture temporaire de la boutonnière ventrale. 11½ h. du soir, c'est à dire neuf heures après la perforation, réouverture du ventre, suture métallique de la vessie; expression de l'urine épanchée, suture définitive de la boutonnière ventrale. Le 23 Juin, l'animal qui s'est bien porté, est sacrifié; vessie libre et pleine; il existe une plaque pseudo-membraneuse sur le rectum; rien ailleurs; pas de liquide dans le ventre; suture de la vessie parfaite. Dans cette expérience, nous avons dépassé la limite de survie atteinte chez le chien (neuf heures à huit heures et demie). Dans les expériences qui suivent, cette limite a été poussée plus loin encore, jusqu'à quarante-huit heures. De telle sorte qu'il semble que l'impressionnabilité du péritoine varie plutôt avec les individus et les conditions ambiantes qu'avec les races et les espèces.

Expérience 4.—Vieille lapine; 15 Juin, 1881, 7 h. du soir, laparotomie, incision de la vessie (2 centim.), plein écoulement de l'urine et du sang dans le ventre réduction de la vessie non suturée; suture temporaire de la boutonnière ventrale. Le 16 Juin, à 6 h. du matin, c'est-à-dire douze heures après la perforation,

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réouverture du ventre, expression du liquide, suture de la vessie. Le 17, crevé dans la nuit. A l'autopsie aucun signe de péritonite; mort par intoxication urineuse; suture vésicale en bon état.

Expérience 5.—Le 15 Juin, 8 h. du soir, cystectomie (1 franc); comme ci-dessus réouverture du ventre et suture de la vessie, douze heures après; crevé dans la nuit du 17. A l'autopsie on trouve la vessie contenant un peu d'urine, suture tenant bien, pas de liquide dans le ventre ni d'exsudats; crevé du choc opératoire péritonéal. Dans les expériences qui précèdent nous n'avons pris aucune précaution antiseptique. Dans celles qui suivent nous avons eu soin de désinfecter les éponges et les instruments par l'immersion dans de l'eau phéniquée, et en outre de laver, avant et après les opérations, le ventre du lapin avec de l'eau phéniquée faible.

Expérience 6.—Jeune lapin; 17 Juin, 1 h. soir, lavage phéniqué du ventre, laparotomie, perforation (2 centim.) de la vessie; issue d'urine qui coule en quantité dans le ventre; réduction de la vessie non suturée; suture temporaire de la boutonnière ventrale. 18 Juin à 1 h. du matin, c'est à dire douze heures après la perforation, réouverture du ventre; la perforation a diminué d'étendue par un commencement d'union spontanée à l'une de ses extrémités; expression du ventre, qui contient peu de liquide; suture de la vessie, &c.; guérison. Le 27 Juin, animal sacrifié; vessie pleine, suture parfaite, pas trace de péritonite.

Expérience 7.—Jeune lapin; 17, Juin 1½ h. soir, lavage phéniqué du ventre, laparotomie; perforation de la vessie (1½ cent.); urine écoulée en grande partie dans le ventre; hernies de nombreuses anses d'intestin qui ont baigné dans l'urine; réduction de la vessie non suturée, &c. Le 18 Juin à 1½ h. du matin, c'est-à-dire douze heures après la perforation, réouverture du ventre; vessie trouvée contenant un peu d'urine; perforation diminuée comme dans le cas ci-dessus, mais restée béante; suture de la vessie; expression des liquides du ventre en petite quantité; suture définitive de la boutonnière abdominale. Le 27 Juin, animal, qui s'est bien porté, est sacrifié. Vessie pleine, suture parfaite, nulle apparence qu'il y ait en péritonite.

Expérience 11.—Jeune lapine blanche; 18 Juin, 1881, 5 h. du soir, laparotomie, large incision de la vessie, dont le contenu très-abondant s'écoule dans le ventre; à la suite de cette inondation péritonéale l'animal est pris d'agitation et d'effarement à un degré extrême; suture temporaire de la boutonnière ventrale. Le 19 Juin, à 7½ h. du matin, c'est à dire quatorze heures et demie après la perforation réouverture du ventre qui contient très peu de liquide. Et cependant la vessie présente un trou dans lequel le petit doigt s'introduit aisément; elle contient de l'urine en assez grande quantité; suture de la vessie, &c.; guérison. Le 28 Juin, animal sacrifié. La vessie offre réparation parfaite; pas de péritonite.

Expérience 8.—Très-jeune lapin gris; 18 Juin, 1881, à 2½ h. du soir, excision d'un fragment de la paroi vésicale (pièce de 50 centim.); tout le contenu de la vessie pleine s'écoule dans la cavité abdominale; suture temporaire de la boutonnière du ventre; animal paraît foudroyé. Le 17 Juin, 7 h. matin, c'est à dire dix-sept heures après la perforation, réouverture du ventre; il s'échappe des flots de liquide. Le lapin faisant mine de vouloir crever, je lui fais des injections sous-cutanées de cognac qui le raniment; suture de la vessie; crevé deux heures après.

Expérience 9.—Jeune lapine grise; 28 Juin, 1881, 3 h. soir, perforation de 2 cent.; urine écoulée en grande partie dans le ventre. Le 19 Juin, 11 h. du matin, c'est-à-dire vingt heures après la perforation, réouverture du ventre; je trouve une réunion spontanée de la perforation par prolifération de la tunique séreuse; la vessie

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contient de l'urine en assez grande quantité et peut l'évacuer ; trois points de suture pour soutenir cette adhésion spontanée ; guérison.

Expérience 10.—Lapin blanc, jeune ; 18 Juin, 1881, 4½ h. du soir, excision large ; la majeure partie de l'urine se déverse dans la cavité abdominale. Le 19 Juin, 10½ h. du soir, c'est à dire trente heures après la perforation, réouverture du ventre. Je trouve la vessie peu distendue ; une réunion spontanée se formait au niveau de la perforation ; en l'examinant, j'en ai déterminé la rupture ; suture vésicale, &c. ; guérison. Ces deux faits nous apprennent que la cicatrisation peut quelquefois se faire spontanément, et que le péritoine peut être inondé par une grande quantité d'urine sans que pour cela son inflammation doive s'ensuivre forcément.

Expérience 14.—Lapin blanc croisé, oreille fendue ; 7 Juin, 1881, 4½ h. du soir, lavage phéniqué du ventre ; laparotomie ; large excision de la vessie ; inondation de la cavité péritonéale par l'urine, dont la quantité est énorme ; pas de suture vésicale ; suture temporaire de la boutonnière ventrale. Le 29 Juin, 4½ h. du soir, c'est-à-dire quarante-huit heures après la perforation, réouverture du ventre, qui contient un peu de liquide sanguinolent ; adhérences pariétales de la vessie ; La perforation est béante ; la tunique muqueuse boursouflée s'en échappe en formant un bourrelet qui laisse passer l'urine, en petite quantité, il est vrai, mais qui s'oppose absolument à toute cicatrisation spontanée ; suture de la perforation après avoir refoulé la hernie de la muqueuse ; les fausses membranes de la surface de la vessie sont enlevées avec l'éponge, &c. ; suture définitive de la boutonnière abdominale ; guérison. Sacrifié le 17 Juillet, 1881. Vessie pleine ; suture parfaite, comme toujours, les anses du fil métallique sont dissimulées par des produits plastiques.

Expérience 15.—Lapin blanc croisé ; 27 Juin, 1881, 5 h. du soir, large perforation de la vessie dans le ventre ; inondation urinaire du péritoine ; suture temporaire de la paroi abdominale. Le 29 Juin, 5 h. du soir, c'est-à-dire quarante-huit heures après la perforation, réouverture du ventre. La perforation est trouvée béante comme dans le cas précédent ; hernie de la tunique muqueuse qui fait office d'obturateur incomplet, ne laissant sortir à la fois qu'une faible quantité d'urine à mesure qu'elle est excrétée, urine qui doit être résorbée à mesure aussi par le péritoine. Des fausses-membranes recouvrent la surface de la vessie et des vésicules séminales ; je les enlève en raclant avec une spatule, puis en essuyant fortement avec une éponge phéniquée. Suture de la perforation vésicale ; expression des liquides &c. ; suture définitive de la boutonnière abdominale. Sacrifié le 17 Juillet. Adhérences de la vessie à une anse d'intestin, et à la paroi abdominale ; elle est pleine, et la cicatrisation de la perforation est parfaite.

La mortalité brute pour ces quinze expériences a été de cinq sur quinze, soit d'un tiers. Il faut éliminer le No. 13, qui a crevé de sa néphrotomie, et le No. 2, qui a crevé des suites d'un épanchement des matières fécales. Restent seulement trois morts des suites d'une perforation vésicale ; le No. 4 crevé d'intoxication urineuse, sans péritonite manifeste ; le No. 5, crevé du choc opératoire péritonéal ; le No. 6 crevé d'intoxication urineuse et de choc péritonéal.

Conclusions.

De cette nouvelle série d'expériences pratiques sur des lapins, dont le péritoine passe pour être particulièrement sensible, il nous semble découler :—

1.—Que le contact de l'urine avec le péritoine n'est pas aussi fatalement grave qu'on le suppose, puisque douze lapins sur quinze ont survécu à une inondation urinaire intra-péritonéale momentanée ou prolongée :

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2.—Que la suture vésicale avec adossement séreux, composée d'anses métalliques indépendantes et abandonnées dans l'abdomen (suture perdue) ayant toujours réussi, peut être employée avec la presque certitude du succès ;

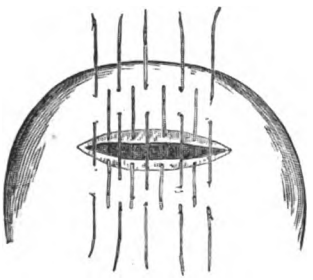
3.—Que dans les cas de solutions de continuité intra-péritonéale de la vessie, quelle qu'en soit la cause, suivies d'épanchement d'urine dans la cavité séreuse, on sauve presque avec certitude le sujet en procédant, immédiatement ou au bout de peu de temps, à la suture vésicale précédée de laparotomie, et cela malgré même des complications graves ; (Exp. 7, dans la même séance hystérotomie et hystérographie, colotomie et coloraphie, cystotomie et cystoraphie.)

4.—Que l'on peut encore sauver le sujet auquel on a fait une perforation intra-péritonéale de la vessie avec épanchement d'urine en ne recourant à l'ouverture du ventre et à la suture qu'au bout d'un certain temps. (Exp. 1, succès, trois heures après. Exp. 3, succès, neuf heures après. Exp. 6 et 7, succès, douze heures après. Exp. 11, succès, quatorze heures et demie après. Exp. 14 et 15, succès encore, quarante-huit heures après.)

5.—La possibilité de la cicatrisation spontanée des perforations vésicales est démontrée expérimentalement même chez les animaux à péritoine sensible (Exp. 9 et 10). Mais la cicatrisation spontanée n'étant pas la règle (deux fois sur quinze), il est indiqué de recourir à l'opération curative (laparotomie, nettoyage du péritoine et suture vésicale) dans le plus bref délai, puisque cette opération faite de bonne heure est à peu près toujours couronnée de succès, du moins chez les animaux. Invoquant les expériences qui précèdent et dont les résultats corroborent puissamment ceux de nos expériences antérieures, nous demandons s'il n'est pas permis de formuler les propositions suivantes, en faisant toutes les réserves que la clinique humaine est en droit d'exiger.

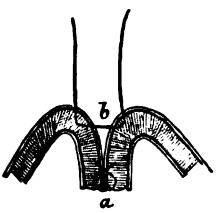
6.—Etant donné l'inexorable léthalité des perforations intra-péritonéales de la vessie, il faut en présence de cet accident recourir à la laparotomie et à la suture vésicale, comme à l'unique moyen rationnel de salut. Notre procédé de suture vésicale à double plan séreux adossée* donne les garanties les plus certaines d'imperméabilité et de résistance. Il permet à la vessie de récupérer immédiatement ses fonctions de réservoir et met à l'abri de la continuation ou du retour de l'épanchement.

Fig. 44.



Vue de face.

Fig. 45.



Vue sur une coupe.

Cette opération, qui n'est autre que l'opération de Walter et de Pittsburg, complétée par Willett et Heath, doit être exécutée le plus tôt possible. Les chances

* Voyez *Revue de Chirurgie*, Juillet, p. 558, et suiv.

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de succès au point de vue de la vie sont en rapport inverse avec le temps qui s'est écoulé depuis l'accident.

Nous le redisons encore, pour nous résumer, de par la clinique et de par l'expérimentation : la laparatomie et la suture vésicale doivent être hâtives comme la kélotomie.

7.—Si les précautions antiseptiques de pansement listérien ont véritablement diminué les dangers des opérations ayant le péritoine pour théâtre ; si, d'autre part, mes expériences paraissent avoir démontré qu'on peut réaliser une suture solide très-sûre des solutions de continuité de la vessie (par instrument tranchant, par armes à feu, par rupture ou déchirure, &c.) n'est-il pas permis de proposer un retour vers l'emploi de la taille sus-pubienne, ne doit-on pas préférer ce procédé de taille (le danger de la péritonite, des infiltrations, et des fistules étant écarté à peu près sûrement) à tous les procédés de cystotomie périnéale ? Ne doit-on pas songer à se débarrasser de l'antique fatras des lithotomites et à ne garder à l'avenir pour l'extraction des calculs de la vessie que deux moyens ; la lithotritie pour les calculs friables ou de petit volume, et la taille hypogastrique pour les calculs volumineux ou d'une dureté extrême ?

Pour acquérir quelque crédit auprès des praticiens, les faits de médecine expérimentale ont besoin de la sanction de la clinique humaine. Ce complément de preuve, je ne puis l'apporter encore. Je n'ai eu qu'une seule fois l'occasion de pratiquer en clinique humaine la suture de la vessie. J'ai suivi naturellement le procédé que j'ai décrit,* et le succès de la suture a été complet. Il s'agissait d'une incision du sommet de la vessie faite en pratiquant une ovariotomie pour un kyste multiloculaire adhérent aux organes du bassin. La perte de substance de la vessie équivalait à une pièce de 5 francs, les lèvres rapprochées mesuraient 7 centimètres de longueur. La vessie, qui avait été vidée au commencement de l'opération ne contenait pas d'urine, par conséquent, il ne s'en est pas épanché dans le ventre. Le plan profond de la suture a été fait avec du catgut, le plan superficiel avec du fil métallique fin, tous les chefs des anses ont été coupés ras et les anses abandonnées librement avec la vessie. Pour m'assurer des conditions mécaniques d'imperméabilité de la suture, j'ai poussé avec force une injection de lait qui a distendu l'organe sans filtrer en aucun point. Cette complication a fait durer l'opération d'ovariotomie une heure de plus. Pendant les premières heures qui ont suivi, la malade s'est plainte d'une sensation de chaleur en urinant, et l'urine était un peu teintée en rouge ; dans la soirée, l'urine avait repris sa couleur habituelle.

Nous n'avons pu maintenir en place une sonde à demeure à cause du spasme que sa présence déterminait. Au bout de quarante-huit heures la fréquence des mictions avait disparu, et de ce côté tout se passait comme avant, ainsi que s'exprimait la malade.

La femme, âgée de 36 ans, très-faible, très-maigre, très-anémique est morte de péritonite le 6 Janvier. Nous avons recueilli sa vessie vingt-six heures après le décès. Malgré la fermentation cadavérique, la ligne de suture n'a point faibli sous le choc d'une forte injection que nous avons poussée dans la vessie. Inutile de dire que la cavité péritonéale ne contenait pas d'urine épanchée. Il n'y avait du reste de liquide d'aucune sorte dans la cavité abdominale. La péritonite avait eu pour point de départ la rupture des adhérences intestinales du kyste. Ce fait clinique confirme notre proposition, à savoir : que le succès est la règle dans la suture immédiate

* Mon Mémoire : de *la Revue de Chirurgie*, pp. 559, 550.

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bien faite de la vessie (voici la pièce anatomique). Je m'abstiens de plus amples commentaires.

N.B.—Si l'on veut réussir dans les expériences que nous avons instituées il importe de découvrir le moins possible les intestins, d'extraire la vessie par l'incision la plus étroite possible, et au moment, de la suture vésicale, de restreindre la toilette péritonéale à la simple expression des liquides épanchés en exerçant de douces pressions sur le ventre. Une éponge phéniquée, placée sur la boutonnière abdominale s'imbibe des liquides que les pressions convergentes amènent à ce niveau. Je ne me sers du *spray* à aucun moment, et me borne à des soins de propreté. Tous les animaux (chiens) dont j'ai étalé au dehors les intestins pour les pulvériser ou les laver avec de l'eau phéniquée ont succombé rapidement dans le collapse et l'algidité.*

Irrigationsapparate für die männliche Harnröhre.

Dr. Adolf Fischer, Buda-Pesth.

Um den ganzen Harnröhrentract gehörig mit einem continuirlichen Strome auswaschen, und sodann eine beliebige medicamentöse Flüssigkeit in die Harnröhre einspritzen zu können, liess ich folgenden Apparat construiren:

Einen 13 Ctm. langen, und 18 bis 20 (Charrière) dicken, metallenen Katheter (Figs. 46 und 47), welcher nur an seinem vorderen Ende in der Ausdehnung von 2½ Cmt. doppelläufig—(*à double courant*)—ist (*b*), und hier an der unteren Fläche eine grössere Oeffnung besitzt, (*c*) durch welche die eingegossene Flüssigkeit abfliessen kann. Zu beiden Seiten des hinteren (visceralen) Endes der geraden Röhre befinden sich vier Oeffnungen (*d*). Der vordere Theil ist conisch (*ee*) und geht in eine concave Scheibe über (*f*), mit deren Hilfe der Abschluss der Harnröhrenöffnung besorgt wird. An der äusseren, d.i. convexen Fläche der Scheibe befinden sich zwei Hauptröhren (*gg*). Die obere (*g*) steht mit der ganzen Länge des Katheters in Verbindung, und führt die Flüssigkeit aus dem Irrigateur (*i*) gegen das hintere (viscerale), mit Oeffnungen versehene Ende, so dass hier die Flüssigkeit in die Harnröhre dringen kann, während die untere Röhre (g^1) mit dem unteren, von dem geraden Theile des Katheters abgesperrten Abzugscanale (Fig. 47 *mm*) communicierend, die in der Harnröhre angesammelte, nach vorwärts dringende Flüssigkeit ausfliessen lässt.

Um nach stattgehabter Auswaschung der Harnröhre, in dieselbe gewisse Medicamente eindringen zu lassen, ist in die obere Röhre ein zweites, kleines Rohr (*h*) eingeschaltet, welches mit einem kleinen, einen Hahn (*k*) in sich fassenden Kautschukrohre verbunden ist, in dessen Lumen eine grössere, mit langen Ansatzstücken versehene Spritze eingeschoben werden kann. Während der Auswaschung der Harnröhre wird dieses Kautschukrohr durch den Hahn abgesperrt. um die Flüssigkeit nicht ausströmen zu lassen. Ist es erwünscht, die Flüssigkeit in der Harnröhre eine gewis Zeit halten zu lassen, so steckt man das Ende des abführenden Kautschukrohres in den Hahn, und verhindert so das Abfliessen der Flüssigkeit.

Das obere Hauptrohr (*g*) des Instrumentes wird mit einem Irrigateur in Verbindung gebracht, und die gerade Röhre (*a*) desselben wie ein gerader Katheter bis

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* Voir mon Mémoire, Plaies pénétrantes de la Vessie, p. 576, No. de Juillet de *la Revue de Chirurgie* d'Ollier et de Verneuil.

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in den häutigen Theil der Harnröhre eingeführt, was durch die Zusammenfaltung des Gliedes mittelst der concaven Scheibe (*f*) leicht geschieht. Damit zwischen Harnröhrenmündung und Instrument keine Flüssigkeit ausströme, wird die Scheibe mit den Fingern der linken Hand gelinde gegen die Eichel gedrückt. Die

Fig. 46.

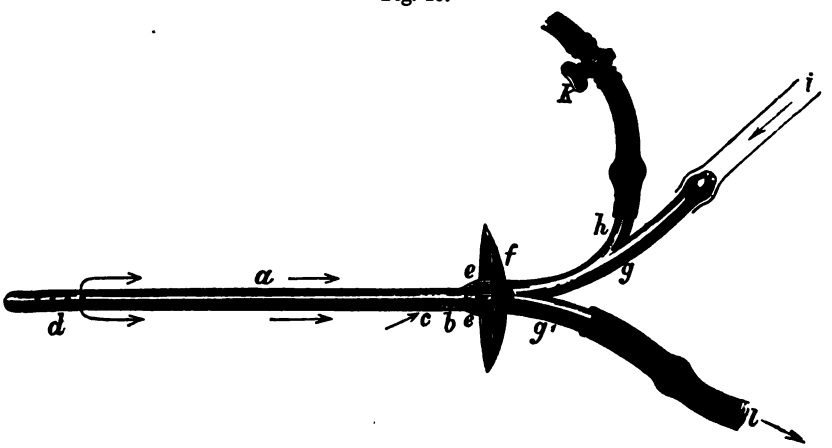
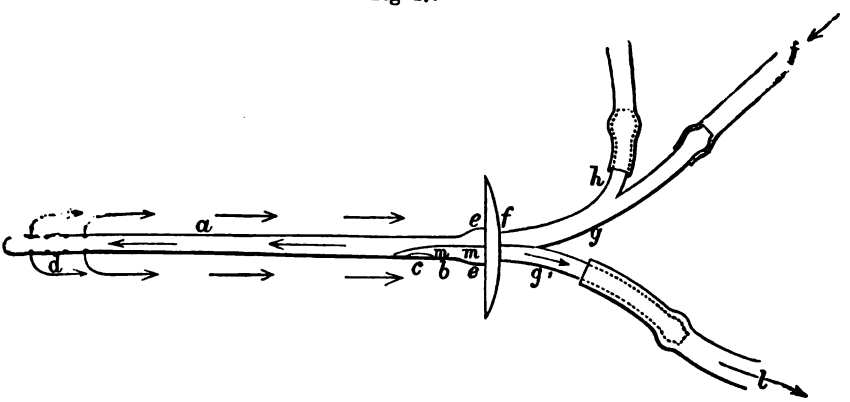


Fig. 47.



Urethral Irrigator.

Flüssigkeit springt so durch die kleinen Oeffnungen des visceralen Theiles in den hinteren Theil der Harnröhre and sammelt sich dem ganzen Verlaufe der Harnröhre entsprechend so lange an, bis sie, gegen die Harnröhrenöffnung dringend, die von der unteren Fläche des Instrumentes befindliche Oeffnung vorfindet und durch diese in das Abzugsrohr (g^1) gelangt.

Auf diese Weise wird der Inhalt der Harnröhre gründlich ausgeräumt, und letztere gehörig ausgewaschen. Wenn es wünschenswerth ist, nach der Auswaschung oder Abkühlung der Harnröhre in dieselbe ein Medicament einzuspritzen, so wird das mit dem Irrigateur verbundene Kautschukrohr mittelst eines Hahnes abgesperrt, während durch das schon beschriebene obere Rohr (*k*) eingespritzt wird.

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Nasenirrigateur zur Auswaschung der Nasenhöhle.

Dr. Adolf Fischer, Buda-Pesth.

Die bis jetzt gebräuchlichen Nasenspritzen und Nasenirrigateurs entsprechen bei Weitem nicht ihrer Aufgabe, wenn es sich namentlich darum handelt, beide Nasenhälften in continuo zu durchspülen, ohne dem Patienten Schmerzen oder sogar eine Otitis media zu verursachen.

Um diesen Eventualitäten vorzubeugen, und um die Nasenhöhle von rückwärts gehörig auszuspülen, construirte ich einen eigenen Nasenkatheter. Dieser besteht aus einer 13 Ctm. langen, im breiten Durchmesser 4 Mm. betragenden, geraden Röhre, welche am vordern Ende unter einem Winkel von 172½° in eine hakenförmige Krümmung übergeht, deren Länge 4½ Ctm., deren Breite 3 Mm. beträgt. An dem Ende dieser Krümmung befindet sich eine 6 Mm. breite, von 4 Oeffnungen durchlöcherte Rosette. Das hintere Ende des Katheters is trichterförmig und mit einem gerippten Rande versehen, welcher in ein, mit einem Glastrichter versehenes, 60 Ctm. langes Kautschukrohr geschoben wird.

Der gekrümmte Theil des Instrumentes wird hinter das Gaumensegel geführt und vom Patienten am Griffe festgehalten. Man giesst nun in den hochgehaltenen Trichter Flüssigkeit; der Patient neigt seinen Kopf nach vorwärts, wo dann das Eingegossene in ein bereit gehaltenes Gefäss fliessen wird. Will man ohne Unterbrechung die andere Hälfte der Nasenhöhle auswaschen, so dreht man den Griff so, dass die Rosette gegen die Oeffnung der auszuwaschenden Nasenhälfte gerichtet ist.

Die Herausnahme des Instrumentes kann leicht bewerkstelligt werden indem man den Griff von unten nach aufwärts hebt.

Blessures à l'arme blanche.

Dr. E. Albanese, Palermo.

Quoique le perfectionnement des armes à feu ait fait mettre hors d'usage, à la guerre du moins, les armes blanches, je crois à propos de faire ici un rapport succinct des observations pratiques que j'ai eu occasion de faire, dans cette dernière période de onze ans, sur les blessures produites par des armes blanches.

A mon ambulance clinique de l'hôpital de la Conception à Palerme, du 1e Janvier, 1870, au 31 Décembre, 1880, il s'est présenté 2,293 blessés à l'arme blanche. Les blessures qu'ils avaient reçues en rixe, en duel ou accidentellement, étaient au nombre de 3,483.

Pour qu'on ne soit pas étonné de ce chiffre, qui d'ailleurs représente à peine la moitié des blessés reçus dans le même espace de temps dans les hôpitaux de Palerme, il est bon de remarquer que les rixes, ou combats corps à corps, sont extrêmement fréquentes dans cette ville.

Ce peuple y a appris à donner le titre d'homme d'honneur à celui qui dans une occasion sait bien manier une arme et "se faire faire place," comme il dit dans son jargon, et à traiter d'infâme celui qui dénonce à la justice des actions sanguinaires ayant pour cause des offenses personnelles; un blessé, invité à parler, se tait, ou attribue au hasard la cause des blessures; effrayé de la mort et de la nécessité de décliner le nom de celui qui a frappé, il répond: "Qui meurt pardonne."

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Les armes blanches le plus communément employées par ce peuple peuvent se diviser en deux catégories principales selon qu'elles sont destinées à la défense personnelle, ou à l'offensive avec intention de tuer, ou bien de vider quelque querelle d'amour ou de venger une trahison amoureuse.

Dans les rixes qui ont presque toujours lieu en des endroits, à des jours et à des heures indiqués d'avance, on se sert du couteau catalan à ressort ou du couteau à manche fixe. Avec ce couteau, on ne cherche pas toujours à tuer, aussi ne frappe-t-on pas toujours à la poitrine ou à l'abdomen : les querelles légères se vident en visant aux muscles, c'est-à-dire aux extrémités supérieures ou inférieures ; on ne vise au tronc ou à la caisse (d'après leur jargon) que quand il s'agit de querelles de quelque importance.

Pour les querelles d'amour entre homme et femme, entre femmes ou entre hommes, on se sert du rasoir à manche fixe, en visant à la figure ou au cou de l'adversaire, afin que la blessure laisse une cicatrice qui défigure pour toujours.

Dans les vrais duels des classes supérieures, assez fréquents aussi, on se bat au sabre, très-rarement à l'épée.

Le poignard est généralement regardé comme une arme vile, dont les assassins se servent, mais bien rarement les autres ; ceux-là du reste n'ont rien de commun avec les prétendus hommes d'honneur qui se tuent volontiers pour une querelle personnelle, mais qui n'attaquent jamais un adversaire sans arme, et ne le prennent jamais en traître ; en effet, le premier mot qu'un homme insulté adresse à son adversaire est celui-ci : "Etes-vous à cheval?" c'est-à-dire "armé?" Et ce n'est que lorsque la réponse est affirmative que la rixe suit immédiatement l'offense. Dans le cas contraire, il donne toujours à son adversaire le temps de s'armer, car tout individu qui, même dans un accès de colère, se précipiterait une arme à la main contre une personne sans arme serait souverainement méprisé.

Comme je l'ai déjà dit, dans l'espace de onze ans, 2,298 blessés furent admis et soignés dans mon ambulance clinique, parmi lesquels 1,938 hommes et 360 femmes. Sur ce nombre 473 hommes et 36 femmes furent admis d'urgence à l'infirmerie à cause de la gravité de leur état. Il en mourut 62, dont 13, à peine arrivés à l'hôpital, d'hémorrhagie ou du collapsus, avant même d'avoir reçu aucun soin.

Sur tous ces blessés, 3,493 blessures furent constatées ; un seul individu en avait 75 sur le corps, produites par un petit poinçon ; deux en avaient chacun 21 ; un 19 ; trois autres en avaient 7 ; 17, chacun 6 ; 22, chacun 5 ; 200 en avaient 3 et 4 ; enfin 340 en avaient 2.

Ces blessures avaient été faites, savoir :—

2,916 . . .	par le couteau
427 . . .	par le rasoir
140 . . .	par d'autres armes, telles que sabres, poinçons, alènes, limes, compas, poignards, verre cassé
3,483 en tout.	

Ainsi :—

2,916 coups de couteau à . . .	2,042	individus
427 coups de rasoir à	228	„
140 par d'autres armes à . . .	28	„
3,483 sommes égales	2,298	„

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Les différents blessés, en raison du sexe et de l'arme, se divisent comme suit :—

à coups de couteau	H.	1,756	2,042
	F.	286	
à coups de rasoir	H.	159	228
	F.	69	
par d'autres armes	H.	24	28
	F.	4	
Total			2,298

Les blessures examinées en raison de la position ont été classées comme il suit :—

Tête et figure	860
Cou	156
Poitrine pénétrée	291
Poitrine non pénétrée	264
Abdomen pénétré	307
Abdomen non pénétré	155
Membres supérieurs	1,201
Membres inférieurs	249
Total	3,483

Les blessés admis dans les infirmeries au nombre de 510 étaient presque tous dans un état fort grave : 211 présentaient des lésions pénétrantes dans la cavité thoracique, 807 des blessures pénétrantes dans la cavité abdominale, et 96 dans les autres parties du corps des blessures compliquées avec hémorrhagie ; 27 fois les blessures avaient ouvert les deux cavités thoracique et abdominale dans le même individu, ainsi un des blessés avait 3 blessures profondes à la poitrine et à l'abdomen et 13 autres disséminées sur tout le corps ; à la figure, aux bras, &c.; un autre avait reçu 7 blessures qui pénétraient dans les deux cavités, et une très-grave au dos ; cinq avaient chacun 4 blessures, et un en avait une fort grave à l'aine avec lésion des vaisseaux.

Les ligatures des artères furent toutes faites selon la méthode directe et en passant toujours deux lacets, l'un à la partie périphérique, l'autre à la partie centrale ; l'artère radiale fut ainsi liée 4 fois ; 9 fois la cubitale ; 6 fois l'humérale ; 1 fois la poplitée, 2 fois la crurale ; 2 fois l'iliaque externe ; 1 fois l'iliaque interne ; 2 fois la carotide primitive ; 2 fois la faciale ; 4 fois les thyroïdiennes. Pas un seul cas d'hémorrhagie consécutive.

La mort s'en suivit en deux cas seulement : l'un à la suite de la ligature de la carotide primitive par pleuro-pneumonie et pyémie, et dans l'autre cas, par suite de la ligature de l'iliaque interne par collapsus.

Plusieurs blessés avec lésion des grosses artères arrivèrent à l'hôpital presque exténués et dans un état de véritable anémie. Un de ceux-là mourut avant qu'on eût pu lui faire la ligature.

On guérit plusieurs fois de graves anémies au moyen de la prompte transfusion du sang, faite quelquefois par les veines blessées, et en se servant dans quelques cas du sang recueilli momentanément des blessures encore saignantes.

En d'autres cas de collapsus par anémie aiguë, en aidant l'individu au moyen de la respiration artificielle, on reconnut comme très-utiles les injections hypodermiques avec la teinture éthérée de musc, déjà recommandée dan les anémies

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aiguës des accouchées par le professeur Breisky de Pragué, en injectant sous la peau jusqu'à 4 gram. de teinture éthérée dans une heure: 1 gram. chaque dix minutes.

La cure adoptée généralement pour toutes ces blessures fut la plus simple; dans tous les cas, au premier-abord, en ayant soin de nettoyer les blessures selon la méthode antiseptique, on rapprocha les bords au moyen de sutures entortillées ou à points détachés, et, dans les cas de petites lésions, en les tenant en contact au moyen de bandelettes de sparadrap. Dans les grandes blessures à lambeau furent pratiqués les petits drainages.

Toujours le repos et le calme furent recommandés, au blessé, et les bandages légèrement compressifs furent pratiqués.

Dès l'année 1870 les pansements furent faits selon la méthode antiseptique: les premières années, c'est-à-dire jusqu'en 1876, sans suivre complétement les règles recommandées par le Prof. Lister, puis en adoptant tous ses préceptes de médication.

Il est inutile d'ajouter que le but de la réunion par première intention fut presque toujours atteint sans le moindre inconvénient dans les blessures simples, que les complications locales et générales furent rarement remarquées; aussi l'erysipèle parut dans peu de cas et seulement dans la première période jusqu'en 1876. Mais cet accident si commun autrefois, lorsqu'on suivait la méthode antiseptique avec toutes les règles recommandées par le Prof. Lister, n'a plus été signalé dans mes infirmeries.

Dans les blessures compliquées la conduite suivie a été à peu près identique: c'est-à-dire en réunissant toujours les blessures par première intention avec la plus stricte cohésion des bords et ayant soin d'éloigner les complications. Cette méthode a toujours été pratiquée dans toutes les lésions, même dans les blessures pénétrant dans les cavités. Et j'ai à me louer de ce procédé parce qu'il a produit d'excellents résultats.

Les blessures même à la trachée, le plus souvent transversales sous la région hyoïdienne, provenant d'homicide ou de suicide, furent aussi toujours réunies par première intention par des points de suture détachée, et je confesse n'avoir jamais vu survenir la moindre suffocation.

Et dans les cas de graves blessures à lambeau au nez, aux oreilles, aux extrémités, en essayant la réunion immédiate, j'obtins de très-heureux résultats, ainsi on put obtenir un jour la réunion de la moitié du médium de la main gauche, qui pendait et n'adhérait plus que par un lambeau de peau d'un demi-centimètre; et plusieurs autres fois je réunis avec succès des fragments de nez, et des lobes d'oreilles presque entièrement détachés; cette pratique déjà en usage depuis des siècles dans les blessures à l'arme blanche, je la dois recommander aussi dans les blessures produites par armes à feu ou par des instruments contondants. Suivant toujours ce procédé, je me souviens avec satisfaction d'avoir pu réunir la main droite de M. le Duc Fici de Palerme, qui, dans un duel au sabre, avait été presque entièrement détachée du poignet.

Dans ce cas le poignet avait été presque complétement coupé avec les os et les tendons fléchisseurs et extenseurs, et la main pendante tenait encore à l'avant-bras par un lambeau de peau de la région dorsale d'environ 4 cent. Après avoir lié les deux artères radiale et cubitale dans la région du poignet, je pensai à tenir au moyen de points de suture entortillés cette main en contact, me servant pour l'immobilité de l'appareil à silicate de potasse.

Cette résolution me parut téméraire ainsi qu'au chirurgien Docteur Lo Cascio, mais, à notre grande surprise, la gangrène, apparue dans la région thénar, ne s'étendit

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pas plus loin, elle se borna même, quoique la température et la sensibilité de la main et des doigts se fussent abaissées sensiblement un temps bien long.

Ensuite la main resta soudée à l'articulation carpique avec mobilité de l'articulation radio-carpique et immobilité des articulations des doigts ; mais, un peu à la fois, les doigts acquirent quelque mouvement, ils peuvent à présent s'écarter et se rapprocher avec quelque agilité, si bien que le Duc peut se servir de sa main pour manger, pour écrire et conduire un cheval : il tient la fourchette, la plume et les rênes entre les doigts avec assez de force.

Et je dois aussi recommander la suture des nerfs et des tendons divisés dans les cas de lésion profonde des doigts, de la région inférieure de l'avant-bras et de lésion artérielle où presque toujours on trouve aussi coupé le nerf satellite de l'artère, car j'ai vu bien des fois se rétablir la fonction des tendons et la sensibilité après les sutures.

Parmi les blessures spéciales je dois signaler un cas de lésion de la moelle épinière déjà publié dans la *Gazette Clinique de Palerme*,* lésion produite par une arme très-fine et pointue, qui, pénétrant par la région massétérienne gauche, alla jusqu'au canal vertébral entre la première et la deuxième vertèbre, coupant deux tiers de la moelle épinière au niveau de l'articulation atloïdo-axoïdienne. Le blessé vécut 57 jours et mourut de tétanos, quand la blessure fut complétement cicatrisée, et les mouvements du corps en partie rétablis.

Pour les blessures pénétrant dans la cavité abdominale, l'emploi de la morphine par injections hypodermiques me réussit toujours, en injectant chaque fois, pour les adultes, de 1 à 2 cent., et tâchant dans les cas graves, avec principe de péritonite, de répéter les injections toutes les trois heures en employant un minimum de 5 centig. de morphine dans les premières dix heures.

Ce moyen curatif, je l'ai même employé comme prophylactique joint à l'usage local de la glace, ainsi j'ai procuré aux blessés des heures réconfortantes de repos et de calme, et rarement les péritonites diffuses, fréquentes dans ces blessures, ont été signalées et très-rarement ont été suivies de mort.

Ainsi dans cette période de onze ans et chez 307 individus atteints de blessures pénétrant dans la cavité abdominale, six fois seulement la péritonite simple diffuse fut cause de mort.

La mort dans les autres cas s'en suivit : 6 fois dans les premiers moments avant même de donner aucun secours, 6 fois dans les premières vingt-quatre heures par collapsus à la suite d'hémorrhagie interne ; 6 fois par péritonite perforative foudroyante ; 4 fois à la suite des complications des blessures simultanées des poumons et du cœur ; si bien qu'on a dû constater 27 fois l'ouverture simultanée de deux cavités thoracique et abdominale, 4 fois la blessure de l'estomac, 5 fois la blessure du foie, 4 fois celle des reins, 4 fois celle de la vessie, 25 fois celle des intestins, et bien que ces blessures fussent compliquées 60 fois de la sortie de l'épiploon et des anses intestinales.

Dans les cas compliqués de la sortie de l'épiploon, il fut toujours lié et coupé ras de la peau Les anses intestinales sorties furent toujours soigneusement lavées à l'eau phéniquée, et les sutures intestinales furent toujours faites à points détachés et perdus, soit qu'on employât la soie carbolisée ou le catgut.

Je crois que l'emploi de la morphine par injections hypodermiques doit avoir contribué à ces heureux résultats.

Parmi les blessures pénétrant dans la cavité thoracique soignées dans mes

* *Gazzetta Clinica di Palermo*, 1879, f. 1 e 2, anno xi.

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infirmeries au nombre de 291, sur 211 individus, 24 sont morts: 4, déjà signalés dans la catégorie des blessures abdominales, par péritonite perforative; 8 par blessure au cœur (six fois dans les premiers moments) 6, par hémorrhagie; 3 par empyème; 3 par pleuro-pneumonite.

Un blessé au cœur est mort après trois jours. Dans ce cas, déjà publié par mon aide clinique, le Dr. Argento,* le malade expira au bout de 58 heures après la lésion, par asphyxie à la suite d'hémothorax déjà remarqué dès le premier jour.

L'autopsie fit constater une grande accumulation de sang coagulé dans la cavité pleurale gauche, le péricarde ouvert de 4 cent. était adhérent au ventricule gauche par des exsudations fibrineuses, et vers la pointe du cœur une lésion de 2 cent. communiquant avec la cavité du ventricule gauche.

La blessure au cœur était comme tamponnée par un coagulum de fibrine en évidente organisation, de couleur grisâtre, et en partie adhérent aux bords en sorte que le ventricule pouvait fort bien contenir l'eau qui y fut versée à dessein.

Je dois mentionner l'usage des injections hypodermiques d'ergotine dans les blessures du thorax compliquées d'hémorrhagies pulmonaires, qui n'étaient pas rares chez nos blessés, car nous les avons observées 107 fois.

Les injections hypodermiques d'ergotine ont été faites selon la formule indiquée par le Prof. von Langenbeck dans la cure des anévrismes, elles ont été répétées avec succès jusqu'à quatre ou six fois dans un jour: chaque fois avec une petite seringue pleine de Pravaz.

J'ai toujours fait accompagner l'emploi de l'ergotine de l'application locale de la glace, en recommandant au malade le calme et le silence le plus complet.

Les exsudations pleurétiques et l'empyème furent presque toujours traités par la thoracentésis et dans le cas d'empyème le drainage fut employé avec des lavages répétés d'eau phéniquée ou de solution de permanganate de potasse.

Je crois que les observations que j'ai eu l'occasion de faire sur les blessures à l'arme blanche peuvent m'autoriser à recommander:—

1.—La réunion immédiate par première intention avec pansement antiseptique comme règle générale et absolue, même dans les énormes blessures à lambeau.

2.—La ligature des artères selon la méthode directe, en les liant au-dessus et au-dessous de la blessure, et en coupant ensuite le vaisseau entre les deux ligatures.

3.—La suture des grosses veines blessées, des nerfs et des tendons divisés.

4.—La ligature et l'immédiate excision de l'épiploon sorti par la blessure.

5.—Les sutures intestinales à points perdus.

6.—L'emploi des injections hypodermiques de morphine, même comme moyen prophylactique, dans les blessures pénétrantes de la cavité abdominale.

7.—L'emploi des injections hypodermiques d'ergotine dans des cas d'hémorrhagies des capillaires, et dans les pneumo-hémorrhagies.

8.—La transfusion du sang et les injections hypodermiques de teinture éthérée de musc dans les graves anémies et dans les syncopes en s'aidant de la respiration artificielle prolongée.

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* "Gazette Clinique de Palerme," année 1877.

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Des Calculs du Périnée.

Prof. Mazzoni, Rome.

Je désire vous entretenir sur le cas d'un calcul, développé au périnée, lequel ne manque pas d'un certain intérêt, tant du côté scientifique, que de celui pratique.

Le cas est le suivant:

Un nommé E. V., natif de la Fara, se plaignait depuis longtemps. et, à l'âge de 30 années, de difficulté à uriner, difficulté, qui au bout de cinq ans, s'était accrue de telle façon que les urines ne s'écoulaient qu'avec peine.

Dans l'intervalle, apparurent des tâches verdâtres, qui, s'étendant, allèrent occuper le périnée et portion du scrotum. L'escarre tombée, on observa au fond de la plaie un calcul volumineux, qui fut extrait. Le malade guérit, et passa plusieurs années en bonne santé, continuant cependant à être toujours un peu gêné dans l'acte d'uriner.

Mais il y a environ dix mois, les anciens phénomènes et souffrances apparurent de nouveau, et le malade se présenta à la clinique. Il est de constitution robuste, de forme athlétique, âgé de 60 ans.

A l'examen, on constate que le pénis est retiré, et, à la base du scrotum, vers le milieu du périnée, une cicatrice de forme irrégulière, en dessous de laquelle se trouve une tumeur ronde, dure, de la dimension d'une orange, la surface sillonnée. La tumeur est mobile superficiellement, mais intérieurement elle est adhérente à l'urètre. Au maniement, on éprouve la sensation d'une crépitation de corps à surfaces rugueuses qui se heurtent entre elles. Le cathéter pénètre dans l'urètre avec difficulté, et glisse sur un corps rude, à neuf centimètres du canal du méat urinaire.

Il est utile de noter que le malade est obligé, pour uriner, de déplacer la tumeur de la ligne centrale.

L'analyse des urines donne les résultats suivants:—teinte orange foncée, réaction acide, apparence troublée, dépôt abondant de corpuscules de pus, de cellules épithéliales et cristaux de phosphate triple.

Le diagnostic est clair : il s'agit d'un calcul périnéo-scrotal, probablement multiple.

Une incision faite sur la tumeur, le long du raphe, découvrit une poche de grande proportion, à parois très-épaisses, et dans laquelle furent trouvés soixante-neuf calculs de formes et dimensions diverses, mais articulés entre eux, de façon à former un corps unique, de figure sphérique, semblable à un kyste, qui fut extirpé dans sa totalité. L'extrémité inférieure de l'urètre, après grande fatigue, fut retrouvée dans la région plus basse, presque cachée sous un tissu cicatriciel. Le cinquième jour après l'opération, il se développa un phlegmon urinaire, qui, du périnée et du scrotum s'étendit aux aines, jusqu'au thorax, pour ne se terminer qu'aux aisselles.

Le malade mourut, le dixième jour après l'opération, de septicémie.

Cette déplorable fin me fit souvenir que de tels résultats sont en pareil cas trop fréquents.

De là, l'idée de recherches, le désir de trouver une méthode qui puisse offrir au moins l'espoir de diminuer le nombre des victimes.

Et cela, d'autant plus que les traités de chirurgie, tant généraux que spéciaux, soit antiques soit modernes, ainsi que d'intéressantes monographies sur cet argument enregistrent un certain nombre de cas de calculs périnéaux, mais tous les traitent beaucoup plus dans le sens de connaître la cause de leur formation, que d'en indiquer la thérapeutique.

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J'ai cru donc utile de revenir sur ce sujet important à mon avis, bien que Kocher l'ait déclaré être seulement une curiosité scientifique.

Je rappellerai donc que Le Dran, dans ses "Observations de Chirurgie," parle d'un grand calcul du périnée observé par lui en 1725.

Il pense que l'urètre, en se dilatant, se prête au développement de la pierre, et la guérison de la fistule consécutive est difficile.

Egalement Heister rapporte deux cas de calculs énormes du périnée qu'il opéra avec grande incision, et se plaint de la rareté des cas de guérison.

Mott rapporte un cas où le scrotum avait acquis un volume dix fois au-dessus de l'ordinaire, et descendait en forme de grappe, à petites proéminences, formées par les calculs urinaires.*

Chélius aussi, dans une vaste tumeur, située entre le périnée et le scrotum, trouva vingt-quatre calculs à facettes, de diverses grandeurs, composés de phosphate de chaux.

Ces deux malades moururent.

Louis est l'auteur de la première monographie sur cet argument, elle réunit dix histoires de calculs du périnée. Dans huit, le calcul était dérivé des suites de la cystotomie, le neuvième est causé par un coup de pied au périnée, le dernier aurait été la conséquence d'une fistule urinaire. De ces dix calculs, sept étaient du périnée, deux du scrotum, le dernier scroto-périnéal.

Morgagni† traite de deux autres, trouvés dans la tunique vaginale droite, et moi-même j'ai extrait du scrotum un calcul, qui n'avait aucune communication avec l'urètre. Je trouvai un cordon fibreux, à guise de trait d'union, entre le calcul et l'urètre.

Le Dr. Meli, sicilien, parle d'un Salvatore Lavoratore, lequel, une fois, après de graves souffrances dans le périnée et les voies urinaires, s'arracha de force, avec ses propres doigts un calcul à facettes, de forme pentagonale, et, après plusieurs jours, de nouveau, cinq autres égaux au précédent ; le tout sans qu'il s'en suivit échappement d'urine au périnée.

Le malade guérit très-vite. Il est nécessaire d'étudier la question relative à la cause d'origine, c'est-à-dire, si la formation des calculs a lieu dans l'urètre ou en dehors de ce dernier.

On sait comme Louis, dans le mémoire cité plus haut, est d'avis que le calcul périnéal se forme à la suite d'une fistule, et par conséquent, en dehors de l'urètre et des voies urinaires.

Au contraire, Monteggia pense que les pierres en dehors de l'urètre occupent à un temps donné le canal urétral, où elles se font lentement une niche, en dilatant d'abord, et perforant ensuite le conduit jusqu'à sa sortie de l'urètre.

Le professeur Porta de Pavie, dans un mémoire intéressant sur les calculs ensachés du périnée, soutient que le calcul au périnée est enkysté et que le kyste est formé de l'urètre dilatée. Il base son assertion sur les preuves suivantes : (1) La dilatabilité du canal de l'urètre. (2) La disparition de l'urètre dans les cas opérés et anatomisés, et la présence à sa place d'un sac avec cavité unique, situé sur l'axe de l'urètre. (3) La structure du sac. (4) La rétraction après l'extraction du calcul.

Le professeur Rizzoli donne des preuves à l'appui que le calcul au périnée peut se former également selon l'opinion de Louis ou l'avis de la Porta.

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* *Philadelphia Medical Journal*, vol. x.

† Epître 64, p. 7.

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Nous savons déjà que dans la fistule urinaire permanente il peut se former des concrétions par le jet continu de l'urine qui la traverse.

Ainsi quand l'ouverture fistuleuse vient de se boucher, le calcul reste en dehors de l'urètre.

De même un calcul dérivé de la vessie peut s'arrêter dans l'urètre, l'ulcérer, descendre dans les tissus cellulaires, se renfermer dans une poche, et y rester indépendant de l'urètre.

Ainsi s'explique l'existence de calculs dans le scrotum et dans le périnée sans communication avec l'urètre.

Après l'exposé et l'étude de ces documents, je crois pouvoir conclure : (1) Que les calculs du périnée peuvent venir directement de la vessie, et sortir du périnée sans donner lieu à l'écoulement d'urine. (2) Qu'ils peuvent se former, le long des fistules permanentes n'ayant aucune communication avec l'urètre. (3) Qu'ils peuvent être contenus dans un sac spécial formé des parois de l'urètre.

En parlant du traitement il résulte que, pour les calculs du périnée enkysté, il n'existe pas de méthode de cure correspondant à la gravité de la maladie, et, pour ce motif, je ne crois pas inutile d'avoir attiré l'attention sur mon nouveau mode d'opérer.

Le calcul donc que j'ai eu l'honneur de présenter appartiendrait à la troisième classe, parce qu'il est contenu dans un sac spécial, et ressemblerait sous ce rapport aux cas de Le Dran, Heister, Mott, &c. Mais mes observations anatomo-histologiques me persuadent qu'un cas analogue n'a été vraiment publié par aucun chirurgien, car les anciens manquaient de moyens d'investigation, et les modernes ne s'en sont pas encores occupés.

Donc, l'examen anatomo-histologique prouve que le kyste du calcul se compose de deux couches différentes, une épithéliale, l'autre conjonctive, unies entre elles, d'une manière qui rappelle la structure de la peau. Il apparaît donc que le réceptacle du calcul n'est ni la conséquence d'une fistule urétro-périnéale ni une dilatation consécutive de l'urètre, car pour la première hypothèse, il manquerait la couche épaisse épithéliale, et le reste de la paroi cystique serait composé de tissus cicatriciels sans limites nettes. Pour la seconde hypothèse, l'épithélium urétrale aurait dû disparaître, ou se réduire à un plan atrophique, à cause de la constante compression et irritation du calcul ; par conséquent, la surface interne n'aurait pas la structure d'un kyste particulier, mais d'un tissu d'infiltration inflammatoire et cicatriciel.

Dans notre cas, au contraire, outre une couche épithéliale très-épaisse, nous avons un tissu conjonctif ordinaire, mêlé de fibres élastiques, formant une paroi cystique bien distincte. Pour cela, j'ai bien raison de croire que dans le cas actuel, il s'agit d'un diverticulum urétral congénital, communiquant avec l'urètre, dans lequel la stagnation des urines a donné lieu à la formation des nombreux calculs que je vous ai présentés.

Il m'a paru utile d'appeler votre attention sur la pathogénèse des calculs du périnée parce que beaucoup de cas, décrits comme ectasie urétrale, à cause des calculs arrêtés dans l'urètre, ne sont en réalité que le résultat necessaire d'un diverticulum congénital de l'urètre.

Pour ce qui concerne la cure, peu ou rien se trouve écrit au sujet des calculs périnéaux, tant enkystés, qu'extra-urétraux. Il me semble, que toute la thérapeutique se soit limitée à l'extraction du calcul. Mais si cette opération donne des résultats heureux, elle en donne qui sont aussi graves qu'inattendus.

En effet, nous avons vu comme il y a des calculs qui se trouvent en dehors

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des voies urinaires. Dans ce cas, la guérison a lieu presque spontanément, à peine le calcul enlevé, et nulle conséquence n'est à craindre, parce que la cavité contenant le calcul n'a aucune communication avec le conduit de l'urètre. Au contraire, dans les calculs enkystés et communiquant avec l'urètre, l'opération peut avoir des conséquences incurables et même fatales. De fait, si la poche de l'urètre est formée de la vraie distension de l'urètre, elle se retracte après l'incision, et la guérison procède de la même façon que dans l'urétrotomie externe.

Mais, si, au contraire le calcul est formé dans un diverticulum, l'incision laisserait une fistule permanente inguérissable. Il ne resterait donc qu'à retirer les calculs ensemble et la poche, mais, dans ce cas, une portion de l'urètre viendrait d'être retranchée, et la mort surviendrait, causée par l'infiltration urinaire, que la sonde ne pourrait éviter.

Et ceci est l'explication des fréquents décès dans les cas de calculs enkystés du périnée.

On peut donc dire, que, si l'ouverture du kyste donne une cure longue et une guérison incomplète, l'extirpation, au contraire, est cause presque certaine d'une funeste fin.

De tout ce que j'ai dit, il résulte que pour les calculs périnéaux enkystés, il n'existe pas de méthode de cure correspondant à la gravité de la maladie.

Je propose une nouvelle méthode :—

1.—Après l'incision des téguments et du kyste, de retrancher de ce dernier une grande partie des tissus latéraux, ne laissant subsister que juste la quantité nécessaire pour pratiquer l'urétroraphie.

2.—Recoudre les téguments avec la suture profonde et superficielle.

3.—A défaut des téguments, exécuter la plastique à redoublement de lambeau, suivant le système Nélaton.

4—Dans le cas d'impossibilité, de faire alors l'hypospadias artificiel.

Je conclurai en rappelant ces deux points nouveaux : le premier, scientifique, c'est-à-dire la pathogenèse ; l'autre, pratique, relatif à l'opération que je crois nécessaire.

Examen anatomo-histologique de la poche.

Les parois du kyste ont l'épaisseur de 3 ou 4 millimètres.

Elles sont bien circonscrites, et ont une consistance fibreuse. La superficie interne est lisse, reluisante, à l'exception de quelques points, où existe un procès ulcératif.

Les sections microscopiques pratiquées dans tous les sens sur la paroi du kyste ont démontré qu'il se compose de deux couches essentielles, une épithéliale et l'autre conjonctive, unies entre elles de façon à rappeler la structure de la peau.

Le plan, ou couche épithéliale très-épaisse, se montre divisé en deux parties presque égales. Le plan intérieur qui regarde la cavité cystique mesure six micro-millimètres, et se compose de cellules cornifiées, si fortement conjointes entre elles que même avec la potasse caustique il est impossible de les désunir.

Le plan externe mesure huit micro-millimètres, et est formé de cellules épithéliales spineuses très-ressemblantes de leur forme et de leur grandeur à celle du corps muqueux de Malpighi.

Ces éléments de l'épithélium pénètrent dans les couches profondes, en guise de filaments de cône et de masses informes, s'incorporant dans les tissus conjonctifs comme des introflexions interpapillaires de l'épiderme pour une extension de 8 à 10 micro-millimètres.

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Entre ces prolongements épithéliaux existe un tissu conjonctif fibrillaire, panne de vases sanguins, et parsemé de jeunes cellules conjonctives. Les vases capillaires se dirigent vers la couche épithéliale, comme dans les papilles du derme.

Le reste de la paroi cystique se compose d'une trame de robustes filaments fibreux, entremêlés de fibres élastiques. On y trouve aussi des vases entrecroisés et quelques jeunes éléments cellulaires.

Notes of a Case of Rupture of the Brachial Plexus and Right Subclavian Artery.

Dr. W. Mitchell Banks, Liverpool.

Injuries to the great cords of the brachial plexus are fortunately very rare. As a result, we are as yet only imperfectly acquainted with their nature. In order to increase our knowledge, the first thing that is necessary is the collection of a series of carefully-recorded cases in which the character of the lesion is not simply guessed at or inferred, but is actually seen. On this account I have deemed the following case worthy of being laid before the Congress:

William Thomas, aged thirty, a coloured seaman, in the beginning of December, 1880, fell down a ship's hatchway. He cannot tell anything about his injury, except that he was taken to hospital, where he was cupped on the chest, and bled at the bend of the right arm. Admitted into the Liverpool Royal Infirmary, February 5, 1881. The right fore-arm and hand were swollen and œdematous. Complete loss of sensation in the hand and back of the fore-arm, and partial loss in the front of the fore-arm and in the upper-arm. Complete loss of motion from the shoulder downwards. Wasting of muscles of shoulder and upper-arm. No pulse in the radial, ulnar, brachial, or axillary arteries. No sign of any dislocation or fracture, but a hard lump, about the size of a walnut, above the outer third of the clavicle. As he stood before us the limb dangled helplessly by his side; down to the elbow shrivelled and attenuated; from the elbow to the fingers swollen, and doughy to the feel. The Faradaic current caused the deltoid to move, also the great pectoral slightly, but no other arm muscle. On March 2, under ether and antiseptics, an incision was made along the clavicle, joined by one along the outer margin of the sterno-mastoid. A careful dissection was made of the posterior triangle of the neck. The hard lump was found to indicate the site of a rupture, probably of the inner and middle coats of the third part of the right subclavian artery, which had been followed by coagulation and closure. The brachial plexus being sought for, was found wanting, with the exception of one small cord, which, being pinched, caused the deltoid and great pectoral muscles to move. The dissection was carried up to the cervical transverse processes, and down to the clavicle, but none of the remainder of the plexus was found. From this it was inferred that the plexus had been torn away bodily from the side of the spinal column, and had been dragged downwards below the clavicle. Listerian dressing. The extensive dissection necessarily made during the investigation was completely healed by the ninth day, without one drop of suppuration. Patient is now *in statu quo.*

The points which appear to me of interest are the following:—

1.—We see that a fall, apparently unaccompanied by any fracture or dislocation, may rupture the subclavian artery, and actually tear the strands of the brachial plexus away from the spinal cord.

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2.—We see that any attempt to reunite a ruptured plexus, even where the upper ends of the nerves are to be found, will be extremely difficult, from the fact that the lower ends tend to retreat downwards, behind the clavicle, to such a distance that, I fear, the surfaces, however carefully refreshed and brought together, would never unite.

3.—The case, I venture to think, shows that, if any doubt as to diagnosis remains in the mind of the patient or surgeon, an exploratory operation is a justifiable proceeding, and, at all events, relieves the mind of the former from that torturing state of uncertainty and anxiety which every one knows to be more unbearable than knowing the worst.

On the Pure Rubber or "Martin" Bandage in Synovitis of the Knee-joint, and its Sequelæ.

Dr. Henry A. Martin, Boston, Mass.

I have, during the past twenty-eight years, treated over 400 cases of synovitis of the knee and its sequelæ of every form and degree of severity, in every variety of diathesis and complication, however chronic or acute, by the use of the pure rubber or Martin bandage, applied to the limb, from the foot to above the knee, the joint having previously been strapped from three inches above to a corresponding point below the patella, and with a plaster invented by myself, as I could previously obtain none which would remain *in situ* beneath the bandage for twenty-four hours, or which, so remaining, would not induce violent irritation and even inflammation of the skin.* This strapping is not applied for the ordinary reasons, but to obviate, or at least mitigate, a troublesome chafing of the skin in the popliteal space, from walking exercise while the bandage is on the limb. One such strapping will remain *in situ* for four or five weeks, and, in a very large proportion of cases, has not to be repeated.

The bandage should be applied as tightly as the patient can be induced to apply it, but always quite within the bounds of perfect comfort. No apprehension whatever need be felt of danger to the circulation from too great tightness of bandage, as no constriction, in the slightest degree endangering circulation, could possibly be endured for a single minute without agony. This is stated with the utmost confidence, as the result not only of almost innumerable applications of the bandage in practice, but of several very extensive series of experiments. The bandage thus applied should be worn day and night for a certain period; and during the day only, or while in the upright position, for a still further period. These periods vary according to the case. In a very large proportion of cases, day and night for four weeks; and afterwards during the day only, for from four to eight weeks more was all that was needed, although patients would seldom be willing to abandon its use so soon, and would often continue to wear it long after it ceased to be necessary, from a fear that, if they abandoned the use of what had been so very useful to them, their troubles might return.

The effects observed from the continued use of the bandage thus applied, are

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* This plaster, with every information regarding it, may be obtained from Messrs. Krohne & Sesemann, 8, Duke Street, Manchester Square, London.

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very, and invariably, remarkable. Great support and comfort are at once experienced, and with these much increased capacity to use the joint. Very soon it becomes evident that absorption of effused fluid and of the interstitial deposits in the tissues of the synovial sac, and of the other tissues about the joint, is going on; and in a space of time too short to be credible to those who have not accurately pursued the practice, and carefully and repeatedly observed the fact, the enlarged and weakened articulation is restored to the normal size, and, if not immediately to its original strength, to a far increased capacity for use; and eventually to a perfect restoration in all respects. Although these statements of results from the use of the bandage *alone* are absolutely and literally true in very many cases (in *all*, in which the fluid effusion within the sac was small, and all those in which the thickening of the sac was the principal element of the case), there were still many cases (those, namely, in which the fluid effusion was large in amount) in which absorption of the fluid, although at last obtained, was obtained very slowly; and a few in which the bandage alone seemed insufficient to induce absorption of the effused fluid within the sac. In even these cases, however, the bandage was of the utmost value as a palliative, patients using a limb freely and painlessly for considerable periods of time, who without it could make but little use of the joint; none without fatigue and pain. The existence of these exceptionally obstinate cases induced me, about nine years since, to add to the use of the bandage a preceding thorough aspiration of the sac, all other points of the treatment being exactly as before described. At first I did this only in very exceptionally obstinate cases, in which the effusion within the sac was large; but I have gradually come to know that the operation is entirely free from danger, and I now practise aspiration in all cases in which, the case being chronic, the synovial effusion is of any considerable amount; and lately even in the most acute cases, in which rapid effusion produces great distension and consequent pain. The narrow limit of time allowed me will only permit these general statements:—

1.— That in a little over nine years I have treated over one hundred and forty cases of synovitis of the knee and its sequelæ by aspiration with a single strapping of the joint, and subsequent use of the bandage.

2.—That in these cases I have over four hundred times punctured the knee-joint.

3.—That in all these cases, with the exception of a very few, and those only in the early stages of treatment, the patient was not only permitted, but obliged to take a daily and considerable amount of walking exercise.

4.—That in not a single instance had there been failure of absolute and entire cure.

5.—That in no instance had this desirable end been postponed for more than seventeen weeks, and in only one instance for so long a time, all other cases entirely ending within eleven weeks.

6.—That although in no instance had any antiseptic measures beyond perfect cleanliness of the aspirating needle been employed, in not one instance had the shadow of a shade of even the approach of an ill-symptom followed the operation.

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The Cure of Hernia by the Antiseptic use of Animal Ligature.

Dr. Henry O. Marcy, Boston, Mass.

In 1871 I first published in the *Boston Medical and Surgical Journal* two cases, operated upon by me the previous year, in which I closed the ring with interrupted sutures of carbolized cat-gut, followed by permanent cure. So far as I yet know they were the first cases operated upon in this manner.

In 1878 I read a carefully prepared Paper upon this subject before the American Medical Association, in which I collected the experience of others, and reported a series of my own cases.* I still think that I there proved histologically that cat-gut, properly prepared and applied antiseptically, is either transformed into or replaced by fibres of living connective tissue.

At the last meeting of this Congress, at Amsterdam, Professor Tilanus read a Paper upon the cure of hernia by a similar method. L. Championnière, of Paris, reports a number of cures by closing the ring with sutures thus applied. In America the operation has been repeatedly successfully performed. In these days of modern surgery it will be the more readily accepted that an incision down upon the hernial opening, wherever situated, cannot be accounted as a severe operation. In the majority of cases of simple hernia this is all that will be necessary; and even when the peritoneal sac itself is opened little risk will be apprehended.

The operation may be described in a few words. Antiseptically I cut down upon and expose the ring. If the sac is small the peritoneum should not be opened, if large or easily bulging it is much better to exsect it. If this is done, in some instances it will be better to first close the peritoneum independently with a fine continuous suture; otherwise, refresh the pillars of the ring or walls of the opening and close by sutures. These are covered in by the external tissues which are held in place by superficial stitches; drainage is also used if the abdominal wall is thick.

The dressings are very carefully applied, and union by first intention almost invariably takes place.

At first I used cat-gut, and the interrupted suture, now I prefer the parallel fibres of tendons antiseptically prepared, and close the ring very carefully with the continuous suture in close juxtaposition. The two great objections to cat-gut ligatures are, that many times they fail to hold the parts *in situ* sufficiently long, and the knots untie more readily than silk. The parallel fibres of lean animals like the deer, kangaroo, or moose, which latter animal affords tendons nearly two feet in length, and quite free from fat, and are very strong and firm, do not change as early in the tissues as cat-gut. Therefore, I think they are preferable for most surgical purposes.

The continuous suture has a double advantage: it reduces the number of knots to two, which may be a considerable gain, and the collateral swelling of the tissues is equalized by distributing the pressure of the suture throughout its entire length. This ensures a uniformity of circulation in the enclosed part, and thereby lessens the devitalization of the tissues, causing a more sure and rapid repair process to take place.

If I am correct in my belief that animal sutures are replaced by bands of living

* See "Transactions of the American Medical Association," 1878.

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tissue there is a corresponding advantage in encircling the yielding relaxed parts by closely taken stitches, since thereby we lessen the danger of the return of the hernia by strengthening as well as occluding the ring.

In all the great variety of operations practised in hernia, one of the prime difficulties and chief causes of failure has been the superabundant peritoneum which easily pouches at its former distended part, is filled with omentum, or intestine, which makes pressure from within outwards upon the closed but weakened ring, and often speedily causes a return of the hernia. Since it now adds so little to the danger of the operation, I think it is generally much better to exsect with care the peritoneum, bring the edges together with a fine over-and-over stitch so as to leave its inner surface unfolded and smooth. When this has been done it effectually closes the abdominal cavity and renders the subsequent steps of the operation quite easy. Any hæmorrhage which may arise from the wound, or from the refreshing of the walls of the opening, is then external to the peritoneum, and the tendinous structures of the ring are brought into just apposition, as stated above.

When drainage is required I prefer horse-hair. This should be previously prepared in parallel bundles of six, eight, or ten hairs, as size may require, held together by carbolized glue or solution of gum, and dried. Then they may be cut in lengths required, and thus we escape not only the use of knots, which of course are objectionable, but can remove one or more of the hairs as thought wise at each dressing. Owing to the difficulty of securing immobility and close approximation of the dressings it is preferable to secure it with an elastic bandage.

I would claim as advantages for this operation :—

1.—We are enabled to clearly see each and every step of the operation: blind surgery is bad surgery, as a rule. This want of clear perception has been the source of many mistakes by even experienced operators, and is a great objection to every form of sub-cutaneous sewing. In my judgment, no less can be said of the very ingenious and improved modern process of astringent injection into the ring, as advocated and practised by Dr. Joseph Warren, of Boston, in his recent work upon hernia. The same objection would hold good of Mr. Spanton's method of approximating the rings by the use of his corkscrew needle, which is left *in situ* for several days.

2.—It is the only method with which I am acquainted whereby the hypertrophic elongated peritoneal pouch, which has been a primal factor of failure hitherto, can be removed.

3.—Hereby we actually re-inforce as well as occlude the ring. In the weakened attenuated condition of the parts in all old cases there is no small gain in securing such effect.

4.—Experience has, I think, now demonstrated that this operation may be catalogued among those safely advised, and that femoral and umbilical hernia are no exceptions. I would not exclude children from the class to be benefited: for in them the vital processes are at the best, and when thus cured they are saved from a life-long disability.

It would be difficult to appreciate the amount of suffering produced by hernia; its frequency and its dangers are well known to every medical man. The truss rarely cures the hernia, often even fails to prevent strangulation. The sufferer from hernia is unable to engage in many occupations, and is practically doomed to a life-long disability. Under the guidance of the conservative teachings of our athers we have relegated to the instrument-maker this whole army of invalids. In the march of progress in surgery, which marks our generation, the cure of

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hernia may be included among its triumphs, thereby sufferings lessened, life prolonged, and the welfare of the race greatly increased.

On the Cure of Hernia, in relation to Parents and the Profession.

Mr. W. D. Spanton, Hanley.

There is one aspect of the large subject of hernia which seems to have escaped that amount of notice which it deserves, and it is that to which I propose to call your attention.

The widespread prevalence of the affection has been variously stated, but a definite idea of its universality is given by the tables of Dr. J. H. Baxter.*

If we take the population of England and Wales at about 26,000,000, and estimate at the low calculation that one in twenty have hernia, we may take it there are in this country more than a million and a quarter of hernial subjects. What becomes of all these persons? They are not confined to any one class, although the death rate is, as we should expect, much greater among the labouring classes. Of the large number who wear trusses, the London Truss Society relieves about 9,000 a year; and Mr. Banks, the Manager of Messrs. Maw, Son & Thompson's, tells me that they make at the rate of about 55,000 a year, most of which are for this country. This is, of course, independent of those made by other makers, which would amount in the aggregate to probably about as many more.

Many others drag on a miserable existence, because unable to wear any support or obtain any relief; some are so sensitive about it as to make it a secret, silently but surely destroying their comfort and peace of mind; probably many, ignorant of its existence or unaware of its danger, take no heed of its presence; and a considerable residue come to grief by strangulation.

The best evidence I can afford of its fatality is in Table I. of Dr. Baxter's Tables,† in which we find that the mean annual rate of mortality for the year 1879, was 45 deaths to every 1,000,000 living; and to make the significance of this more manifest, I may point out that while calculus killed 237 persons in the year 1879; and all malformations (except spina bifida) put together, 219; gout 662; and all uterine diseases only 1,068—hernia caused the death of 1,119 in the same period. And if we examine Table I. (p. 248) we find that 263 deaths occurred during 1879 from operation for strangulated hernia—*i.e.*, nearly one-fourth of the total number of deaths.

In Table II. (p. 249), of hospital mortality, it will be observed that the average rate of mortality after operation, in eleven large hospitals, taken from 1,870 cases, with 782 deaths, is 41·80 per cent. This shows very clearly the extreme fatality of those cases which have to be submitted to operation for strangulation. It is difficult to estimate the proportion of strangulation cases, but they must be numerous. Further, the proportion of deaths occurring under five years of age, constitutes about 7 per cent. of the whole, and, in a series given by Mr. Bryant,‡ 14 fatal cases

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* Dr. J. H. Baxter's Statistics of Examination for Military Service in the United States Army during the war of the Rebellion.

† *Loc. cit.* ‡ "Guy's Hospital Reports," 1856, vol. ii. p. 73.

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of 83 recorded occurred under twenty years of age—*i.e.*, 16·87 per cent., and of these 14, 7 were congenital. It seems tolerably certain that a large proportion of cases occurring in later life date really from infancy; for although they are not strictly speaking "congenital," inasmuch as the congenital condition has disappeared *quoad* the hernia, yet the lax state of the abdominal rings has remained, with a corrresponding liability to a reproduction of hernia at a future time. No doubt most surgeons have noticed how many of these cases present themselves. This is a strong argument in favour of affecting a cure while the hernia is recent, irrespective of the age of the patient. Of this I shall speak presently.

A hernial subject is debarred from many of the privileges which are open to others. If left an orphan while young, a presentation is obtained to enable him to enter a charity school, he will not be received; if while a boy, he wants to enter the army or navy, he is ineligible; if committed by the magistrates to an industrial school, he cannot be admitted, but must be relegated to prison; when he has to work hard for his living, he is unfitted for any laborious occupation; when he offers his services to his country in any of the public services he cannot be passed, and must seek some work for which he is in every respect far less otherwise fitted. If, on the other hand, in a higher sphere of social condition, he is forbidden by the Jewish law to enter the ministry as a priest; and, in these more practical days, if he wishes to effect an insurance on his life, he will have to pay from 2*s.* 6*d.* to 7*s.* 6*d.* per cent. extra premium, and possibly, if he suffers from double rupture, may fail to find an office to insure his life at all.

So far as danger may arise, in 1879 from this cause alone 1,119 deaths occurred, and 263 of these persons underwent operation. Looking at the subject from this, its more serious aspect, it becomes the duty of the surgeon to consider whether he is justified in advising a palliative course of treatment. Veterinary surgeons have a keener sense of the value of their patients. If a colt is found suffering from hernia—and it is not an uncommon affection among colts—and if, when the testis ascends, the congenital rupture is not naturally cured, the veterinary surgeon operates without hesitation. "Bandages and all topical applications are found useless or worse" (W. Williams), and death occurring after radical cure is an extremely rare event. But prejudice, and the absence of accurate knowledge of the subject, would seem to have deterred most surgeons of the present day from advocatiug any such valuable relief for mankind. Much might be urged against the use of trusses in a large number of cases, but this is hardly the occasion for it. That they are often quite useless, that in numbers of instances they increase the danger from strangulation, that they are expensive, troublesome, and annoying, few will deny. On the other hand, the number of cures effected by their instrumentality is infinitesimal, if we may judge from the report of the London Truss Society, where we find that of a total of 96,886 persons relieved by trusses, only 4,387 are stated to have been cured—*i.e.*, 4·53 per cent.!

Now, if it be possible to effect the cure of rupture early in life (thereby eliminating at once one-eighth of the whole number of cases) by an operation which shall be both safe and efficient, we are led to inquire, in the words of Mr. Spencer Wells, "whether it may not be better to operate even on young children, than to expose them for several years to the inconvenience of a truss, with the probability that, after all, a radical cure may not be obtained?"* Parents have a certain duty

* "On the Radical Cure of Reducible Inguinal Hernia." By T. Spencer Wells. 1858.

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to perform towards their offspring in the matter of physical defects; and, in the performance of this duty, it is generally the province of the medical adviser to recommend the course which should be pursued.

With modern antiseptic precautions the maximum of safety may be attained, and the experience of late years has clearly shewn that operations near and connected with the peritoneum may be carried out not only without undue risk, but actually with less danger to life than many which are well recognized and almost universally advocated for the mere removal of deformity or the relief of inconvenience. Hernia is surely a source of greater risk to life than club-foot, or hare-

TABLE I.

Table showing the Number of Deaths from Hernia of all kinds in England during a series of twenty-eight years, and the Number of Deaths after Operation.

Year.	Deaths from Hernia at all Ages.		Deaths under 5 Years.		Total Number of Deaths.	Deaths after Operation.		Proportion of Deaths after Operation to whole No. of Deaths per cent.
	Male.	Female.	Male.	Female.		Male.	Female.	
1852	370	313	65	13	683	No statistics given of operations performed.		
1853	406	373	53	12	779			
1854	407	421	45	11	828			
1855	426	448	43	15	874			
1856	441	407	50	7	848			
1857	414	400	46	13	814			
1858	416	350	42	10	766			
1859	443	319	52	6	762			
1860	418	399	51	10	817	11	14	3·06
1861	408	444	43	9	852	12	13	2·93
1862	405	422	45	9	827	14	21	4·23
1863	424	424	54	8	848	13	19	3·77
1864	409	396	44	11	805	19	15	4·22
1865	463	427	48	17	890	22	22	4·94
1866	465	409	47	11	874	17	23	4·58
1867	467	460	37	15	927	36	44	8·63
1868	461	446	33	10	907	43	48	10·03
1869	457	494	53	15	951	66	80	15·35
1870	487	492	52	5	979	80	100	18·38
1871	519	503	50	10	1022	75	96	16·73
1872	524	489	54	15	1013	90	92	17·96
1873	557	457	66	22	1014	76	78	15·18
1874	512	500	53	15	1012	63	71	13·24
1875	585	549	65	17	1140	83	108	16·75
1876	533	555	63	19	1082	87	106	17·84
1877	565	529	70	16	1094	95	97	17·55
1878	565	582	83	16	1147	101	111	18·48
1879	546	573	55	12	1119	128	135	23·50

lip, or nævus, a crooked limb, or an ankylosed joint. Yet these are conditions for which an operation, and not unfrequently a fatal one, is readily admitted and recommended; whereas it is thought usually sufficient to palliate hernia by the advice to wear a truss, and allow the dangers and other drawbacks incident to it to continue uncured. It is time this opinion changed, and I feel convinced that those who will not be unwilling to see for themselves the advantages of an operation for the cure of hernia over the uncertain and unsatisfactory treatment by trusses, will, in a large number of cases, advocate its adoption. Operative measures

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in modern days have not had a fair trial; they have not been carried out on a sufficiently extensive scale to demonstrate their real value.

My own plan of operating in inguinal hernia I described in a paper read at Cork, in August, 1879.*

I have now a record of thirty-four cases, of which thirty have been quite successful, the remaining four being much benefited, but not completely cured. No death has occurred, nor, indeed, any condition to call for serious anxiety in any case.

If by some simple and safe operation such as mine, we can effect not only a radical cure of the hernia, but also an immediate and permanent one; and if by so doing we can protect the subject of it from the multiform dangers and disabilities to which such a condition sooner or later must invariably give rise, I cannot but believe that not only are we, as practical surgeons, justified in performing it; but

TABLE II.

Summary of Returns from eleven Hospitals, showing Number of Operations for Strangulated Hernia, Number of Deaths, and Average Rate of Mortality.

Name of Hospital.	Number of Operations.	Number of Deaths.	Average Mortality per cent.
St. Thomas's	213	104	48·82
St. Bartholomew's	415	164	39·51
Guy's	441	172	39·00
St. George's	213	93	43·66
Middlesex	91	26	28·57
London Hospital	99	39	39·39
Bristol Royal Infirmary	30	10	33·33
North Staffordshire Infirmary	39	12	30·76
Liverpool Royal Infirmary	79	32	40·50
Leeds Infirmary	96	42	43·61
Manchester Royal Infirmary	154	88	57·14
Total	1870	782	41·80

that the fulfilment of such a duty is imperatively called for in all those cases which are fairly amenable to treatment.

Can the scientific surgery of the present day boast of nothing better than to follow the teaching of three hundred years ago? And are we to be told that because the barbarous customs of ancient days are unjustifiable and wicked—owing to their being based on a want of exact knowledge—the same sweeping condemnation is to be passed on the recent procedures for the cure of this serious and formidable affection? Surely not. Operations which will occur to every one, such as early ovariotomy, oöphorectomy, and so on, have been denounced as trenchantly as some would decry what I am now endeavouring to advocate; but the day will come, and is I trust rapidly approaching, when this branch of preventive surgery will achieve results of incalculable value to mankind, such as only large and increasing success can warrant.

* *British Medical Journal*, December, 1880, and January, 1881.

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On the Treatment of Fractured Femur.

Mr. Rushton Parker, Liverpool.

Among the many uses to which the splints of Mr. Hugh Owen Thomas, of Liverpool, may be put in the mechanical treatment of injuries and diseases of the lower limb, none is more conspicuous in fitness, frequency, and effect than their employment for cases of fracture, especially of the femur. A glance at the models or diagrams will show this, and will besides, I hope, throw upon my words a light in which they could hardly otherwise be seen, as to many here addressed the objects and methods may be unfamiliar.

For fractures of the shaft and condyles, Thomas's knee-splint is advised, and for fractures of the neck, the hip-splint of the same author.

In fractures of the condyles the knee-splint is applied in its most simple style and in a way exactly similar to that adopted for uncomplicated inflammation of the knee-joint (1). The oval padded ring of iron rod encircles the root of the limb, lying in the fold of the groin in front, and similarly behind, being in close contact with the skin covering the ischium. Two strips of bandage, laid one along each side of the leg, and fixed there firmly, but not tightly, by plenty of adhesive plaster, are tied to the lower end of the splint, so that the leg is pulled straight and the splint kept constantly against the perineum by what has the appearance of slight extension and counter-pressure. The limb itself lies between the parallel bars of the splint, suspended evenly and steadily, in a sheet of calico or a towel pinned to the bars at each side, and reaching from a little above the ankle to a little below the hip. A wide bandage round the middle of the leg, and another round the middle of the thigh, keep the limb perfectly still and straight, the whole thing constituting an exceedingly comfortable and a really artistic adjustment, in a perfectly natural attitude.

The patient rapidly loses any pain and tenderness that may have existed can sit up in bed, make use of the bed-pan, and otherwise move unassisted, without interfering with the treatment and good progress of the case. There is, in all surgery, no more perfect means of securing, with great facility, the linear immobility of the lower limb, and giving at the same time a great range of liberty to the patient. The knee is left uncovered, with little or no readjustment, requiring chiefly to be let alone till union of bone, disappearance of all swelling of the soft parts, and complete slackness of the joint, have ensued. Passive movement is, under this system, never employed by either the author of the apparatus or by myself, being replaced by a patient confidence in its strict avoidance, a few additional weeks of which easily purchase immunity from that stiffness of which passive movement is not unfrequently, although unintentionally, the cause.

For immediate use the "bed-splint" (*a b*) is preferable to the "walking-splint," though the latter is, perhaps, better known (*d*). The former is made with a symmetrical oval ring, and can be employed on either side of the body. The lower end of the bed-splint has hitherto been made simply square (*a*), and may conveniently rest upon a block or a book laid in the bed. But a recent improvement consists in an oblong rectangular frame, by which the splint and the limb are raised a few inches off the bed.

In the walking-splint (*d*) the ovoid ring is unsymmetrical, being shaped according to the section of the thigh, more prominently curved behind, around the hamstrings, than in front, where it is flatter. It must, therefore, be made

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"right" and "left." The lower end, moreover, is furnished with a circular, oval, or ovoid pattern, to touch the ground, while the limb is slung in a sheet of leather for more permanent fitness and durability.

Again, while referring briefly to modifications in form, I may add that instead of two bars merely, the knee-splint is now often fitted with three, the additional bar reaching from the back of the upper ring to a point about half-way down the calf, where it is attached by a semicircular piece of rod behind the other two welded to all three. This third bar existed in an early development of the splint, but was cast aside as not essential to rigidity as at first supposed. But it has now been found to be invaluable in preventing the back of the knee from coming in contact with the bed in the recumbent attitude, and from striking the chair when the patient sits in using the walking-splint. It is thus a great, and sometimes a necessary, addition, protecting the joint against even slight, though painful, or at least detrimental, movements. Incidentally, the third bar diminishes the pressure of bandages without necessarily impairing their efficiency.

In fractures of the shaft of the femur the knee-splint is of great value. The side-straps are applied as before said, and tied to the lower end of the splint permitting adjustment of the limb at its full length, by extension and counter pressure on the ischium, as in the case of the "long splint" and "perineal band" of Liston. The leg from the knee downwards is supported, slung between the bars on a towel or sheet of calico, which may be conveniently used double, folded over one bar and pinned to the other. It is well to place a soft pad behind and in front of, or even all round the leg, as a protection to the skin in contact with the tibia and fibula, before applying the wide bandage. The thigh is separately supported by a hollow firm splint of padded sheet metal (zinc or iron), or wood, as in Gooch's material, or even thickly folded paper, closely fitting the limb, reaching from the ring at the buttock to a little way below the knee. Contact should be secured behind the knee-joint by interposing, if required, a small mass of extra padding slipped in to relieve the soft parts behind the knee from the irksome strain of supporting the bones. This splint behind the thigh is now slung to the side bars by strips of bandage two or three in number, suitably adjusted and knotted. Other short hollow splints, of the material that may be chosen, or that comes most handy, are now placed so as completely to encircle the thigh, and strips of bandage tied moderately tightly around them, and the included thigh inside and outside the iron bars according to fitness.

The extension straps are tightened up from day to day, if slack, so that the full length of the limb is, if possible, secured within a week or so, especially in children, whose bones so quickly unite. When the length is secured, all that is required is a vigilant maintenance of immobility and correct line, by tightening the encircling bandages as they become slack during the wasting of the limb, and by such other adjustments as common sense demands. In case the skin of the perineum becomes tender, apply grease, and so prevent or allay excoriation.

By this mode of treatment shortening may be more often prevented than is generally imagined, and in a good proportion of cases can easily be averted.

It is obvious that shortening must often occur in fracture of the shaft of the femur, but by this method of treatment the frequency and the extent of the deformity are, to say the least, less difficult of control than by any other.

The liberty of the patient in bed, while securing the full efficiency of the treatment, is also obviously greatly increased. In osteotomies and other compound

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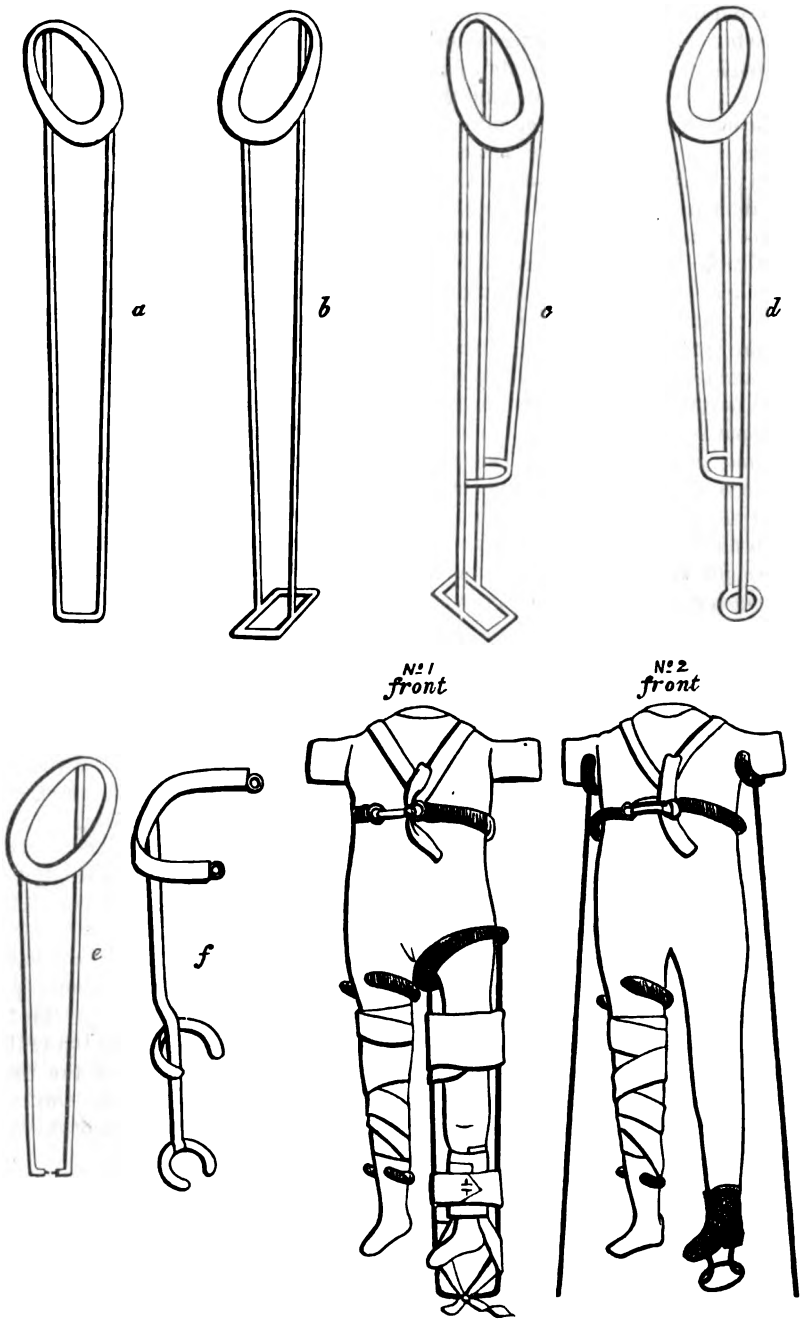
Fig. 48.



a. Ordinary bed-knee splint for either side.
b. Ditto, with new end to raise it off the bed.
c. Bed splint with three bars for right side.
d. Walking knee splint, for left side. Three bars and terminal patten.
e. Calliper walking splint, to clip in boot heel.
f. Hip splint, for right side.
1 and 2. Method of application of splints.

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fractures the same means are employed, with the addition of Lister's arrangements and dressings.

During convalescence, after union, the walking knee splint (fig. *d*), in which the weight of the body is transferred to the perineum, as the limb hangs harmlessly between the bars, and the sound limb is provided with an elevated boot or patten, should be used until union has become inflexibly hard. In adults the period varies, but is one that may be counted by months—roughly speaking, from six to twelve after the receipt of the injury, in favourable cases. It is well to let the patient know this at first, for the sake of his or her arrangements; and in order to do so with confidence, it is well for surgeons to provide for the supervision of the case during some such length of time, as it is to be feared that they not seldom erroneously substitute the six to twelve weeks properly claimed in books for the mere process of union, in computation of the average duration of the more lengthy process of a finished cure.

In children the precautions taken need not be so stringent, as their bodies are lighter, and their bones, although soft, less liable to bending at the seat of union, in comparison; though the possibility of secondary deformity must be borne in mind and guarded against. The calliper walking splint may be substituted for the longer knee-splint as soon as the limb is fit to bear the whole or most of the weight of the body, while still requiring to be maintained in the straight position. The elevated patten is thus dispensed with, but even then, if required, part of the weight may be taken off the confined limb by making the splint just long enough to press on the perineum in the attitude of full linear extension (fig. *e*).

Fractures of the neck, or immediately below the great trochanter, are best treated in Thomas's hip-splint (fig. *f*), applied exactly as in hip-joint disease. In fracture of the neck without impaction, as there is not seldom an absence of shortening at first, it is well to apply the splint as early as possible after the receipt of the injury; in fact, when opportunity occurs, before the patient is moved *at all*. Otherwise, during transport shortening to even a great extent may any moment be caused.

In impacted fracture the shortening is accepted, but the use of this splint is found to be most gratifying, not only by removing and diminishing pain, but by greatly extending the patient's liberty.

The unassisted use of the bed-pan and other considerable voluntary changes of situation and attitude can be painlessly effected in bed, with the aid of the hands and the other leg, and the enforced recumbency thus made less wearisome. The bed should be soft for the use of both kinds of splints; in that for the hip the patient lying chiefly upon the back, which should be uniformly supported by the bed, into which, when sufficiently soft, the splint is depressed, and prevented from bearing more than a trifling share of the recumbent weight of the body.

In the event of recent shortening in unimpacted fracture of the neck, in a vigorous patient at or under the middle age, extension straps should be well fixed to the thigh, and tied to the lower end of the splint, which then should have no shoulder bands, and which, by its observed tendency to slip downwards towards the feet, will be found to act as a sufficient drag in maintaining the length of the limb. But if that do not prove enough, the inner horn of the thigh crescent may be thickly but firmly covered with extra padding, and comfortably employed in counter-pressure on the ischium. When locomotion is resumed, on the cessation of all tenderness and the occurrence of union, the injured

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limb is kept off the ground, and all flexion and other possible strain avoided at the now pliable neck, by the use of crutches under the arms, a wooden clog, or iron patten, under the boot of the sound leg, and by a continuance of the splint behind the broken limb (fig. No. 2.) The only postures permitted are the vertical and the horizontal.

In the event of non-union, in fracture near the head, from whatever cause occurring, the walking knee-splint may be employed with advantage, as a valuable aid to progression, and a useful substitute for crutches. In compound fracture, incision of joint or of abscess, or in case of any other wound or open inflammation, a Listerian dressing can be applied to the locality just as well with this splint as without it, the splint of course being outside the dressing.

A Case of Gastrostomy, performed for Stricture of the Œsophagus in a woman, aged 66, who survived the Operation eighteen months.

Mr. R. J. Pye-Smith, Sheffield.

E. S., a widow, aged 66, came under my care at the Sheffield Public Hospital and Dispensary in March, 1879. She complained of great difficulty of swallowing, of pain in the throat, and of loss of flesh. The symptoms dated from the commencement of the year; she assigned no cause for them, and denied having ever swallowed any corrosive or very hot liquid. There was neither history nor sign of syphilis; and, beyond very considerable emaciation, there appeared nothing abnormal in her condition of body (including thoracic and abdominal organs), except that on swallowing water it took several gulps to get a mouthful down. A diagnosis of epitheliomatous stricture of the œsophagus was made. She was ordered a liquid diet and five grains of iodide of potassium three times a day. Her symptoms continued to increase, and she begged that something might be done, as she was starving. She was admitted as an in-patient on March 24, and ordered milk and beef-tea to drink, and nutrient enemata of minced sweetbread and beef. It was now found that on swallowing a small mouthful of fluid, about half of it was regurgitated in about half a minute. On attempting to pass a bougie down the œsophagus it was stopped at a level some little way below the cricoid cartilage, and not even a No. 5 (the smallest size that was used) could be got lower. A little blood and mucus came up after the passage of the bougies, and the operation caused great discomfort and sense of suffocation. During the next few days a somewhat larger proportion of the fluid taken into the mouth was got down. Bougies were then tried again with the same result. On March 29 her weight was 6st. 10lbs., her height being 5ft. The amount of regurgitation soon increased again, and the enemata ceased to be well retained. On April 2 her weight was 6st. 7lbs. She was obviously emaciating, and was very anxious to undergo an operation, as her constant sense of hunger was very distressing to her. After consultation with my colleagues, gastrostomy was decided on. On April 4 she was accordingly placed under the influence of ether, and under a carbolic acid steam spray. The operation was performed as follows:—An incision, 2½ inches long, was made nearly vertically downwards (very slightly outwards), from the lowest part of the eighth costal cartilage of the

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left side, dividing consecutively the skin and superficial fascia, the aponeurosis of the oblique muscles, some fibres of the transversalis muscle, and the transversalis fascia and peritoneum. The stomach now at once presented at the wound, having a healthy appearance; the edge of the left lobe of the liver came into view at each inspiration, and at the lower part of the wound the great omentum began to protrude. Two or three small arteries were twisted, but the bleeding was very slight, and no blood entered the peritoneal cavity. A curved needle in handle, armed with a doubled length of silver wire, was then passed from without through the parietes about three-fourths of an inch to the left of the upper part of the wound, into the presenting part of the stomach (apparently near the great cul-de-sac), transfixing all its coats, out again an inch or more further to the right, and then through the parietes three-fourths of an inch to the right of the upper part of the wound. The needle having been removed, the suture was temporarily fixed at each end over a piece of bougie, and a second suture was then similarly passed and secured near the lower part of the wound. The stomach being thus held in contact with the parietes and plugging up the wound, a vertical incision about an inch long was made into it, and then, by means of a catgut continuous suture passed through all its coats, it was attached closely to the margins of the lower two-thirds of the wound, the upper third of the wound being closed by a separate suture. One small artery in the wall of the stomach had to be twisted. As the catgut suture was applied the mucous membrane became somewhat everted, and a few drops of translucent, viscid fluid of pale yellow colour escaped from the cavity of the stomach. The two double wire sutures were now loosened from the bougie, and the middle parts being drawn out from the interior of the stomach were divided. The two interior ends of each of the resulting four double wires were then twisted together around the substance of a small india-rubber ring* at four points of its circumference. The exterior ends were now pulled tight, so that the ring was drawn closely against the mucous membrane of the stomach inside the wound, and the two wires coming from each puncture in the skin were then twisted together over a piece of bougie bent in a circle round the wound. By this means the peritoneal surface of the stomach and that of the anterior abdominal parietes were held closely in contact for the space of half an inch or so all round the wound. The artificial mouth thus made would just admit the end of the little finger. An india-rubber tracheotomy tube, cut short and plugged with a cork, was now put into it and fastened by means of a thread attached to the shield of the tube on each side and tied round the body. No food was now given, and the wound was dressed with carbolized gauze, &c. There was no retching during the anæsthesia (which lasted about an hour and a half), and the pulse remained good throughout.

Two hours and a quarter after the completion of the operation, the cork was removed from the tracheotomy tube, and a soft india-rubber tube passed through it, apparently into the duodenum. By means of a funnel, half a teaspoonful of Darby and Golden's "fluid meat," mixed with two ounces of water, was poured in and retained. The patient complained of some pain

* Such as is used for holding together the ribs of a closed umbrella, and is very useful in performing circumcision, and in amputating fingers, &c.

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in the hypochondriac region. During the night she was fed in a similar manner every three or four hours with a few ounces of milk, brandy, egg, and "fluid meat," and she was allowed a little water to sip. She got some sleep after a small dose of laudanum. The temperature remained about normal, and the pulse below 80. During the next two days and nights the feeding was continued about every four hours with quantities gradually increased from two to eight fluid ounces. At the end of this period, the number of feedings by the stomach was reduced to three or four a day, and at the same time the patient was allowed to take milk freely by the mouth, and was able to swallow it better than before the operation, so that she got down a pint or more daily. Nutrient enemata were continued occasionally for nearly a fortnight. Within a week of the operation solid food was administered through the fistula, and very soon the artificial feeding was restricted to solids, sufficient fluid being taken by the mouth. Previous to this there was frequently a regurgitation and overflow of fluid from the fistula during the process of feeding. The solids consisted mostly of boluses made of bread-crumbs mixed with "fluid meat," macaroni, moderately boiled, oysters, and small sausages. Many other articles of diet were occasionally given in addition, such as meat cut into fingers, haricot beans boiled, gem biscuits, and Brand's meat lozenges. These substances were pushed through the fistulous opening (the tracheotomy tube being removed), and packed into the stomach by the little finger. When the meal was thus supplied, the opening was plugged, by means of either a piece of india-rubber tubing, closed by a clip, or an india-rubber nipple-shield.

There was some suppuration about the upper and outer parts of the wound, and it was not thoroughly healed for a month after the operation, but there was no sign of peritonitis or traumatic fever. Then began the greatest trouble in connection with the case. The plug was never quite water-tight, and, in spite of careful padding and bandaging, a little gastric juice was constantly oozing from the fistula into the skin around; and although the mucous membrane of the stomach kept perfectly sound and healthy, the cuticle of the skin was eroded, no doubt by a process of digestion, from the margin of the mucous membrane to an inch or two beyond. Partial relief was afforded by alkaline ointments, especially by one of carbonate of magnesia with vaseline, and, in spite of considerable discomfort from this source, the patient continued to thrive fairly, so that on July 4, three months after the operation, she weighed precisely the same as she did two days before the operation—viz., 6st. 7lbs. She frequently sat in bed, and on two or three occasions was up in the ward for a little while, but most of her time was spent lying in bed reading.

About this time, I saw a case in which Mr. Howse had successfully operated with a very small wound through the fibres of the rectus, and seeing the great advantage of this method (which entirely prevented any gastric juice erosion), I persuaded my patient to allow me to attempt the closure of the present fistula and the substitution of a smaller one for it. Accordingly, on August 2, she was placed under the influence of ether, and the anterior wall of the stomach was fixed, by the following method, to the abdominal parietes half-way between the fistula and the middle line of the abdomen. Six pieces of silver wire, each about a foot long, were attached in the middle of their lengths, and at equal distances apart, to an

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india-rubber umbrella ring. Both ends of each wire were threaded through the eye of a straight needle, and then, one needle being taken at a time in a pair of holding forceps, each was passed through the fistula into the stomach and made to perforate the anterior wall of the stomach and the abdominal parietes, so as to form, by their six points of exit from the skin, a circle an inch in diameter. By pulling the wires tight, the india-rubber ring was then dragged into the stomach (through the fistula), and pressed the stomach wall close to the abdominal wall behind the rectus muscle. A second india-rubber ring was now laid on the skin, and each of the six double wires was twisted firmly over it, one end of each wire passing inside and the other outside the ring. Four days later a small vertical incision was made within the area of the outer ring through the skin and rectus muscle. A pair of rather sharp-pointed dressing forceps was then pushed into this wound, and through the remaining tissues into the stomach, entering it within the area of the inner ring. A piece of india-rubber tubing was then guided through the old fistula into the blades of the forceps, and one end of it drawn out through the new wound, whilst the other end was left in the stomach. The edges of the old fistula were then drawn together from side to side with strapping, and the patient was fed through the new tube. A week later the patient was again anæsthetised, and, the six wires being cut externally and the outer ring taken away, the inner ring with the wires attached to it was removed through the old fistula, which had contracted considerably during the past week. The edges of the old fistula were then pared, and the mucous membrane was, to a slight extent, separated from the adjoining tissues, and its edges brought together by fine silk sutures. The skin edges were then fastened together by interrupted silver-wire sutures and a quill suture. This operation was done under the carbolic-acid spray, and the wound was dressed with carbolized gauze, &c. The old fistula remained closed till the sixth day after the operation, and then a little escape of fluid took place at the puncture mark of one of the sutures. The sutures were now removed, but in two days more the lower part of the wound broke open, and one of the silk sutures came away. A small fistula at the old site thus resulted, and, as the patient declined to have any further operation, it had to be plugged as before, and consequently continued gradually to increase in size by stretching. The plugging, however, was now more successful than it had previously been, a piece of india-rubber tubing plugged by cork being used, and henceforward there was very little trouble from escape of gastric juice.

Instead of the solid food previously given, milk and beef-tea, containing finely chopped meat and vegetables, were poured into the stomach through the tube traversing the rectus muscle, by the side of which there was never any leakage; the tube being kept constantly in, and closed, except at meal times, by a clip. About two pints of nutrient fluid were poured in daily in three feedings, and the patient usually took the best part of a pint of milk by the mouth in addition. She was very anxious to swallow solids, and soaked bread was tried on several occasions, but she could never get it down. For a year longer she continued to live in this state in very tolerable comfort, but by September, 1880, she was getting decidedly more emaciated; and by October 15 she was unable to swallow anything. She

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then rapidly sank, and died on October 19, eighteen months and a half after undergoing the first operation.

Autopsy.—The body was much emaciated, but the weight was not ascertained. On opening the abdomen the stomach was seen to be of moderate size, and was pretty normal as to position and structural appearance. The two fistulous openings, which were situated about midway between the œsophageal and pyloric orifices, were each completely surrounded by strong peritoneal adhesions, but there were no adhesions elsewhere in the peritoneum. A little above the level of the bifurcation of the trachea the calibre of the œsophagus was found to be closely constricted, an ordinary probe only just passing through. The constriction was three-fourths of an inch long. The walls of this part were somewhat thick and hard, but not greatly so, and there was no ulceration, but a scar-like appearance of mucous membrane, with some small firm ridges or bridles. Above the constriction the tube was slightly dilated. Several of the mediastinal glands in the neighbourhood were somewhat enlarged. The lungs were congested posteriorly, and both apices were partly consolidated with recent inflammatory effusion, pus being found in their smaller bronchial tubes. On examining a portion of the constricted part of the œsophagus under the microscope nothing was seen of an epitheliomatous or carcinomatous structure.

The autopsy, therefore, showed that my diagnosis of cancerous stricture was wrong. Some weeks afterwards I ascertained from a relative of the patient that about three months before she presented herself at the hospital she had been found one day with her mouth white and sore, and would give no account of what had produced that condition; but a bottle of strong ammonia was found in her room, and, as she had been much depressed at the time, it was supposed she had purposely swallowed some. She went to a surgeon a month or two afterwards on account of difficulty of swallowing, but unfortunately for herself she kept back from him, and subsequently from me, the fact that she had taken strong ammonia.

Although the case is thus not such an interesting one as during life I thought it, it shows, like several other recent cases of the same operation, that opening the stomach may be performed with safety; that the operation, if decided on, should not be delayed too long; that considerable quantities of food may (and should) be given immediately the fistula is established; and that the plan that Mr. Howse and others have practised of making a vertical opening through the fibres of the rectus so small that the feeding-tube fits closely is preferable to the method I adopted; and, lastly, the case furnishes a warning as to the danger of trusting to a patient's history, where motives may exist for misrepresenting it, no less than as to the universal importance of a complete and exact diagnosis.

I must not conclude this record without acknowledging the great patience and interest expended on the case by the House Surgeons in charge, Mr. C. C. Claremont, Mr. J. M. Willey, and Mr. E. S. Morgan; and the constant care bestowed by the nurses of the ward. My thanks are also due to Dr. T. H. Morton for the photographs he has taken of the morbid specimen.

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Excision of the Tongue.

Mr. Walter Whitehead, F.R.S.E., Manchester.

On November 3, 1877, I removed the whole of the tongue through the mouth with scissors, and in the *British Medical Journal*, December 8, 1877, p. 303, I referred to the case, and briefly described the method of operating.

This case was then, to the best of my knowledge, the first instance of the entire tongue having been removed for disease through the mouth by simple excision.

More than thirty tongues have to my knowledge been since removed by the same plan, and I now submit to the judgment of the profession the merits and the results of this operation.

The operation is conducted after the following simple manner:—

1. The mouth is opened to the full extent with Mason's, or any other suitable gag, the duty of attending to this important part of the operation being entrusted to one of the two assistants required.
2. The tongue is drawn out of the mouth by a double ligature passed through its substance an inch from the tip. This ligature is given in charge of the second assistant, with instructions to maintain throughout the operation a steady traction outwards and upwards.
3. The operator commences by dividing all the attachments of the tongue to the jaw and to the pillars of the fauces, after the manner suggested by Sir James Paget, with an ordinary pair of straight scissors.
4. The muscles attached to the base of the tongue are then cut across by a series of successive short snips of the scissors, until the entire tongue is separated on the plane of the inferior border of the lower jaw, and as far back as the safety of the epiglottis will permit.
5. The lingual, or any other arteries requiring torsion, are twisted as divided. It is generally found that a moment's pressure with a small piece of sponge, held in sponge forceps, suffices temporarily, if not permanently, to arrest any bleeding; it is, however, regarded as desirable to twist, either immediately, or after the tongue is removed, every bleeding vessel.
6. A single loop of silk is passed by a long needle through the remains of the glosso-epiglottidean fold of mucous membrane, as a means of drawing forwards the floor of the mouth should secondary hæmorrhage take place. This ligature may with safety be removed the day after the operation, and, as it is invariably a source of annoyance to the patient, it is always desirable to adopt this rule.

The after-treatment consists in feeding the patient for the first three days absolutely and solely by nutritive enemata; satisfying thirst by occasionally washing out the mouth with a weak iced solution of permanganate of potash; forbidding any attempt at speaking, and requiring that all the wishes of the patient shall be expressed in writing, or by signs. The difficulties and dangers of the operation are few, and more imaginary than real. Hæmorrhage, the *bête noire* of most surgeons who contemplate removing the tongue, is in reality easily controllable, and frequently trifling. I have twice removed the entire tongue without having to secure a single vessel, and more than once have only had to twist one lingual artery.

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The table on the opposite page shows that the tongue has been removed twenty-eight times with only one death as the immediate result of the operation—an old man aged sixty-nine (Case 28). Two other deaths occurred after the operation, but they resulted from remote causes.

In Case 10, a child aged two, tracheotomy had been considered necessary as a preliminary safeguard before removing the tongue, and the child's death appeared to have more to do with the tracheotomy than with the operation on the tongue, inasmuch as the wound from the latter healed, whereas that of the former sloughed.

It will also be observed that Case 26 involved the removal of other important structures as well as the tongue. I have, however, felt bound in justice to include this case in my table.

Taking, therefore, the most unfavourable estimate, the deaths in the twenty-eight cases only amounts to 21 per cent., and, when contrasted with the 30 to 60 per cent. of deaths resulting from removal of the tongue by any other operation, I venture to affirm that substantial evidence has been submitted in favour of removal of the tongue with scissors by the method here described.

In order to illustrate the few remarks I feel justified in making on cancer of the tongue and its treatment, I have selected two of the operations out of the twenty-four I have performed as being well adapted for my purpose. The first case was recorded by Mr. Maguire, and to him I am principally indebted for the following notes :—

John Davenport, aged fifty-two, living at Runcorn, in Cheshire, was admitted into the Manchester Royal Infirmary on the 19th of February, 1880. He was a boatman, and had been a great smoker. Previous to this illness he had very good health, although twenty-eight years ago he had gonorrhœa, and two months afterwards a chancre; he gave, however, no account of any secondary syphilitic symptoms. He was the father of seventeen children, three of whom were still-born. There was no history of cancer in his family. Seventeen years ago he noticed a small warty growth on the tip of his tongue, and this gradually increased in size. Three years ago he noticed his tongue becoming covered with a whitish fur, while, for seven weeks, as he himself stated, a sore had been forming behind the warty growth, and this had increased to the size shortly to be mentioned. He had lost much flesh during the previous two months, and in the last six weeks had become deaf on the left side. At the left side of the extreme tip of the tongue, and on the dorsal surface, there was a small growth of a circular form, and about half an inch in diameter. This was slightly raised above the general surface of the tongue, and showed projections resembling filiform papillæ, the whole being covered with moist soft whitish scales. The mass was firm and somewhat rough to the touch, but was not painful.

Behind this was a circular sore, one inch in diameter, which reached from the left border of the tongue to a little to the right side of the middle line. It did not, however, extend to the under surface of the tongue. It was moist, and bright red in colour, except in its centre, which was occupied by a whitish brown slough. Its surface was granular, and its edges were everted and fungating. The tissue of the tongue near the ulcer was for a little distance swollen and hard. The inferior surface of the tongue was much swollen, hard, and tender to the touch in the region of the ulcer, but was not ulcerated.

The general surface of the tongue, from the tip to the level of the posterior margin of the ulcer was covered by whitish scales, that accumulated into a small

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CASES OF REMOVAL OF THE ENTIRE TONGUE WITH SCISSORS.

No. of Case.	Initials.	Sex.	Age.	Date of Operation.	Name of Operator.	Place where Operation was Performed.	Result of Operation.
1	J. H.	F	64	1877. Nov. 23	Mr. Whitehead	Manchester Royal Infirmary	Secondary hæmorrhage. Recovery. No return when last heard of six months after operation.
2	T. B.	M	56	1877. Sept. 27	" "	" " "	Recovery. No return when last seen two months after operation.
3	E. C.	F	34	1878. Jan. 12	Professor Lund	" " "	Discharged convalescent, Feb. 7th.
4	W. L.	M	43	1878. Sep. 7	Mr. Bradley	" " "	Left hospital convalescent, Sep. 21st.
5	T. G.	M	...	1879. June 14	" "	" " "	" " "
6	...	M	...	1879. Oct. 18	Mr. Whitehead	" " "	Convalescent in six days.
7	M. W.	F	39	1879. Nov. 15	" "	" " "	Left hospital convalescent, Dec. 1st.
8	R. T.	M	39	1879. Nov. 15	" "	" " "	" " " Dec. 3rd.
9	J. H.	M	...	1880. June 29	Professor Lund	Harpurhey, Manchester	Died Dec. 10th, 1880.
10	G. H.	F	2	1880. Feb. 12	Mr. Whitehead	Manchester Royal Infirmary	Died March 20, 1880. Asthenia. Removed for macroglossia. Case published by Mr. Maguire, *Journal of Anat. and Phys.*, vol. xiv. p. 417.
11	T. S.	M	57	1880. Feb. 25	Mr. Ewart	Higher Broughton	Died twelve months after operation. Recurrence in submaxillary glands.
12	J. D.	M	...	1880. Mar. 8	Mr. Whitehead	Manchester Royal Infirmary	Alive May, 1881, and free from return.
13	S. R.	F	46	1880. Mar. 22	Mr. Heath	" " "	Left hospital convalescent, April 15th.
14	H. W.	M	58	1880. April 10	Mr. Whitehead	" " "	Discharged relieved, April 24th.
15	T. P.	M	55	1880. April 19	" "	" " "	Discharged May 8th. Not a bad symptom throughout.
16	S. B.	M	60	1880. June 12	" "	" " "	Alive March, 1881.
17	J. R.	M	67	1880. June 19	" "	" " "	Discharged July 8th, and reported himself well three months after operation.
18	J. S.	M	49	1880. June 22	" "	" " "	Left hospital apparently cured, July 8th.
19	P. K.	M	57	1880. Sep. 24	" "	" " "	Left hospital convalescent.
20	J. W.	M	50	1880. Sep. 24	Mr. Heath	" " "	" " " Oct. 15th.
21	W. C.	M	43	1880. Nov. 5	Professor Lund	" " "	" " " Nov. 24th.
22	J. H.	M	43	1880. Nov. 27	Mr. Whitehead	" " "	" " "
23	R. B.	M	53	1880. Dec. 16	Mr. Hughes	Ashton-under-Lyne Infirmary	" " "
24	W. H.	M	63	1880.	Mr. Bartleet	The Birmingham General Hospital	Reported well three months after the operation.
25	...	...	...	...	Mr. Jolly	" " "	Died on the 21st day after operation.
26	W. B.	M	51	1881. Jan. 22	Mr. Whitehead	Manchester Royal Infirmary.	Tongue, floor of mouth, sublingual and submaxillary glands removed with scissors through incision below jaw. Death 12th day. Septic pneumonia.
27	M. D.	M	...	1881. Feb. 19	Mr. Jones	Manchester Royal Infirmary	Left hospital convalescent, March 12th.
28	J. D.	M	69	1881. Feb. 11	Mr. Lediard	Cumberland Infirmary, Carlisle	Died fourteen hours after operation from shock.

I am acquainted with the particulars of two other successful cases of removal of the entire tongue with scissors in private practice, but for special reasons I am requested not to include them in this list.

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mass near the posterior part of the ulcer. The tissue in the floor of the mouth was a little swollen, but did not feel hard.

The patient had lost some teeth opposite the sore, but had no carious teeth. The glands beneath the tongue were felt externally to be enlarged and soft, but there was no enlargement of those at the angle of the jaw or in the neck.

The patient complained much of pain, which shot up along the left side of the face to the forehead and ear, but never affected the right side. The pain was increased by eating and drinking, especially cold fluids. There was great salivation, increased (the patient said) when he sat up; he also described the saliva as tasting sweet. The breath was fœtid.

On the 8th of March, 1880, I excised, with scissors, the whole of the tongue through the mouth. The arteries were twisted, and a silk ligature was left attached to the mucous membrane in front of the epiglottis.

For seven days after the operation the patient was fed entirely by nutritive enemata. The temperature never exceeded 100°, and no pain was complained of.

On the twelfth day after the operation the patient was sufficiently recovered to leave the hospital. The floor of the mouth was in a state of healthy granulation. The patient could swallow fluids perfectly well, but could not masticate or swallow solid food. His sense of taste was perfect as far as could be ascertained. There was no glandular swelling of any kind in the neighbourhood of the mouth, and there was no pain in the side of the face. He could speak fairly well, making himself perfectly understood. The patient is alive, and free from any return of the disease at the present time, August, 1881.

This case presents many points of clinical interest, and is of great pathological importance, as presenting a somewhat rare combination of tumours of the tongue, and also as having afforded an opportunity for the examination of the so-called ichthyosis of the organ. Clinically, the diagnosis of the three different growths was clear. The nature of the papilloma was evident from its form, its slow growth, and the fact of its having been stationary for such a lengthened period.

The ulceration was inferred to be of an epitheliomatous nature from its position, its sprouting red granulations, and its everted edges, in place of the clean cut borders usually met with in cases of syphilitic ulceration of the tongue.

The patches of ichthyosis, or the leucomata, as Hutchinson calls them, were too typical to admit of doubt, and whether we regard the ichthyosis as a first stage of epithelioma, or as an independent disease, it is universally acknowledged to be at least a forerunner of the cancroid. The ichthyosis is evidently nothing more than a hypertrophy of the epithelium of the part, and is not related in any way to the disease of the same name occurring on the external surface of the body. The most important part of the microscopical examination was that which showed the epithelioma to be merely a further development of the ichthyosis, the hypertrophied epithelium of the latter disease extending into the submucous tissue, and forming the columns of cells of epithelioma.

Although the disease in this case had not apparently extended to the glands, yet the cells of the lymphatics were seen to have assumed an epithelioid character, thus affording ample justification for the removal of the entire tongue.

Now it so happened that there was admitted into hospital the previous day, February 18, 1880, a man aged fifty-eight, suffering from almost a precisely similar condition of tongue. He had a papilloma on the right side, and on the left an ulcer with indurated and everted edges and red granulating centre. On the right side,

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in front of the ulcer, and extending to the under surface of the tongue, there was a patch about half an inch square of white, indurated, enlarged, and raised epithelium, possessing all the features of the so-called ichthyosis. The papilloma had existed thirteen months, the ichthyotic patch nine, and the ulceration two months. Although there was no distinct evidence of syphilis, he confessed to having had gonorrhœa twenty-seven years previously. He had been a great smoker from his youth, and had generally used a clay pipe. There were also decayed and broken teeth in the vicinity of the ulceration. His family history was good.

On the 10th of April I removed the whole of the tongue with scissors through the mouth. He made a rapid recovery, and left the hospital convalescent on the twelfth day after operation. He died, however, from recurrence of the disease in the submental glands ten months after the tongue was removed.

These two cases may be regarded as somewhat uncommon, and I look upon them as of extreme value in demonstrating the stages of transition from the healthy mucous surface of the tongue to that morbid condition which we term epithelioma.

The mucous membrane of the tongue is highly vascular, and consequently subject to activity of change, the natural epithelium passing through the phases of its life with singular rapidity. This vitality constitutes a surface intolerant of disturbing influences, controlled, however, by remote nervous, vascular, or constitutional conditions which affect the part for the time being; and as the hereditary tendency to cancer is probably owing to an increased susceptibility of the epithelium to irritation, it can readily be understood how the irritation of a rugged tooth, or the friction of a clay pipe, generally determines the site of the future epithelioma. When we regard the histological elements of the disease in its advanced stage, what more do we find than an exaggeration and multiplication of the normal epithelial tissues of the part extending beyond their original limits, or penetrating into remote structures, and an excessive proliferation of the epithelial cells lining the alveolar walls and lymphatic ducts?

In the cases just mentioned we have illustrations of the three possible directions in which the morbid process can extend. In the ichthyosis there are the superficial and lateral extensions; in the papilloma the upward growth, and in the ulceration the deep penetration; and as it is the direction of the growth which not only regulates the history of such cases, but also their final results, we must recognize in it a feature of the greatest clinical value.

In the cases where the disease is superficial the lymphatic glands are late in becoming infected; but where the morbid process follows the direction of the lymphatic channels, glandular complications rapidly take place, and it is on these grounds we explain the long duration without gland affection in some cases, and the immediate liability to recurrence after removal in others, and it is from this knowledge that the operative procedure in each individual case should be mainly governed.

What could be more suggestive of the possibility of interrupting the disease than the history of these cases: and in what respect does the surgery of the present day fail to accomplish this apparently simple task? The majority, and we must admit, the large majority, of those suffering from cancer of the tongue die from a recurrence of the disease after operation; and although we have the satisfaction of knowing that the fault is not entirely our own, as most patients decline to be operated upon until operative treatment is hopeless, nevertheless we are to a great extent responsible, when we undertake to operate, and stop short of removing the

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whole of the disease. It is in these cases that life, and the reputation of surgery, are frequently and unnecessarily sacrificed.

If the cancer recur, we know from the nature of the disease that some part of the morbid structure has been left behind; if a permanent cure follow, the converse is equally certain. The fact that a few isolated cases of removal of the tongue affected with epithelioma remain free from disease for many years is of little value in face of the vast number of cases that end fatally within a few months from a recurrence of the disease; nevertheless, a single well-authenticated case of recovery is sufficient to disprove the dogmatic assertion that every case operated upon eventually dies from a return of the disease either in the floor of the mouth or in contiguous glands. If instead of a few cases a number could be proved to have made permanent recoveries, surgery would receive an impetus which would never be allowed to subside.

With the object of obtaining some definite and reliable information concerning the ultimate results after removal of the tongue for epithelioma, I have distributed to nearly every hospital in the kingdom forms carefully drawn up for the record of cases. I now possess 250 returns, from which I have obtained considerable information, and many valuable suggestions; but as the mere recital of statistics would be out of place on the present occasion, I reserve them for a more suitable opportunity.

In 1872 I published in the *Lancet* the particulars of an excision of the entire tongue by the galvanic écraseur. The patient at the time I operated was in her sixty-ninth year, and I have recently had the satisfaction of seeing her in perfect health at the age of seventy-eight. There has never been any sign of the disease returning, and there never was the slightest doubt entertained by the many who examined the case of its being well-marked epithelioma. The interest of this is somewhat increased by the fact of her brother having previously died from epithelioma of his lip. In this case eight years have elapsed without recurrence of the disease; but in a case recently published by Professor Buchanan, an interval of fifteen years occurred without any return of the disease. To these cases must be added a patient of Billroth's, who lived five years and seven months, and another four years. A man operated upon by Nunneley lived four years, and eventually died of an independent disease. Hutchinson mentions four patients living and well at periods of three years or more after the operation, in whom the microscope left not the slightest doubt that the disease was cancer; and in one case Mr. Lund removed a portion of the tongue of a man who two years afterwards suffered from an epithelioma of the lip, which was removed, and the man is alive and well at the present time. There are other cases recorded as recoveries where the nature of the disease is more equivocal.

Scanty as the evidence may appear, there is sufficient to suggest that if the whole of the disease is removed reasonable prospects of permanent cure may be entertained.

The different ways of removing the tongue have been frequently described.

The object of all is to remove the whole of the disease, and which is the one that best fulfils this purpose can only be proved by the results of extensive experience. At present the data are insufficient to form a definite opinion, and, pending the publication of more cases, we must rest content to act according to our individual judgment.

I have now had considerable experience in removing the tongue by most methods that have been advocated, and, as the main features of all may be

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reduced to the three principles of crushing, burning, and cutting, I may perhaps be allowed very briefly to refer separately to each.

I object to crushing, as effected by the écraseur, on the ground that it more frequently than not fails to accomplish the object intended. If the tissue of the tongue be friable, the mere weight of the instrument has been known to remove the tongue without the exercise of any special action. If the structure of the tongue be firm, generally either the wire breaks, or it fails to prevent hæmorrhage.

Removal of the tongue by the galvanic écraseur, and the galvanic or thermo-cautery, has led me to conclusions shared by many surgeons, that the risks to life are materially augmented, that the instruments are difficult of precise application, that the operation is frequently followed by secondary hæmorrhage when the eschar separates, and that it is more prone to engender conditions favourable to septic dangers than any other mode of procedure.

A very important opinion was recently expressed at the Clinical Society by Professor Stokes, from which I very materially differ, and notwithstanding the opinion comes from so eminent an authority, I feel bound to express opposite views.

He stated "that he had noted that removal of the organ by a cutting operation is not only more liable to be followed by hæmorrhage, but also, what is of more serious moment, by septic infection."

This statement is somewhat surprising, as it is directly opposed to the conclusions drawn from the returns of over 250 cases. My own experience would induce me to say that the danger from hæmorrhage when either the knife or scissors is used is simply *nil*, as the arteries, should they require it, which is not always the case, are either twisted or tied.

I do not admit that, because in some of the cases where the tongue is removed by the écraseur no bleeding takes place, the absence of the bleeding is the consequence of any action the écraseur may possess in crushing the arteries; on the contrary, I am inclined to the belief that the hæmorrhage stops from pressure, just as it will frequently stop it, when the knife is used, by the momentary pressure of a finger.

The construction of the écraseur is opposed to the belief that it can strangle within the final loop the arteries of the tongue. It is only necessary to screw up any écraseur to its utmost tension, and then observe how much larger the tightest loop is in comparison with the size of a lingual artery. If the loop is excessive compared with the arteries, what must it be when compared with the lymphatics?

If the crushing was conducted by an instrument with blades that came into final contact, I could imagine that arteries and lymphatics might be permanently occluded, and hæmorrhage and septic invasion prevented.

The difference between crushing and cutting in the influence over direct septic infection through the lymphatics invites more than passing attention. If it could be shown that the écraseur by crushing occluded the mouths of the lymphatics, as it is credited with closing the arteries, there would be no difficulty in comprehending how one of the channels for the introduction of septic poison into the system should be cut off, and thus would be established the advantage of the écraseur over cutting. But it has yet to be proved that the écraseur is even effectual in crushing the arteries, consequently it cannot be conceded that it closes the lymphatics, which are undoubtedly much smaller.

The operation I have finally adopted is the one just described, and, as I have

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never found any difficulty in boldly removing the whole of the tongue through the mouth, I see no object in dividing the jaw. If the floor of the mouth or the glands below the jaw are involved, I advocate the excision of the tongue through the mouth first, and afterwards the removal of the glands and other structures by an independent incision below the jaw.

During the last four years Billroth, who appears to have had by far the largest experience of any surgeon in removal of the tongue, has entirely abandoned the use of the écraseur on account of the enormous mortality, which he estimates at 61·5 per cent.

He now commences his operation by tying one or both lingual arteries, and removes through the wound made for this purpose, enlarged if necessary, any glands that may be affected, and afterwards utilizes the wound as a channel for the drainage of the subsequent secretions from the mouth after removing the tongue. After having filled the wound with antiseptic gauze, the mouth is opened to its full extent by means of Herster's gag, and the angles of the mouth and the lower lip are drawn apart by three double hooks.

Where the floor of the mouth is extensively affected, the teeth of the corresponding side are extracted, and the gums detached from the inside of the lower jaw by means of a raspatory. He then excises the diseased structure by repeated short cuts with a pair of curved scissors. Any hæmorrhage that may occur is immediately arrested by the ligature of the bleeding vessels. If both lingual arteries are previously tied hæmorrhage is nearly absent, and the stump looks pale, like the muscles of a limb after Esmarch's bandage has been used. If only one lateral half of the tongue has to be removed he first makes a median division, and afterwards a transverse section of the affected side.

When the operation is completed, drainage is carefully provided for, and the surface of the wound is cauterised by means of a strong solution of permanganate of potash, or the same salt in powder.

After the operation has been conducted in this manner Billroth has never experienced cellulitis, diphtheria, or bronchial pneumonia. The temperature remains almost normal, and between the third and seventh day the drainage tubes are removed. The patients are fed by means of an œsophageal tube for about the first week.

Billroth observes that the tongue can easily be removed with the curved scissors through the mouth, which he regards as a simple matter after the lingual arteries have been ligatured. The success of his operations by this method amounts to 84·2 per cent, and further testifies to the advantage of excision over the écraseur.

From this account it will be observed that Billroth's operation much resembles the one I have been in the habit of practising without knowing that any other surgeon relied entirely on scissors for excision of the tongue. The only difference in our modes of procedure appears to have been that Billroth made the ligature of the linguals a preliminary operation, while I left these arteries intact until divided during the excision of the tongue. This is a minor difference, and when the operation involves more than removal of the tongue, Billroth's method is probably to be preferred, as the submental wounds are quite as available for the removal of diseased glands as the one I am accustomed to make subsequently and independently for the same purpose.

I have recently sent to the surgical registrars of hospitals a letter urging them to adopt some uniform system of recording the results of operations on the tongue

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for cancer, and, with your permission, I will conclude by taking this opportunity of repeating the grounds upon which I think this desirable.

The past records of excision of the tongue have not hitherto been given in sufficient detail, or in such numbers as to enable the profession to arrive at any definite conclusions on many vital and vexed questions affecting this operation. Amongst the most important points in dispute, I venture to mention that considerable divergence of opinion exists as to whether removal of the tongue for cancer materially prolongs life, or alleviates suffering; which of the numerous methods advocated for removal of the tongue fulfils in the widest sense the objects of the operation; which operation is attended by the least risk to life, and prevents to the greatest degree the dangers from hæmorrhage, immediate and remote shock, and septic dangers; and which operation is the least frequently followed by a return of the disease, and is attended by the longest interval without recurrence; the advantage or otherwise of partial over complete removal, and the subsequent influence of each on the patient's powers of articulation.

There are other considerations, such as the causes influencing the rapidity of growth; the conditions regulating immediate or retarded implication of contiguous glands; and the connection between the assigned effects of irritation and initial growth; each consideration requiring the light of accumulated evidence. It is not, moreover, unreasonable to suppose that, with a methodical record of cases, aids may be found that will considerably lessen the difficulties which now obscure a differential diagnosis.

There are also many other points in connection with cancer of the tongue which are associated with doubt in the minds of surgeons, and which can only be settled by a wider field of investigation, based upon more rigid and precise lines of inquiry than those hitherto adopted. A large number of cases, carefully recorded on some uniform system, would enable us to decide many of these questions.

The vital significance and surgical importance of the tongue as the seat of cancer is best appreciated by a reference to the returns of the Registrar-General, which show with what alarming strides cancer is increasing in England.

In 1851 there were 5,218 deaths from cancer, whereas in 1879, the year of the last report, the deaths amounted to no fewer than 12,799. To make this contrast more conspicuous, I have compared the population of the two years with the number of deaths in each, when I find that the ratio of deaths in 1879 was 509 per million, whereas the deaths per million in 1851 were only 290; and on the assumption that cancer will continue to increase at the same rate during the next eighteen years, the deaths per million in 1897 will be 893, or more than three times as many deaths per million as occurred in 1851.

How far, as the Registrar-General observes, "this apparent increase is simply due to improved diagnosis, and how far to a real augmentation, is doubtful;" but when we consider that an improved diagnosis is also likely to tell in the opposite direction, we have every reason to fear that the returns of deaths from cancer are rather below than in excess of the actual number.

We have only, I regret to say, very imperfect data for calculating the number of deaths from cancer of the tongue, as there is no attempt made in the returns of the Registrar-General to specify the locality of the disease; but suppose, as I believe, from an estimate I have made, that 6 per cent. of the number included in the generic term "cancer" are cases of epithelioma of the tongue, a percentage which, I think, will not be found to be an exaggerated proportion, we may assume that 640 people died from cancer of the tongue in this country during the year 1879.

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The grave character of these statistics cannot fail, I think, to arrest the attention of thoughtful men, and stimulate the minds of scientific surgeons to increase the means at their disposal of lessening the mortality of this disease.

On the Radical Cure of Urethral Stricture by Dilating Urethrotomy.

Professor Fessenden N. Otis, New York.

Operations for the relief of stricture of the male urethra by dilatation, divulsion, or division, had been in use from time immemorial; but, according to the teachings of the best surgical authorities throughout the world, strictures are not absolutely cured by any one of these methods. All the numerous instruments and procedures which had been recommended and practised for the treatment of urethral stricture were acknowledged to be inadequate for its radical cure—in other words, a cure so complete as no longer to require subsequent treatment by the occasional passage of bougies or sounds.

Fig. 49.

The Urethrameter.

The operation of dilating urethrotomy (literally, a dilatation of strictures up to the normal urethral calibre, and then thoroughly dividing them at some one point) was first proposed by me ten years since.

In order to effect this object with any degree of certainty, it became necessary, in the first place, to ascertain with precision the exact normal calibre of the urethra in which the strictures were located.

At that time a standard calibre for the human male urethra, accepted by the profession at large, and made a basis for operations, had been fixed at 8 or 9 of the English scale (corresponding to lines in circumference), and 21 of the French (representing millimètres in circumference).

It was claimed that when a urethra, the subject of stricture, was by any means brought up to 8 or 9 of the English scale, or 21 of the French scale, the urethra could no longer be considered as strictural, and that further active treatment was unnecessary; the occasional use of sounds or bougies was, however, to be continued indefinitely.

The urethrameter, presented by me to the profession in 1874, consists of a straight canula, about 10 millimètres in circumference, terminating in a series of short steel springs (B), hinged upon the canula. These are connected also by hinge joints with the terminus of a fine metallic rod, which traverses the interior of the canula, and is moved by the stationary screw at the handle of the instrument. By the action of the screw this rod is made to retract the springs, causing them to assume an elliptoid, or bulbous form (A), which increases or decreases in circumference from 15 to 45 millimètres, in harmony with the movement of the screw. To the screw is attached an indicator, which traverses a dial marked off in millimètres, and shows the exact degree of the expansion of the

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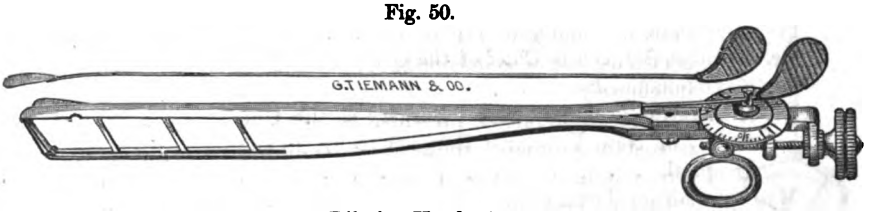
bulb. A thin rubber stall (C), drawn over the end of the closed instrument, protects the urethra from injury, and prevents the access of the urethral secretions to the interior of the instrument. When introduced, closed, into a healthy urethra, as far as the bulbo-membranous junction, and then expanded to a point which is recognized by the patient as completely filling the urethra, and is yet easily movable, the index on the dial then shows the normal calibre of the urethra at that point. By the term normal calibre, I understand the limit of easy distension without injury to healthy mucous membrane.

Accurate measurements of a large number of normal urethræ by means of the urethrameter, show that there is great variation in the size of the urethra in different individuals, and hence, that a fixed standard is impossible. The urethra varies from 28 mm. in circumference to 40 mm. or more, and the average is not less than 32 mm. It may, furthermore, be demonstrated by these measurements that the urethra bears a certain definite relation in size to the organ in which it is situated. That when the flaccid penis measures 7·05 centimètres, or 3 inches in circumference (at about the middle of the penile portion), the urethra has a capacity of 30 mm. When the penis measures 3¼ inches (8·01 c.) the urethra will be found to have a normal calibre of 32 millimètres in circumference; 3½ inches, 34; 3¾ inches, 36; 4 inches, 38; 4¼ inches, 40 m. in circumference, *or more*, the urethra increasing in size relatively to the size of the penis, two millimètres for every ¼ inch of penile circumference. In several hundred cases measured, the urethral calibre was often over, and *never* under, the foregoing figures.

A knowledge of these positions, established by subsequent experience applied to urethral explorations, make it possible to ascertain, with an exactness previously unattained, not alone the normal calibre of the urethra, but as a rule the locality and degree of stricture in any given case.

My dilating urethrotome, first presented to the profession in 1871, was constructed for the complete division—the absolute sundering of strictures—on a basis of exact knowledge of the normal calibre of the patient's urethra, and of the locality and degree of stricture. This instrument consists of a pair of steel straps connected together with short pivotal bars, on the plan of the ordinary parallel ruler—as shown in the expanded instrument, Fig. 50. Its expansion

Fig. 50.



Dilating Urethrotome.

and contraction are effected by means of a screw which traverses the handle, connected with the lower shaft, and is moved by the finger-button. This screw acts against the upper bar of the instrument as a fixed point, and on being turned moves the lower bar, dilating or contracting the size of the instrument. To the screw is attached an indicator which is thus made to traverse a dial placed upon the upper bar, which registers exactly, in millimètres, the degree of separation of the bars, and thus the amount of dilatation which is being effected in any

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given case. Up to this point the instrument is simply a dilator or a divulsor, and may be thus used by introducing it closed into the urethra, until its distal extremity is beyond the ascertained point of stricture. Then, by turning the screw, the instrument is expanded, dilating the stricture up to the previously determined normal calibre of the urethra. The upper bar of the instrument is traversed by a steel wire, at the extremity of which is a thin blade not exceeding two millimétres in breadth, and when in place is concealed in a deep slot at the end of the upper bar. Now, by means of its handle, the urethrotome is drawn out of its concealment and made to traverse the stricture (firmly fixed and made thin by the previous dilation), dividing it quickly and completely.

After an experience in over fifty operations, in 1873 I called public attention to the results of complete division of urethral stricture, by dilating urethrotomy, in six cases. A careful re-examination of these cases, three at one year from the date of operation, two at six months, and one at five months—showed entire freedom from stricture, although in the interval no instrument of any description had been passed through the urethra in either case.

The apparent radical cure of the strictures, through complete division and simple maintenance of the normal calibre of the urethra until the wound of operation had healed, without subsequent passage of sounds or bougies, seemed to warrant the expectation of similar good results in other cases. After an experience with this method of nearly ten years, it may now, I think, be safely claimed that this expectation has been amply realized.

A general summary of 1,331 operations made in accordance with the principles above enumerated, may be found recorded in my volume "On Stricture of the Male Urethra."

This covered an experience of my own in	635	operations.
That of Dr. C. H. Mastin, LL.D.	296	"
Dr. Thomas R. Brown, Prof. of Surgery in the College of Physicians and Surgeons of Baltimore	300	"
Dr. R. W. Pease, Prof. of Surgery of the University of Syracuse, New York	100	"
Total	1,331	"

Since that time—

Prof. Bevan, of Baltimore, has reported	300	operations.
Dr. Eldridge, Surgeon in Chief of the General Hospital at Yokohama, Japan	100	"
Dr. J. L. Little, Prof. of Clinical Surgery in the University of New York, and Surgeon to St. Luke's Hospital	30	"
My own additional operations	125	"
Making a grand total of	2,163	operations

without a death or permanent disability of any sort.

There have been re-examinations reported at various dates subsequent to operation, with the following results :—

Dr. Bevan, of Baltimore, out of 200 tabulated cases, reports thirty re-examinations, at periods varying from four and a half months to two years. Re-contractions were found in but *two* cases.

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Dr. Mastin, of Mobile, reports twelve re-examinations, at from four months to two and a half years, with re-contraction in *three* cases.

Prof. Pease, of Syracuse, reports that out of his first series of forty-five cases (operated on 1875 to 1877), re-examination at dates between four months and two years (thirteen of which were over one year), showed that no re-contraction had taken place (see Otis, "On Stricture," p. 262). Of his subsequent experience in 296 operations, under date July 21, 1881, he writes as follows :—" The whole number of cases in my private practice, of which I have a record, not published, is 273, comprising 395 strictures. Of this number forty-seven were deep, or between 6¼ and 7¼ inches. Out of this series of 273 cases, eighty have been re-examined, at from six months to four years from the date of operation. Out of this number of re-examinations, fifty-nine gave perfect results; twenty cases had re-contraction in one or more strictures, upon twelve of whom re-operations were made, and subsequently dismissed cured; two have drifted away, and six are awaiting a favorable time for re-operation. Urethral fever followed in nine cases—in three very severe. All recovered, however, with no bad results. In three of the deep operations severe hæmorrhage followed, controlled by the perineal crutch. In fourteen cases there was curvature of the penis, lasting from one to fifteen months.

Of my own first 100 tabulated cases, out of thirty-six re-examinations with the bulbous sound, thirty-one were found free from stricture; twelve over 6 months, three over 1 year, one 2½ years, and one 3 years after operation. In my second series of 136 tabulated cases, out of eighty-two re-examined, sixty-seven cases were found entirely free from stricture.

3	cases	6	years,	and over	6 months	after	operation.
2	"	5	"	"	"	"	
3	"	4	"	"	"	"	
10	"	3	"	"	"	"	
7	"	2	"	"	"	"	
20	"	1	"	"	"	"	
10	"	6	months	"	"	"	

In the second edition of my work two cases were cited, re-examined in May, 1880 (and thus over eight years from the date of operation), and found to be free from every trace of stricture, the urethra being free to the passage of a 30 bulbous sound in one case, and a 32 in the other. In the first case, five strictures were originally present, the smallest of a calibre of 22f. In the second case there were also five strictures operated on, the smallest 16f. In addition to these cases was one (a surgeon, operated on March 6, 1875, for four strictures, defined by a 24f. bulb, in a urethra of normal calibre, 36f. Re-examined May, 1880, and found free from stricture by the easy passage of a 36 bulbous sound—six years after operation—and no instrument introduced in the interval. Again, within the last two months I have had an opportunity of re-examining two other cases; one (the first), whose strictures, four in number, were divided from 28f. to the normal urethral calibre (in this case 36f). The second, where six strictures were originally present, ranging from 24f. to 36f. in a urethra of 38f.; and when an acute inflammatory discharge had been present for over four months. In each of these cases a radical cure was demonstrated: in the former, seven years after operation, and the latter over eight years.

Dr. Stuart Eldridge, of Yokohama (formerly Professor of Anatomy in the Georgetown University, U.S.A.), reported a case in the *New York Medical Journal*

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of May, 1879, where, from long-standing disease, the urethra was contracted to a filiform size throughout the anti-bulbous portion. The operation of M. Maisonneuve was first performed, and this immediately was followed by the introduction of my dilating urethrotome, and the urethra raised through its use to the supposed original calibre of the canal. The patient made a good recovery, and on a post-mortem examination of the case made two and a half years after the operation, the urethra was found to be entirely free from stricture. In his description of the post-mortem appearances, Dr. Eldridge says:—" The most careful examination of the specimen failed to discover the slightest pathological constriction at any point; while neither thickening nor induration could be discovered by the most painstaking search." This statement was fully verified through the specimen which was forwarded to me by Dr. Eldridge, and presented to the New York Pathological Society, in the latter part of the year 1879.

When it comes to be considered that the operations reported in the foregoing pages have occurred in the practice of but six surgeons, and that many hospitals in America, and nearly, if not quite, every hospital in the City of New York is supplied with instruments for the performance of dilating urethrotomy, and that the operation there is no longer considered a novelty; when, also, it is considered that the operation is practised by many surgeons in private practice in America, it will be understood that a record of operations considerably exceeding two thousand, might, had this occasion been sooner anticipated, have been greatly increased, and have still further confirmed the statements as to the safety and utility of the operation of dilating urethrotomy.

In my own experience of over seven hundred operations, not only have I never had a death or a permanent disability of any sort, but I can say to-day, that I have never performed the operation—as advised in the foregoing pages—either to my own regret or without marked and acknowledged benefit to the person so operated on. The operation of dilating urethrotomy, like any other legitimate surgical procedure, is important in proportion to the amount of difficulty and the gravity of the complications present.

In subjects affected with disease of the bladder or kidneys, any operation on the urinary apparatus is more or less hazardous. Dilating urethrotomy, however, has been fully demonstrated to be the least so, as in its performance the least degree of injury, necessary to division of stricture is inflicted, and besides, in the very great proportion of cases, it is not necessary to pass any instrument through the deep urethra, or beyond the locality of the stricture.

In direct opposition to the statement of authorities, who have not examined the urethra by means of the urethrameter, but who have made up their statistics from post-mortem inspection, it will be found that by far the greatest majority of strictures calling for operation, or any other treatment, are situated in the penile urethra, and will be found in less frequency the further we recede from the meatus urinarius. In point of fact, being the result of inflammatory action, stricture will be found most frequent in the anterior portion of the urethra where inflammations are most frequent and severe.

In my experience, the greatest number of strictures which call for operative measures, are the so-called "strictures of large calibre," which produce and prolong urethral discharges, sometimes causing reflex disturbances, which result in recurring epididymitis, spasmodic stricture, and retention of urine, frequent micturition (painless or otherwise), catarrh of the bladder, neuralgias—abdominal, dorsal, or sciatic; and besides, are the possible cause of a legion of troubles, mental

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as well as physical which can only be efficiently reached by removal of the stricture. For all such cases, dilating urethrotomy presents a promise, more or less positive, of speedy and permanent relief, and, as a rule, when properly performed, results in a radical cure of the stricture.]

Description of a New Surgical Engine.

Dr. G. A. Bonwill, Philadelphia.

This is the same in many respects as the dental engine. It has the same sized driving wheel, frame, flexible arm and hand-piece, except heavier. The driving wheel has attached to it two geared wheels, with a crank to be used by hand, by which the speed is increased to 15,000, and even 18,000 revolutions per minute. It can be used by foot-power, also, as a dental engine.

Any number of special instruments can be attached at the end of the flexible arm. Circular saws, from half inch to four inches in diameter, can be used for any of the major operations upon the bones, also a reciprocating saw, which cuts both ways, moving only three-eighths of an inch, and making 15,000 cuts at each movement (30,000 both ways), which cuts the bone as smoothly as if done with a plane. By it, resections of every bone in the body can be made. A screw attachment is made to the handle of the saw, which is fastened into the bone to be cut, holding it firmly to the frame of the saw. With this the thigh bone can be resected as far up as the upper third, and the end cut off at any angle with mathematical precision.

The great speed enables the surgeon to drill a hole of any size up to a quarter of an inch in any bone lying loose in the tissues, in which screws can be placed, and the bones held by them as a splint until ossific union takes place, and then unscrewed. There is no uncertainty as to the result of such an operation, as the bones can be cut so as to come into direct apposition and there held by the screw splints. The bones can be riddled with holes in a few moments, where stimulation is needed to throw out new material for union.

Large burrs of half to one inch in diameter are supplied, by which necrosed bone, whether occupying but a limited area, or the full length of the longest shaft, can be attacked and the parts shaved down in a few moments, an operation which may take hours by the chisel and mallet. Besides saving so much time for anæsthetising the patient, the parts are sure to be smoothly done and all dead bone removed.

By the saws, burrs and drills, sections of bone can be removed, taking only such as are diseased, and preserving the continuity of the remaining portion; one of the most important advances in surgery.

The removal of mucous surfaces, for the purpose of uniting them in union by suture, is no small part of its function. Operations upon the womb and any of the soft tissues in any part of the body can have the mucous membrane cut off without injury to the sub-mucous substances, and healthy granulation by first intention is the result.

The importance of this engine in this branch of surgery alone is of vast import. Its success here is beyond dispute. Instead of steel burrs, coarse corundum wheels, made of shellac as a body, rasp off the surface as if done with the keenest knife, and much smoother, and nothing but the mucous surface is taken.

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Where stone in the bladder can be reached within four inches, a diamond drill in the engine can be used to puncture the stone with numerous holes, which destroys its cohesive powers, and the stone can be broken with impunity by the fingers or weak forceps.

Where the stone can be held even slightly, the drill works fast and certain, and without moving it out of position.

The diamond cuts very rapidly on slight pressure and plenty of water, and there is little danger of displacing the stone.

In trephining, a section can be removed of any shape without endangering the membranes beneath.

Suppuration within Right Knee-joint: co-existing Necrosis of Right Tibia: Drainage of Knee-joint by Channels gouged out through head of Tibia from below. Bony Anchylosis. Subsequent Fracture through head of Tibia. Recovery.

Dr. W. Newman, Stamford.

A girl, aged 14, came under notice June, 1880, poor and ill-nourished. For six months had had necrosis of right tibia, and for some weeks implication of knee-joint; joint much distended and seat of continual pain. Large sequestrum removed from tibia. Joint conditions so bad that amputation through thigh was proposed as only means of saving life, but this was absolutely negatived. Free openings made by gouge from below upwards on two occasions through tibial head into cavity of joint with immediate relief, large discharge of pus and diminution of tension.

Six months afterwards was discharged with firm anchylosis between tibia and femur, in good health, and with a limb which bade fair to be very useful.

Within a month after leaving infirmary had a severe fall: the limb was bent backwards as she fell. Three weeks after accident the limb was found to be at an obtuse angle, and there was fracture through the tibial head. Limb straightened at once under anæsthetics, grating evident during the manipulations. Treated as recent fracture; eight weeks afterwards consolidation had well advanced, and again the child is able to walk with crutches, bearing some weight on the damaged limb.

Papers on the following subjects were offered, but, from want of time, were not communicated to the Section:—

L'Application de la Mégéthomètrie au Traitement de la Fracture Simple du Femur.

Dr. Houzé de l'Aulnoit, Lille.

Des suites de la Fracture du Tibia seul, et de la Rèsection Partielle du Peroné dans les Fractures.

Dr. F. Cuignet, Lille.

Système des Proportions Appliquè à la Situation des Interlignes Articulaires.

Prof. O. Soupart, Ghent.

Ueber den Werth und Vorzüge der Urethrotomia interna nach Maisonneuve.

Prof. Kosiuski, Warsaw.

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INAUGURAL ADDRESS,

BY THE PRESIDENT,

Professor THOMAS LONGMORE, C.B.,

Professor of Military Surgery in the Army Medical School at Netley.

COLLEAGUES of the naval and military services of our own and foreign countries, and all friends who are good enough to assist in the work of this, the Section of Military Medicine and Surgery,—Allow me, in the first place, to say how sensible I am of the

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honourable and responsible nature of the position in which I have been placed as President of this Section, and to express my thanks for the trust you have reposed in me. I accepted the office with very great diffidence, in obedience to expressed wishes which I felt I could not do otherwise than comply with. I only entertained the hope that I might be able to discharge its duties satisfactorily with the help of the eminent vice-presidents, council, and other officers of the Section, on whose support and assistance I felt assured I might confidently rely.

In the second place I wish, in the name of my British colleagues, to say a word of welcome to our foreign friends, who have not hesitated to come in many instances from places at remote distances, to join in the labours of this section. We feel honoured that men so distinguished in the science and practice of naval and military surgery should have come among us, many of whom bear names that are not only household words in their own respective countries, but are familiar in every part of the world where the progress of military medical science is watched, and improvements in its practice are studied. It is a peculiar pleasure to me to welcome those of our foreign *confrères* who have been able to attend the Congress; for among them are many who have been friendly colleagues at previous meetings in various parts of Europe for purposes closely akin to the objects of our present gathering—the advancement of surgical knowledge and the improvement of the means of alleviating the sufferings inseparable from a state of warfare. We hope that our foreign friends may find themselves well repaid for their journey to London on the present occasion; and that, on returning to their respective countries, they may not only feel that the part of the time which has been devoted to the discussions at the sectional meetings, and to observation of the scientific collections which have been specially formed for the occasion, has been passed profitably, but that also the time spent in our social gatherings has afforded an opportunity of renewing old friendships and forming fresh associations which will remain sources of very many pleasant reminiscences in the future.

Although this is the seventh meeting of the International Medical Congress, it is the first, I am informed, at which there has been a special section for the consideration of subjects connected with military medical and surgical practice; and yet there seems to be a wide field of work for such a section in a professional congress of the kind. Although the true principles of medicine and surgery must be true everywhere, in military as in civil practice, yet everyone practically acquainted with the conditions inseparable from service in the field knows that the application of those principles has perforce to be so modified that the modes of application themselves become a distinct branch of study. In saying this I do not confine myself to the mere physical or manual application of these principles—to the performance of a surgical proceeding in this way or that way—I refer to matters of far more general influence. Consider how the whole range of what is generally spoken of as "conservative surgery" has to be modified in field practice from the mere influence of the circumstances by which our patients are surrounded in campaigning. How many injuries are there that would be appropriately treated in civil hospitals by an expectant method of treatment, by methods calculated to preserve the injured parts, and with good grounds for hoping to secure restoration of their function, which on numerous occasions the military surgeon would not dare to undertake, from his acquaintance with the dangers to life that such a practice would entail on his patients under the exposures, repeated changes from station to station, and other sources of disturbance to which they would have to be subjected in field practice. Remember also the special considerations which arise out of the characters and complications of the great bulk of the injuries themselves which have to be dealt with in military practice—those caused by gunshot. How

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greatly, again, the constitution, organization, the internal administration, the mobility of armies, affect everything bearing on surgical practice in them; the quantities and descriptions of surgical and medical materials that the military surgeon can have at his disposal for his patients; the amount of protection, care, and attention he can obtain for them; the means at his command for transporting them from place to place without aggravation of their injuries. How much patient thought, what prolonged examinations and trials, what experience have already been brought to bear on these subjects, and how much still remains to be done in order to attain results which may harmonize with military necessities and, at the same time, conduce to the welfare of the patients who are placed under our charge! I may allude, further, to the many subjects for deliberation which the crowding together of bodies of troops and animals in the close quarters in which they often have to be placed gives rise to; the means of warding off the diseases which such conditions are apt to engender, the means of combating and extinguishing them when they have sprung into existence. In naval professional practice, too, in ships of war, the need of special attention being given to many subjects of medical and surgical interest seems to be obvious. Although the officers and men have the advantage of being in their ordinary house and home on board ship, with its usual furniture, and many things may be carried with them that are not available to soldiers in the field, still, in modern ships of war, with their artificial light below the water-mark, artificial supply of air, large amount of complicated machinery, and limited space for movement, what a necessity there must be for special consideration regarding the means of preserving health, and of the arrangements to be made, and the means to be adopted for dealing successfully with injuries which may happen on board, especially when they occur in large numbers, as in the case of an action with the enemy. I need not dwell further on the importance of a section in such an International Congress as the present being specially devoted to subjects bearing on the pursuit of medicine and surgery in fleets and armies. A large proportion of those who are present probably have some personal experience of military practice, and, if so, already know how much advantage may be anticipated from meeting at annual intervals for the purpose of discussing special professional topics which are still involved in doubt or obscurity; for making mutually known and sifting the experience gained under varied military circumstances and conditions; for trying to arrive at definite rules of practice; and, in short, for acquiring any knowledge which may particularly conduce to the benefit of the officers and men who depend, often without power of appeal, on our judgment and skill for restoration to health in sickness and for safety and repair when subjected to wounds and injuries. Although belonging to all nations, we have the advantage of being able to meet together without national jealousy, and with no other rivalry than that of vying with each other in endeavours to discover what may most benefit the sick and wounded. It is not with our province of thought and action, as it is in some measure with those other parts of military science and practice, on which national safety or superiority in power may depend. A certain amount of reticence in regard to them is justified by national self-interest. We can speak quite openly of all our professional plans and arrangements. If they contain any features better than those belonging to our neighbours of other countries we have no fear of imparting them. We hope, indeed, if they are really better, that they may be adopted and turned to account; for, if practically applied, our own people may possibly be among those who will be benefited by their adoption. And, if we lack anything in our military hospital system which our neighbours have better than ourselves, we have no reason to suppose that they will object to imparting information on the subject to us; they, in turn, may be benefited by our improvements. Neither surgeons nor patients on either side can be harmed by mutual confidence in matters appertaining to technical

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knowledge or departmental arrangements. Even in time of war there are no enemies within our sphere of action.

The subjects on which observations are to be read at the present meeting of this section, and on which discussions are expected, are all subjects having an important bearing on naval and military surgery. I need not enumerate them, as they are already before you in the printed programmes. I will only observe that the most urgent of the questions named for debate at this meeting appears to be the manner in which the antiseptic treatment of wounds can be best carried out in time of war. This question, more or less, covers and influences many of the other subjects put down for consideration, such as the advance of conservative surgery in field practice, the treatment of injuries of blood-vessels in the field, improvements in field-hospital equipment, and several others. As you well know, the experience that has hitherto been gained in the strict application of antiseptic principles to the treatment of wounds in the field has been obtained only under exceptional circumstances and is, comparatively speaking, still exceedingly limited; but, such as it has been, the published results have been so largely superior, both as regards saving life and also as regards the restoration of the usefulness of wounded parts, to the published results of any other methods of treatment in the field, that, so long as this difference holds its ground, we are morally bound to try and extend the practice. All military surgeons, however, can readily perceive the practical difficulties that lie in the way of applying the antiseptic precautions and details of treatment inculcated by Lister, owing to the peculiar conditions incidental to military arrangements in time of war. It appears to me that one of the chief points to be settled, looking at the question from the point of view of military practice, is whether the action on the air by the antiseptic spray is an essential part of the treatment; whether some of the other forms of antiseptic treatment advocated by eminent surgeons are, or are not, capable of producing equally favourable results? If the action on the air by atomized antiseptics be an essential part of the proceeding, then the hope of applying it under the ordinary circumstances of warfare seems almost desperate—so free, not to mention other obstacles, is the access of air, and frequently so strong is its movement, in the field, at the dressing station, and in all tent hospitals. On the other hand, if this part of the process can be dispensed with, then the question will be greatly simplified, and attention will only have to be directed to the description of antiseptic applications and dressings which will best answer the intended purpose, and to the manner in which these dressings can be rendered available, consistently with other military requirements, when and where they may be needed, and in adequate quantities for meeting the wants of each particular occasion. Experience is not wanting of antiseptic treatment being carried out by eminent surgeons in some civil hospitals without the use of the spray at the operation table, and in other instances without its use during any part of the course of treatment; and, it has been alleged, with no less beneficial results than when the spray has been employed. It remains to be proved how far these observations are thoroughly correct, and satisfactory proof on the point can only be arrived at when sufficient experience has been collected on the subject.

I have been asked to give at this meeting an explanation of the system by which help is arranged to be afforded to the wounded among troops on active service according to the existing regulations of the British service. It has been suggested that to those who have not had occasion to study the subject this description would not only possess some features of novelty and interest in itself, but would also furnish indications of the extent to which particular modes of treatment of wounds and injuries might be capable of application under the arrangements described. In accordance with this suggestion I brought the two diagrams which you see before you as a ready means of furnishing the explanation required.

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The surgeons who are now entering the British military medical service can scarcely realize the greatness of the changes which have occurred in their branch of the profession during the last twenty-five years. When I commenced my military service, the British army was scattered in comparatively small detachments over the kingdom and in every colony. It was for the most part engaged as a safeguard for peace and good order in our own possessions, and occupied in performing duties that are now discharged by police, rather than in preparing itself for the sterner necessities of a time of war. Still more marked was this aspect of matters with respect to the army medical service. It was entirely a peace establishment; and its duties were conducted as if there were no liability to the state of peace being interrupted. Nothing was prepared for a condition of war, whether as regards plans of administration for field service, organization of field hospital establishments, means of transporting sick and wounded during a campaign, or the descriptions of field hospital equipment to be employed. The results of the practical experience in these matters which had been gained during the Peninsular campaigns had gradually disappeared. In conducting the duties of the medical service, the attention had come to be largely devoted to economical details in small matters, which, in the aggregate, produced but comparatively trifling results; while the general system on which the hospitalization of the sick was conducted was cumbrous, wasteful, and needlessly costly. This condition of things received a rude shock when the Crimean war occurred, and when, as the Director-General of the Army Medical Department at the time testified, without any records or patterns to guide him, everything for active service in the field, both as regards the field-hospital establishments, kinds of supplies, and forms of ambulance vehicles, had to be improvised for the occasion. The break-down from want of systematic preparation which then occurred (not a break-down so far as the surgical staff were individually concerned, but a break-down in respect to the establishments with which they were connected) led to prolonged investigations, which not only demonstrated the need for a thorough reform of departmental arrangements and regulations, but also showed the directions in which the changes were required to be made. The experience of successive wars on the continent of Europe and in the United States since that period has, step by step, led to further developments in military medical organization for field service no less than it has done in the purely combatant parts of the army.

The principal problem which the medical department has had to solve has been to devise a scheme by which help and protection should be afforded speedily and effectively to the wounded over the large area which modern battles in Europe now usually occupy, and to provide for their subsequent treatment so long as circumstances may render their stay on the theatre of war a matter of necessity; one, at the same time, which should be capable of being modified and adapted to all the varying conditions of warfare—that is, variations as to features of country, difference of climate, seasons, numbers of troops engaged, opportunities of shelter, and other such matters; that should not only not impede, but, on the contrary, should work in harmony with, all the other military arrangements; that should be economical in regard to the number of officers and men employed; that should include an equipment which, while adequate to the needs of the hospital service, should not exceed the means of transporting it; and, lastly and particularly, a scheme that, as regards the *personnel* necessary for the field-hospital and ambulance duties, should be little more than a redistribution of the *personnel* ordinarily employed in the stationary hospitals at home; so that, acting as a peace establishment as long as peace might last, the *personnel* should, at a very short notice, be capable of being organized and arranged into the different parts composing the war establishment.

The plan adopted for accomplishing these various objects may, for description, be

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conveniently divided into two parts—viz.: (1) that for the medical service from the base of operations to the limit of the area of active operations of the army in the field, and (2) that for the medical service with the army itself. The hospital establishments belonging to these two divisions of the medical service are shown separately in the two diagrams. They are mutually in a great measure independent of each other, although connected and working in concert; and they differ in their qualities, administration, and in many articles of their equipment, although the officers and men of the army medical department serving in them are interchangeable. The establishments at the base of military operations and along the lines of communication with the army have more or less of a stationary character, while the others are organized for being as movable as the troops which they accompany.

In making a survey of these establishments it must necessarily be a rapid and rather superficial one. It will be convenient, perhaps, to start from the base and to follow them to the front, so far as the establishments along the lines of communication with an army in the field are concerned.

The insular position of our country leads to complications in the medical preparations for war from which continental nations are for the most part free. The officers and men of the medical service must at first be conveyed in detached bodies on board ship, as well as the hospital stores and equipment. The vehicles which are to convey the stores, as well as the carriages for the wounded, must be made capable of being folded up into convenient packages for being carried by sea to the place where the army is to be formed before commencing its operations. Continental armies can generally move in their accustomed ways, and collect their forces in a convenient place on their own frontiers. Wherever a British army may have to be concentrated for hostile purposes, whether on part of a coast belonging to a friendly ally, or whether it be in a position secured by force from a hostile power, the establishment of a new hospital in the place is one of the first necessities experienced. It is required at once to receive the sick and hurt who have accumulated in the transports during the voyage from England, and will be wanted to receive the casualties that are sure to happen while the force is being collected and formed prior to starting for the special purpose of the expedition. It is required for the reception of all the medical and surgical stores that have been brought from England for use during the campaign. If the position is retained as the base of operations, the hospital thus established will continually grow in importance as long as the campaign lasts. When once the military operations are in progress, a large proportion of the patients that result from them will find their way ultimately to this hospital, and here they will be disposed of, whether they are sent back to activity in the field or are invalided to England. Again, as the military operations approach their termination, and other hospitals that have been formed successively in advance are broken up, their occupants will fall back upon this base hospital, so that it will be the last medical establishment to be closed as well as the first to be opened in a campaign. The base hospital is consequently required to have a more permanent character than other hospitals in the field. Some available buildings are usually secured for its occupation, and these may be supplemented by subsidiary buildings or encampments, according to circumstances. The administrative as well as the executive staff are large in number, especially the executive staff, not only on account of the extent and variety of the duties to be performed, but also because it is the principal position where reserves of medical officers can be conveniently retained for replacing casualties in the field and for supplying the demands which the military movements occasion.

As soon as the army quits the place of rendezvous and commences its march, casualties will occur of various kinds, and often in larger numbers than might be anticipated by

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those who have not studied the experience of such occasions. While comparatively near to the base, those that become disabled can be sent back to the general hospital established there, but after the troops have advanced to some distance this would be inconvenient, and fresh hospital establishments have to be opened at suitable positions along the roads which the troops are following. These, then, become the stationary field hospitals along the lines of communication, or, as they were formerly called, the reserve, or intermediate, field hospitals. They are placed in situations which are not only suitable as regards sanitary considerations and hospital needs, but also as regards safety; in positions which the military authorities believe to be safe from incursions of an enemy, and from which the communication with the front in one direction, and with the base in the other direction, may be expected to remain secure. They may be established in buildings, in villages, or towns, or camps near them, or near railway-stations. The equipment allotted to each of these hospitals is very similar to that of a field hospital, only differing in having an increased quantity of hospital clothing, and in not being supplied with special transport vehicles.

In front of the stationary field hospitals, between them and the movable field establishments, is the "advanced depôt." At this station a supply of medical and surgical supplies and appliances is stored, ready for issue to meet wants in the bearer companies and movable field hospitals. This is the station, too, to which the sick and wounded are brought from the field by the bearer company's ambulance waggons, and from which they are forwarded by vehicles obtained from the commissariat department to the stationary hospitals along the lines of communication with the base.

These comprehend the establishments between the area of active military operations in the field and the base. The duties to be performed, so that the patients in the stationary field hospitals may have their wants properly attended to, so that there may be no interruption of the movements of men and materials between the field and the base to and fro, are so onerous and important that they are placed in the separate charge of a particular surgeon-general named for the purpose, as the military duties are in that of a general officer distinct from the general commanding the troops in the field. A surgeon-general acting under the orders of a general officer commanding the lines of communication has the special direction of all the medical duties along the lines and at the base. He is responsible to the surgeon-general-in-chief of the army for their regular fulfilment. The hospitals and the movements of sick along each road of communication are supervised by a deputy surgeon-general, acting under the directions of the commandant of the road. The charge of the advanced depôt is placed in the hands of a surgeon-major. At the base are three deputy surgeons-general, whose respective duties are indicated on the plan before you. It is at the base hospitals and in the stationary field hospitals along the lines of communication with the army that the regulations direct all civilian surgeons and other persons affording voluntary aid to the sick and wounded to be employed.

The establishments with the army actually operating in the field must now be glanced at. They are of three kinds: (1) The regimental establishment. (2) The bearer company. (3) The field hospital. It will be more convenient to trace these from the front to the rear, from the fighting line to the line of the field hospitals, this being the direction in which help has to be afforded by them on the occurrence of an action with the enemy.

The first surgical establishment belongs to the battalions and other bodies of troops composing the brigades and divisions of the army, and is of a very spare character. It is only organized for giving temporary help during halts of the troops on the march, or, in case of action, affording such primary aid to the wounded as may be necessary

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before the second establishment, the bearer company, can reach the spot. Each corps has a surgeon with it, and two men of each company are trained as stretcher-bearers. These form the corps field surgical staff. The ordinary equipment consists of two "medical field companions," cases containing surgical materials and medicines, carried by straps over the shoulder, two water-bottles per company, and a stretcher per company, carried in the company cart. In case of a corps being detached on outpost or other duties, larger cases, field panniers, carried on the backs of pack animals, and other equipment, according to circumstances, are supplied.

The second establishment, the bearer company, is the most important source of aid in case of the occurrence of an action, and is very fully and carefully equipped for its duties. It comprises the means of performing all surgical operations of urgent necessity, and of applying surgical dressings and giving preliminary aid to all wounded, wherever they may be met with, before their removal to the field hospitals in the rear. It is specially organized for giving this aid in a systematic manner at certain important stations: in the immediate rear of the fighting line; at places to which the badly wounded are first carried, and where they can be transferred to wheeled conveyances; and at the established dressing-stations. It is by the bearer company that the wounded are removed from help-station to help-station, on stretchers from the place of fighting to the transfer or collecting station, by ambulance-waggons from the transfer to the dressing-station, and again from the dressing-station to the field hospitals, and from them again subsequently to the advanced depôt. To accomplish all these purposes, each bearer company in the field has a considerable *personnel*, over two hundred in number, allotted to it—medical officers, officers of orderlies, transport officer, men of the Army Hospital Corps, stretcher-bearers, drivers, and artificers. The details of these establishments may be found in the Code of Army Medical Regulations. The equipment is also large, and includes all the supplies for forming the dressing-stations, for performing the necessary surgical duties, stores of medical comforts, the ambulance conveyances, and a variety of articles, the lists of which are also laid down in the Medical Regulations. Each bearer company is divisible into two "half bearer companies," with *personnel* and equipment complete for its duties. The whole company is under the command of a surgeon-major. Four such companies constitute a bearer column, the complement for an army corps; and one of these companies has its transport and equipment adapted for work in a mountainous country, where wheeled transport could not be employed. The command of a bearer company is a very responsible one: the movements of the company, whether in marching with troops, in camping, or in discharge of its special functions on the occasion of an action, have to be conducted with the same military discipline and precision as the movements of all other parts of the army; the bearers must be drilled and exercised in the proper modes of carrying the wounded, and both they and the men of the Army Hospital Corps must be taught and practised in the modes of rendering first assistance in the absence of surgical aid. All this constitutes part of the work which is now systematically done in the Army Hospital Corps Training Depôt at Aldershot. I may here mention that arrangements have been kindly made by the principal medical officer and the medical staff at Aldershot, to receive a certain number of visitors who may wish to attend the bearer-company exercises at that station on the 10th instant. It is all the more important that the duties of the bearer company should be performed with thorough efficiency, especially as regards the preliminary dressing and treatment of the wounded, as, owing to the great range of modern projectiles, the field hospitals, as shown by modern experience in European warfare, are often established so far away from the place of conflict, that many hours may elapse before the wounded in the transport vehicles will be able to reach them and get fresh

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surgical help. This is especially liable to happen when an action only ceases as daylight is declining, and the roads between the battle-ground and the places where the field hospitals have been established become encumbered by military vehicles of all sorts, as well as by the movements of troops.

The field hospitals remain to be noticed. A great deal of thought has been given to the proper constitution of the establishments so as to combine qualities of portability and readiness at any time for use with the requisite efficiency. A field hospital must be so arranged as to be always within reach of the troops; ready for reception of the wounded from the bearer company, shortly after the occurrence of an action, with all requisite means for their protection, care, and treatment, for at least several days, if necessary; and it must be capable, as soon as the patients have been removed from it to the advanced depôt, of being quickly packed up again ready for further movement forward, so as to be available for the reception of a relay of patients in case of another action with the enemy. The field hospital, as now arranged in the British service, is fitted for the reception of 200 patients. The requisite number of tents, each calculated for four patients, is carried for acommodating the whole number, but if a farmhouse with out-buildings be available, or suitable houses in a village can be obtained, the tents would probably not be employed. Each field hospital has its *personnel*, equipment, and transport so arranged as to be capable of division into two half field hospitals complete for 100 patients. Twelve field hospitals form part of each army corps, two being allotted to each division of the army corps, the remaining six being reserved for disposal wherever they may be specially needed. In addition to four store waggons for the ward and cooking material and medical stores, four for tentage and equipment, and two water-carts, each movable field hospital has two pharmacy waggons, containing a complete equipment of instruments, surgical appliances, medicines, and means of dispensing them, with a stock of medical comforts. The contents of the waggons, and modes of packing them, the order to be observed on the line of march, and the plan of encamping a field hospital, with other details regarding such establishments, may also be found in the Army Medical Regulations.

It will be seen from the account I have just given that the regulated plan of medical and surgical assistance for troops on active service is a very complete one, and that it anticipates all the wants that are likely to occur during a campaign. It is obvious that, to carry out the arrangements when the strain of a general action occurs, and help is demanded for a large number of wounded, previous training, intelligence, and active exertion on the part of all concerned, will be necessary for success. An adequate staff is provided by the regulations for the purpose. In addition to the special surgical and subordinate staff of the field hospitals and of the bearer companies, the surgeon-general-in-chief with the army corps has a deputy as a field-inspector, whose duty is to see that the circulation of the system of help is properly maintained, two orderly medical officers, and other officers to assist him in his charge. Each division of the army has a deputy surgeon-general at its headquarters, and each battalion and separate corps its surgeons. In all these arrangements, the calamity of a state of warfare in Europe has been particularly had in view. But even in other parts of the world, even in half-civilized countries, the general principles of the system can be maintained; the manner in which they are carried out has alone to vary, according to the nature of the military operations, the condition of country, and other local circumstances.

The sketch I have laid before you of the existing arrangements for ensuring systematic help and skilled attention to the wounded in case of this country unhappily becoming involved in war, has been unavoidably a superficial and imperfect one, and I must apologize for its incompleteness. The description, short and partial as it has been,

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may, however, not be without some advantage, if it has succeeded in conveying a general idea of the arrangements under which any particular system of treatment, whether the strictly antiseptic treatment of wounds, or any modification of it, will have to be applied among troops when on field service.

What did the late Russo-Turkish Campaign teach us concerning the Antiseptic Treatment of Wounds in War?

Surgeon-Major H. Melladew, London.

It taught us that the more rigidly the method of antiseptic treatment is carried out, the more brilliant are the results obtained. However unfavourable the conditions under which it is practised, however much its completeness of detail is interfered with, and even rendered next to impossible, yet we find that the principle of antiseptics when carried out always conferred benefits upon the wounded, by lessening their sufferings, by promoting more rapid healing of their injuries, and saving limbs and lives. It lightens the work of the surgical staff by diminishing the number of dressings, and affording more time for examining the more extensive injuries and necessary operations. More rapid evacuation of the wounded men from the front into better air and more cheerful surroundings, far from the battle-field, where, with better food, careful nursing, and more thorough rest than in the overcrowded hospitals near the theatre of war, convalescence is hastened, and many a man returned to the ranks who in former days would probably never have rejoined his comrades in the field. From a study of various reports which I have been able to procure, of surgeons employed during the late Russian campaigns, and from conversations with those whom I had the privilege to meet during a visit to Russia last winter, I have no doubt that the more the principle of antiseptics is pushed on to the battle-field, followed at the dressing station, and continued in the field and other hospitals, the more satisfactory will be the results obtained.

The dictum of Professor Esmarch, that the "Schwerpunkt" of antiseptic treatment is on the battle-field itself, and that of Professor Von Nussbaum, that "the fate of the wounded man lies in the hands of the surgeon who first attends him," have proved themselves true to the letter in the late war. It was with primary antiseptic treatment that Bergmann and Reÿher were able to save almost all their cases of gunshot fractures into the knee joint.

Other figures, published by Pirogoff, Cammerer, Pinkerton (with the Turks), Subbotin, Unterberger, Watrazewski, speak in favourable terms of the antiseptic treatment of wounds in war; but all reports show clearly how hopeless, and next to impossible, it is to carry out the method in its completeness of detail as practised in peace hospitals. With the large numbers of wounded suddenly thrown upon the surgeon's hands, no one method of treatment can strictly be followed. Follow it as much as possible; for any part of it, however small, is better than none. With the best will to carry Listerism fully into practice, Dr. Bergmann, at the crossing of the Danube, when the wounded were comparatively few in number, found himself face to face with obstacles which soon combined to render details impossible. The Danube water, collected for dissolving the carbolic acid, was thick with mud and sand, and no amount of acid would take away its foul smell; there was no time for boiling or filtering it. The spray diffusers soon became choked and useless. On the other hand, it was no easy task to clean the wounded limbs of soldiers after weeks of marching over the dusty Roumanian

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steppes; and when, lastly, dressings had been applied, days and days followed spent on country carts, springless, and on wheels which were square instead of round, over a roadless country, which was either bottomless with sticky mud, or overlaid by a cloud of the blackest dust.

The practice of the more successful surgeon has always been to close the wound with the least delay possible, and without disturbing it, by a firm pad overlaid by cotton wool or other antiseptic material, and immobilising the limb afterwards in case of fracture or injury to a joint, thus rendering it fit to bear a lengthened transport, with the danger of infection of the wound reduced to a minimum. This is one of the greatest boons conferred by the antiseptic treatment, and the principal aid to conservative surgery. In Professor Bergmann's most successful cases of gunshot fractures into the knee-joint, the first dressing on the battle-field consisted of wrapping the limb, after it had been washed with a solution of carbolic acid, in ten per cent. salicylic cotton wool, thickly rolled round the wounded joint; a bandage confined this dressing close to the limb, which was finally immobilised in plaster of Paris. Many wounds thus closed by immediate occlusion, were found to be healed on removal of the first dressings. One highly important condition, much insisted upon, is, however, attached to this, that no examination of the wound by probe or finger be made on the battle-field or dressing station. If at the dressing station no large cavities are found to be opened, and no large vessels injured, it suffices to clean the parts surrounding the wound by some antiseptic lotion and to occlude the wound without drainage. Wounds which gape, in which foreign substances are apparent, or which already show signs of infection, should be examined; if explored, drainage tubes must be adjusted before the dressings are applied. Retained bullets should not be searched for or extracted at the dressing station, but in the field hospital when necessary in consequence of inflammation or suppuration. The best surgeons should be at the dressing station, for here must be decided upon the necessity or otherwise of primary amputation or resection; the general course of treatment to be pursued should here be fixed. If an exploration of the wound be decided upon, every eventuality must be prepared for; if this is impossible, better leave the examination to some future day. If possible, complete antiseptic apparatus should be here, and it is a great desideratum that the surgeons who first dress the serious cases at the dressing station should continue attending them in the field hospital.

The overwhelming majority of wounds in modern warfare being from rifle bullets are especially well adapted to being at once closed on the battle-field by an antiseptic pad; time is thus gained at the dressing station for the more important injuries. The application of immobilising dressings, such as plaster of Paris, &c., takes the place in the great majority of wounds of primary operations at the dressing stations. No fracture should be moved without properly applied splints or other means of keeping it thoroughly at rest. Watrazewski's experience of the plaster of Paris dressings, especially in gunshot fractures of the femur, when applied before reaction has set in, is very unfavourable. The long transport which such cases had borne before their arrival at a hospital without the possibility of having them watched, resulted frequently in blood-poisoning, mortification and gangrene. He recommends Professor Volkmann's flat tin splint instead of plaster of Paris. Septic wounds can be made aseptic even after a fortnight has elapsed between the receipt of the injury and the first dressing; of this there is abundant evidence in the writings of Bergmann and Reyher.

A great variety of dressing materials rendered antiseptic in many different ways were used during the late war; every surgeon on setting out had probably the wish to carry out closely the precepts of the antiseptic treatment, and his own favourite material and method of using it, but circumstances soon combined in overwhelming force, obliging him

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to rest satisfied with but a small attempt, or even forbade one altogether. In the varied list of antiseptic materials employed by the surgeons at work during the Russo-Turkish war, carbolic acid is always found in solutions of various strength, or combined with various dressing materials, such as cotton wool, gauze, or tow. Of Bardeleben's moist carbolized tow, Cammerer and the German surgeons under him speak in the highest praise as a first antiseptic dressing for wounds after a long transport, wounds which generally were in the worst possible condition. Dr. Vahl recommends dry carbolized tow especially for transport, as the wet is apt to dry lumpy and cause eczematous rashes. Bergmann used salicylic cotton wool, others gauze; Dr. Gaehae boric lint. Marked success attended the labours of those surgeons who carried out the principle of antiseptics: that it is of the highest importance, in time of war, has been amply proved in the late Russian campaigns. It was the good fortune of but few to be in the position fully to carry it out, but the reports of those surgeons who had the means at their disposal amply show what line of treatment should guide the military surgeon in future wars, and that certain changes in his own equipment, as in that of the soldier, of the bearer companies, &c., are urgently called for.

I hope that in conclusion I may be allowed to say a few words about an antiseptic dressing packet proposed to be carried in future wars by every soldier, sewn, if possible, in the breast of his tunic immediately below the collar bone, where it would form a soft cushion.

I have tried several materials in different ways, and the result lies before you.

No. 1.—The envelope is made of waterproof cloth, closed air and water-tight. Its price, empty, in large quantities, would be about three half-pence; its size, filled, is 4¾ by 4 inches. It contains, as shown in the open packet, a full-sized triangular bandage with safety pin, wrapped round two squares—3 by 3 inches each in size, of a new antiseptic material, called, as far as I can make out at present—"wound dressing." These squares are each enclosed in air-tight folds of waterproof paper, closed by means of a solution of india-rubber. When the "wound dressing" is taken out of its paper envelope it expands again to its normal thickness, as shown by the specimens. One is intended for each wound, to be overlaid by its present wrapper. The wholesale price of this packet complete would be three-pence.

Packet No. 2 consists of an envelope of pure india-rubber, and has similar contents to those of No. 1. It is more expensive than the other—about four-pence wholesale, empty; and, though able to bear a much greater heat than it would under ordinary circumstances be ever exposed to, it probably would not stand so much rough usage as the other.

Antiseptic Treatment of Wounds in the Field.

By Deputy Inspector General Lilburne, R.N., London.

I will divide the subject into two heads: first, the method to be adopted in the field and temporary hospitals while following the troops in action; and, second, that to be pursued in the stationary hospital at the base of operations; giving special attention to the first.

1st. The field and temporary hospitals.—The two great difficulties to be overcome are the antiseptic spray and gauze; the first on account of the necessity of an assistant to attend to it, and the second the large quantity of gauze that would be required. For the spray I would substitute a brass syringe with a metal nozzle, and for the antiseptic gauze carbolized oil, a mixture of eucalyptus oil in spirit and

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water, or, what would be more handy in the field, a mixture of this oil in olive oil similar to the carbolized oil. The following is a list of the materials necessary for carrying out the proposed treatment:—

A solution of carbolic acid, 1 to 20.

A mixture of carbolic acid in oil, 1 to 7.

A mixture of eucalyptus oil in olive oil, 1 to 10.

A solution of chloride of zinc, 40 grains to the ounce of water.

Lint, hand-made in preference, as it is easily torn; protective, jaconet cloth on gutta-percha tissue; silver wire with buttons for deep sutures; carbolized and chromicized catgut for sutures and ligatures; india-rubber tubing or chromicized catgut for drainage purposes; carbolized oakum, common oakum soaked in the carbolic acid solution will answer very well; sponges, kept carefully clean, or a piece of clean rag will suit for cleansing the wound; bandages; a brass syringe with a metal nozzle; a metal saucer for the oil, and a basin for the solution.

I shall now endeavour to illustrate the mode of use of these materials, assuming a case of lacerated wound of the fleshy part of the thigh. Wash out the wound with the carbolic solution, laying over it a guard of lint or linen rag soaked in the ordinary one-to-twenty solution, keeping as much of the wound covered as possible, and irrigating the exposed parts at intervals by means of the syringe or a sponge. When the hæmorrhage is checked, and before laying in the drainage material, wash out the whole wound with carbolized lotion, using the syringe for the deeper parts of the wound, to make certain that all septic influences are removed, then lay in the drainage material, either the chromicized catgut or india-rubber tubing, taking care that they are in favourable positions for free discharge from the wound. If the wound is deep and requires support, use the silver-wire suture with leaden buttons, taking a deep hold of the lips on each side. In the intervals between the button sutures use the carbolized or chromicized catgut sutures; the latter, from various circumstances, may not be necessary in the first instance, especially if it is deemed advisable to re-dress soon. The wound is now ready for the outside dressing; lay over it a piece of protective, then apply three or four layers of lint soaked in carbolized oil or the eucalyptus preparation, over this a pad of oakum, soaked in the carbolized solution, or carbolized tow, covered by a piece of jaconet or gutta-percha tissue, and a bandage, the jaconet is the best. The pad of oakum or tow, in addition to soaking up the discharges, gives support and protection from injury during the removal of the patient. Should there not be time or opportunity for all the above dressings, the following may be used as a temporary substitute: Wash out the wound carefully, check the hæmorrhage, lay in between the lips of the wound a piece of lint well soaked in carbolic or eucalyptus oil, bring the parts together, apply a large pad of oakum, and over all a piece of jaconet and a bandage.

2nd.—On the subject of treatment in the stationary hospital I have only to say, Professor Lister's method should be strictly carried out. It would be an advantage to attach to each patient sent in from the field a card stating the necessity, or otherwise, of immediate attention, so that the medical officers at the stationary hospital could at once select and attend to the urgent cases.

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Ueber den antiseptischen Wundverband im Felde.

Oberstabsarzt Dr. D. Port, Munich.

Da beim Lister'schen Verbande das flüchtige Antisepticum einen Stein des Anstosses bildet, so habe ich mir die Frage vorgelegt, ob durch Annahme fixer Antiseptica die Methode keine Einbusse erleidet.

Ich habe zu diesem Behufe bei einer grossen Reihe von fixen antiseptischen Mitteln die Grenze ihrer Wirksamkeit festzustellen gesucht und dann geprüft, wie sich diese Mittel in den erforderlichen Concentrationen gegen Haut und Wunde verhalten. Da es ganz gleichgiltig ist, ob die antiseptische Wirksamkeit an wirklichen Wundsecreten oder an anderem eiweisshaltigen Material geprüft wird, so konnte der erste Theil der Aufgabe durch Laboratoriumversuche gelöst werden, welche wegen der Gleichmässigkeit der äusseren Bedingungen den klinischen Versuchen entschieden vorzuziehen sind. Ich gehe nämlich von der wohl feststehenden Voraussetzung aus, dass die Aufgabe der Antiseptik ganz einfach darin besteht die Wundsecrete vor Zersetzung zu bewahren. Frisches Rindfleisch wurde in Verbandstoffe eingewickelt, die vorher mit den verschiedengradigen antiseptischen Lösungen getränkt und dann getrocknet worden waren. Das umwickelte Fleisch wurde in Gläser gebracht, die mit einer Glasplatte geschlossen waren und dann der geeigneten Temperatur ausgesetzt. Nach Ermittelung des unerlässlichen Procentgehaltes, den die Verbandstoffe haben mussten, um Fleisch vor Fäulniss zu bewahren, blieb es der klinischen Beobachtung vorbehalten, sie weiterhin auf ihre Verträglichkeit mit der Körperoberfläche zu prüfen.

Bei diesen Untersuchungen ergab sich das unerwartete Resultat, dass die meisten der fixen Antiseptica in einer solchen Concentration angewendet werden müssen, dass sie auf der Haut, besonders wenn dieselbe benetzt ist, einen intensiven Pustelausschlag hervorbringen. So sind fast alle Chloride der Metalle sehr wirksame Mittel zur Hintanhaltung der Fäulniss, aber sie lassen sich wegen der ätzenden Wirkung, die sie gleichzeitig auf die Haut ausüben, nicht verwenden. Auch das Chlorzink, das von Bardeleben empfohlen wird, kann ich nach meinen Versuchen nicht gutheissen. Zur sicheren Verhinderung von Fäulniss ist eine höhere Concentration erforderlich, als die von Bardeleben angegebene, und in dieser Concentration verträgt es sich nicht mehr mit der Haut. Salicylsäure in Form von 10 pro Cent Salicylwatte hat durchaus nicht die gleiche antiseptische Wirksamkeit, als wenn sie in Pulverform aufgestreut wird. Dagegen hält sich Fleisch, das in Carbolgaze gewickelt und dann unter Glasverschluss aufgehoben wird, vollkommen unversehrt beliebig lange Zeit.

Ich muss aus meinen Versuchen mit Entschiedenheit den Schluss ziehen, dass fixe antiseptische Stoffe nicht im Stande sind, die Carbolsäure im Listerverbande zu ersetzen.

Um von der überraschenden Wirksamkeit der Carbolsäure eine recht anschauliche Vorstellung zu bekommen, empfiehlt es sich folgendes kleine Experimente anzuztellen. Wenn man unter einer grösseren Glasglocke ein Stück Fleisch aufhängt und auf den Boden der Glocke, also weit entfernt vom Fleische ein Paar Tropfen einer concentrirten spirituösen Carbollösung bringt, so wird durch die Carboldämpfe, die sich unter der Glocke ansammeln, das Fleisch in kurzer Zeit so vollständig imprägnirt, dass es auch nach dem Herausnehmen aus der Glocke nicht leicht eine weitere Veränderung eingeht. Es wird unter der Glocke vollständig geräuchert. Dieses Experiment verhilft uns auch wie mir scheint, zu einem richtigen Verständniss der ingeniösen Einrichtung des Listerverbandes. Ich glaube, wir können denselben unbedenklich als eine sinnreich modificirte Räucherkammer betrachten, deren Wand vom Mackintosh gebildet wird. Die Carboldämpfe, die sich aus den carbolisirten Verbandstoffen entwickeln, durchdringen alle Theile dieser Räucherkammer und conserviren dadurch die ergossenen Wundsecrete.

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Die Weglassung des Mackintosh wäre die unglücklichste Modification, die man an dem Listerverbande anbringen könnte, denn man hätte dann eine Räucherkammer ohne Wand, aus der die Carboldämpfe unbenützt entweichen würden. Ich glaube, dass man am Listerverbande ungestraft verschiedene Modificationen anbringen kann, indem man z. B. den Spray und Silk protectif weglässt oder die Gaze durch Jute ersetzt, aber an dem flüchtigen Antisepticum und an dem luftdichten Abschluss des Verbandes wird man niemals rütteln dürfen.

Es gibt übrigens einige fixe antiseptische Mittel, welche die Wunden nicht reizen, z. B. Salicylsäure, und mit welchen vorzügliche antiseptische Erfolge erzielt werden können, wenn man nur nicht darauf besteht, sie in Form des Listerverbandes anwenden zu wollen. Sie müssen als Streupulver verwendet werden, und bedürfen weder eines Verbandstoffes, mittelst dessen sie aufgetragen werden, noch eines luftdichten Abschlusses. Im Gegentheil, diese beiden Zugaben, die beim Listerverfahren unentbehrlich sind, sind hier nachtheilig.

Wenn wir das Listerverfahren als einen Wundräucherungsprocess aufzufassen vermögen, so wird es uns auch nicht schwer fallen, die Analogie zu erkennen, die meines Erachtens zwischen dem Streupulververfahren und dem Einsalzen des Fleisches besteht. Die Salzlake, welche der Fleischsaft mit dem Salze bildet, entspricht der concentrirten antiseptischen Lösung, welche das Blut und die Wundsecrete mit ihrem eigenen Wasser aus dem aufgestreuten Pulver bereiten müssen. In solchen concentrirten Lösungen können Zersetzungen nicht stattfinden. Um einen möglichst hohen Concentrationsgrad sicher zu stellen, wird es sich empfehlen, nicht nur viel Pulver auf die Wunde zu streuen, sondern auch der Verdunstung des Wassers keine Hindernisse zu bereiten. Zum Streupulververfahren gehört Nichts, als das Pulver und höchstens noch eine leichte, permeable Bedeckung, um das Pulver vor dem Abfallen zu schützen. Neben dem Listerverfahren scheint mir auch das Streupulververfahren für die Verwendung im Kriege in's Auge zu fassen sein.

Nachdem wir die unerlässlichen Bestandtheile des Listerverbandes kennen gelernt haben, können wir uns nunmehr der Frage zuwenden, durch welche Vorkehrungen die Anwendung desselben im Kriege zu ermöglichen ist. Noch bis vor Kurzem erschien es als eine unüberwindliche Schwierigkeit beim Ausbruch eines Krieges die erforderlichen Quantitäten carbolisirten Verbandstoffes aufzutreiben und dieselben ohne kostspieligere Behelfe für entsprechend lange Zeit in wirksamem Zustande zu erhalten.

Von den fünfzehn Feldsanitätsanstalten, die zu einem deutschen Armee-Corps gehören, führt jede fünfzig Kilo Jute. Diese grossen Quantitäten in den wenigen Mobilisirungstagen nach der alten Methode zu imprägniren, zu zerzupfen, zu trocknen und schliesslich wieder zu pressen, wäre ein Ding der Unmöglichkeit. Man würde dazu sehr viel mehr Zeit, Hände und besonders Platz brauchen, als jemals zur Verfügung gestellt werden können. Es ist daher als ein grosser Gewinn zu betrachten, dass wir durch Oberstabsarzt Dr. Münnich in Berlin ein Imprägnationsverfahren kennen lernten, welches an Einfachheit und Raschheit Nichts zu wünschen übrig lässt und welches darin besteht, dass die gepressten Jutepackete, ohne zuvor gelockert zu werden, mit einer spirituösen Lösung von Carbolsäure übergossen und dann, in Pergamentpapier eingewickelt, eine halbe Stunde liegen gelassen werden. Nach dieser Zeit ist die Durchtränkung des Packetes eine ziemlich gleichmässige geworden und die Jute zur sofortigen Verwendung geeignet. Die Carbolgeist-Jute von Dr. Münnich ist als ein höchst schätzbares Präparat willkommen zu heissen.

Gerade die ausserordentliche Einfachheit dieses Verfahrens könnte übrigens zu einem Irrthum verleiten, dessen Besprechung ich nicht unterlassen möchte. Man könnte es nämlich für zulässig halten, anstatt fertige Carbolgeistjute mit in's Feld zu nehmen,

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dieselbe erst draussen bei jedesmaligem Bedarfe frisch zu bereiten und zu diesem Zwecke Jute und Carbolgeist getrennt in den Lazaretwägen mitzuführen. Es muss constatirt werden, dass ein derartiger Plan kaum ausführbar ist. Die Quantität Carbolgeist, die für die Tränkung von fünfzig Kilo Jute erforderlich ist, beläuft sich auf 42 Liter. Ob man diese 42 Liter in einem Fass oder in 42 Literflaschen mitnehmen wollte, jedenfalls werden sie einen ganz ungebührlichen Raum in Anspruch nehmen, dessen Beschaffung kaum zu ermöglichen ist, da die Lazaretwägen mit andern Gegenständen bereits vollständig angefüllt sind. Jeder Gegenstand, der neu in die Lazaretwägen aufgenommen werden soll, muss sich in die bereits vorhandenen Gegenstände hineinstecken lassen. Es ist durchaus nothwendig, Jute und Carbolgeist schon vor dem Ausmarsch miteinander zu vereinigen, dann braucht der letztere keinen besonderen Raum mehr und es handelt sich nur noch darum, Mittel ausfindig zu machen, welche die Carbolsäure in den Jutepacketen zurückzuhalten vermögen.

Die Entweichung der Carbolsäure zu verhindern, ist lediglich Sache der Verpackung. Wenn wir z. B. die fünfzig Kilo Jute eines Feldspitals in eben so viele Blechbüchsen verpacken würden, so hätte es keine Schwierigkeit, durch Verschmieren und Verkleben der Deckelränder die ursprüngliche Quantität Carbolsäure beliebig lange zu conserviren. Aber auch wenn Blechbüchsen nicht zur Verfügung stehen, können wir uns durch folgende Verpackungsweise aus aller Verlegenheit ziehen.

Wenn wir ein imprägnirtes Jutepacket in trocknes Pergamentpapier recht dicht einwickeln, so geht vom Inhalt Nichts verloren, weil das trockne Pergamentpapier sowohl für Spiritus als Carbolsäure impermeabel ist. Um das Pergamentpapier möglichst dicht an die Jute anzupressen, und um überhaupt eine gegen äussere Schädlichkeiten schützende, feldmässige Verpackung herzustellen, empfiehlt es sich Packpapier in mehrfacher Lage darüber zu kleben. Damit aber die Feuchtigkeit des Kleisters nicht zum Pergamentpapier gelangen kann, so muss vorher noch eine Umwicklung mit Paraffinpapier gemacht werden. Nach dreimonatlicher Verpackung zeigte sich das Innere eines solchen Packetes noch feucht vom zurückgebliebenen Weingeist und die Jutefasern zeigten intensiven Carbolgeschmack, da von der Carbolsäure wahrscheinlich gar nichts verloren gegangen war.

Das luftdichte Material, das zur Deckung der Wundverbände erforderlich ist, wird man am bequemsten in der Weise unterbringen, dass man es gleich den einzelnen Jutepacketen beipackt. Statt des theuren Mackintosh wäre vielleicht mit Paraffinhanfpapier auszukommen, wovon etwa zwanzig Bogen auf ein Packet Jute treffen müssten. Der geeignetste Platz dürfte zwischen der Pergament- und Paraffinumhüllung des Packetes sein und zwar auf der Ober- und Unterseite des Packetes, während die Seitenflächen desselben zum Aufschneiden freizulassen wären.

Es ist gewiss als ein Vortheil zu betrachten, wenn statt roher Jute, die erst einer halbstündigen Vorbereitung bedarf, ein Präparat mitgeführt wird, das zum augenblicklichen Gebrauche geeignet ist. Keine Arbeit, die im Voraus gethan werden kann, sollte im Kriege auf den letzten Augenblick verspart werden, wo so viele unvermuthete Hindernisse und Verlegenheiten zusammenzukommen pflegen. Richtig und sicher und allgemein wird die Antiseptik nur dann zur Ausführung kommen, wenn es zum Grundsatze gemacht wird, sowohl Jute, Gaze, als Baumwolle nur im antiseptisch fertigen Zustande auf dem Kriegsschauplatze erscheinen zu lassen.

Als kleine antiseptische Ausrüstung habe ich mit Prof. von Nussbaum ein Nothverbandzeug zusammengestellt, das von jedem Soldaten mitgeführt werden soll, und das folgende Bestandtheile enthält:

1.—Zwei Pulverkapseln mit je 1·5 Gramm Salicylsäurepulver zum Bestreuen der Wunden;

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2.—Ein Paar Gramm Salicylwatte, die auf die bestreuten Wunden zu liegen kommt. um das Abfallen des Pulvers zu verhüten;

3.—Eine zwei Meter lange Rollbinde mit Nadel.

Diese Verbandzeuge sollen in die Uniform eingenäht werden und müssen desshalb sehr flach sein. Da sie ferner in der Uniform verschiedenen mechanischen Insulten ausgesetzt sind, z. B. beim Ausklopfen der Kleider, ferner der Benetzung durch Regen und Schweiss, so muss die Verpackung nicht nur sehr solid, sondern auch wasserdicht sein. Die vorhin genannten Ingredienzen wurden desshalb in eine dünne, nicht verlöthete Blechkapsel eingeschlossen und darüber Packpapier in mehrfachen Lagen geklebt, dessen Oberfläche schliesslich mit Paraffin und Schellack überzogen wurde. Die Blechkapsel, welche dem Packet seine Widerstandsfähigkeit verleiht, dient auch dazu, beim Aufschneiden mit dem Messer den Inhalt zu schützen.

Antiseptic Dressing of Wounds in Times of War and Peace.

Surgeon-General Dr. Beck, 14th Army Corps, Carlsruhe, Baden.

I wish to state my opinion, and the results of my experience, of a simple antiseptic dressing which I have employed for some time past. It is made of cheap materials, and can be easily used in a dry state and without any special preparation.

The Lister dressing materials are expensive, and the imitations and modifications of them do not answer. These materials are not calculated for the poor, or for the army, particularly in the field. The dry antiseptic dressing materials prepared in manufactories, as salicylic wadding, carbolized wadding, or jute, soon lose their disinfecting powers from evaporation, and become inefficacious. The preparation of such dressing materials in hospitals, even when it is much simplified, requires much time. If large stores are kept, and remain a long time on hand, the loss of their antiseptic properties is to be feared. Bandages dipped in antiseptic solutions, and applied damp, have not had the desired effect; the influence of the antiseptic remedy is lost by evaporation, and the humidity has often proved pernicious. Led by predilection for soft, flexible, and elastic bandages, such as imbibe moisture quickly, I have, for at least thirty years, always chosen fine, clean, soft wadding, and I still prefer the absorbent cotton wadding to all other materials for dressing. I endeavoured also to discover a means of rendering every bandage quickly and easily antiseptic, in such a manner that, even after remaining for some time on the wound, the antiseptic portion of the material should not be so much diminished as to be without effect. In the course of the year 1880, experiments for the purpose were undertaken in the surgical division of the garrison hospital in this town under the superintendence of Dr. Tomasczewsky, and these led to the most favourable results; the healing of wounds being all that could be desired. As the description of dressing referred to has been used by other surgeons, also with the best results, I think it well worth while to publish an account of it. The method is as follows:—When the dressing is to be applied, cut square pieces of the best absorbent cotton wadding, according to the size of the wound. These pieces may either remain of their original thickness, or may be separated into halves. The inner side of every half must then be sprinkled with 10 per cent. spirituous carbolic-acid solution, care being taken that the carbolic-acid solution penetrates the dressing material in its full strength. The sprinkled inner surfaces are now laid one upon another, and the

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pieces of wadding, thus prepared, immediately applied to the wound. To give an idea of the consumption of dressing material, according to this method, I will mention that from one pound of absorbent cotton wadding (price for the Carlsruhe garrison hospital one mark and a half) a sheet of from 80 to 100 centimetres square was obtained, and for sprinkling one pound of wadding on an average 120 grains of the 10 per cent. spirituous carbolic solution was used. Of course, the solution might be used stronger, if necessary.

The wadding is generally applied in single layers, laid on the protective; but in important cases is used in double, or even in thicker layers. For this purpose additional clean wadding, slightly sprinkled with the solution, is employed. In cases where a profuse discharge exists, or is expected, a piece of disinfected gutta-percha tissue must be laid over the whole. The dressing must be secured by prepared gauze bandages, which have been dipped in watery carbolic solution of 2 per cent. strength, and well wrung out just before being used. Several turns of this bandage should be laid over the cotton wool dressing. The bandages must be tightly drawn, so that the wound may be closely covered. Of course, the time of changing the dressing must depend upon circumstances. According to the report of my military colleague, this mode of dressing possesses all the acknowledged good qualities of the absorbent wadding. The material is easily pressed together and packed so as not to take up much space, is very easily divided by the fingers, and, still better, when necessary, with scissors. The dressing of a wound can be accomplished with facility, as the softness, flexibility, and elasticity of the wadding cause it to be easily adapted to all inequalities of surface. The wadding does not easily become loose. Although it is compressed during the dressing it soon equalizes itself by its elasticity, and it fits and closes tightly the sides of the wound, a most important point. A further advantage of the absorbent cotton wadding is its air-filtering qualities. Another advantage is its considerable imbibing power. In one respect, this is a disadvantage; for, if the suppuration of a wound be very copious, the wadding soon becomes saturated with matter, and is therefore no longer an effective air-filter. This, however, is seldom the case. The secretions of the wound can only penetrate the network of the wadding, so long as they are thin, as, when thick, they cannot do so. The secretions are then retained, and in this case the dressing must be renewed. The imbibing qualities of the absorbent cotton wool cause it to be a very appropriate material for cleaning the wound and the parts surrounding it. It was thought that impregnation of the wadding with a fixed antiseptic material, such as salicylic acid, instead of a volatile one, was the most efficacious method of preventing decomposition of the matter imbibed by the wadding. It has been proved, however, that salicylic wadding is quite an uncertain preparation, since it often contains only one-fourth of the quantity of salicylic acid stated. This results not only from the escape of salicylic acid in the form of dust, but also from the fact of the salicylic acid in the watery solution used for impregnating the wadding evaporating considerably during the process of preparation.

Of all the various antiseptics used, carbolic acid is generally considered to be the most reliable. It is, however, very volatile, and it is difficult to keep it fixed in the bandages. The dry carbolic jute dressings are injurious from the dust they give rise to, their want of flexibility, and absence of imbibing qualities. Generally speaking, jute loses almost all the carbolic acid contained in it after lying twenty-four hours on the surface of the body. Jute dipped in a strong spirituous solution of carbolic acid loses the best part of its carbolized contents during the process of drying, and the rest soon evaporates. A description of dressing must therefore be sought for in which the carbolic acid is so fixed that its evaporation shall only take place by slow degrees. It was for

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this purpose that the interpolation of the carbolic acid between the inner surfaces of the layers of absorbent wadding was adopted in the manner previously described.

Experiments made by Dr. Issleit in the dispensary of this place under the superintendence of the Staff-apothecary Mavors, showed the following results. On the 18th of October a 2 per cent. carbolized wad was prepared by laying on with the spray 40 grm. of a 10 per cent. alcohol-carbolic acid solution on 20 gr. of absorbent cotton wadding in the manner before described. The wetted layers were laid one upon another, folded up, enclosed in thick packing-paper, pressed by a piece of wood with a weight of 10lb. upon it, and kept in a temperature of from 10° to 12° R. The definition of the phenol contents of the wadding was executed according to the method of Koppeschar.*

In thirty-two days, as a result of experiment, the loss was found to be 0·621 per cent. or 0·0225 per cent. per day.

The occurrence of only a moderate diminution of the carbolic acid was thus confirmed. After becoming dry, the carbolized wadding retains what remains of the carbolic acid so firmly that its antiseptic qualities may be safely warranted. In seven days the contents sink only 0·2 per cent. But the first dressing can seldom remain so long; it is almost always necessary to renew it at a shorter interval of time.

I will not enlarge upon the consideration that wet bandages are apt to cause infection by fermentation. A practical disadvantage of the use of wet bandages is the possibility of the bedding becoming damp, if the waterproof underlayer is not carefully attended to; besides, many patients dislike the feeling of coolness accompanying them, which, indeed, is often injurious. A principal fault of the wet jute dressing is that it presses on particular parts, unduly, as it cannot be laid on as evenly as the cotton wool, while also, in itself, it is not soft and flexible, but uneven, rough, and heavy. With my dry light wad-dressing, nothing of the sort is to be feared; on the contrary, the patients dressed with it felt very comfortable, and the parts were maintained in an equable temperature. It is also certain that the wet carbolized dressing, owing to the continual contact of the skin with the carbolized solution, causes an irritation which leads to erythema, eczematous ulcerations of the skin, and even to deep decubital necrosis; and thus it often becomes impossible to continue the wet carbolic dressing. My dry wad-dressing, on the contrary, is a very mild method of treatment, and can be applied for weeks to the most sensitive parts of the body, as to the eyelids, genitals, &c., without causing any pernicious results. Lastly, it has been observed that wet carbolized bandages have caused toxic effects; with my method of dressing this can never happen.

From these observations it may be deduced that my dry carbolized wad-dressing possesses great advantages in comparison with other antiseptic bandages, as it can be prepared easily under all circumstances, even at a dressing station in the field.

I hope therefore that it may be taken into general use.

DISCUSSION.

Professor Esmarch, Kiel: The question, where to begin with antiseptic treatment in field practice? is a question of principle. On the battle-field a genuine antiseptic treatment is impossible, and a paramount principle ought to be laid down: "non nocere!" The experiences of our Russian colleagues have verified my opinion that a very great number even of the most severe gunshot wounds remain aseptic, if not touched or examined by dirty fingers or instruments. Every insufficient trial of rendering wounds aseptic on the field of battle, as well as examination of any kind, ought to be omitted; the

* *Journal of Analytic Chemistry of Fresenius*, xv. 3, 1876.

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wounds should only be sheltered from the obnoxious influences of transport, by covering them with any antiseptic material and the usual bandages. From this point of view, I have constructed this little bandage parcel (the contents of which are inscribed on the cover). If every soldier has such a parcel about his person, his wound may be dressed even without a surgeon's assistance.

Dr. SHINKWIN, Cork: spoke in favour of the cotton wadding plan of treatment, and gave his experience of it, but strongly advised the use of the spray in conjunction with its employment.

Dr. MARINUS W. GORI, Amsterdam: It is true that most of the questions as to whether asepticism can be applied on fields of battle must be decided by experience; but to establish how much of antiseptic apparatus may be omitted without danger, is a question to be solved in time of peace. Two principles are predominant: (1) To touch the wounds with nothing but aseptics; (2) To cover them as soon as possible with antiseptic material. The covering of the wounds as a preparation to transportation is the most important part of first help under all circumstances. I therefore draw attention to a kind of wound-cover manufactured at Schaffhausen, and bearing the name of "compresses antiseptiques." This is a combination of A. Guerin's cotton wadding dressing with Lister's gauze, and a piece of paraffin paper. As the size or importance of the wound may require, this dressing may be folded double, once or twice, and secured by a swathe or triangular kerchief. A second point of equal importance in connection with Listerism in military surgery is how to preserve the antiseptic dressing. Several dressings have been proposed to be carried by the soldiers of the ranks and of the bearer companies. However beautiful it may appear, this proposal will meet many difficulties in application. By keeping these dressings in the uniform they will lose their antiseptic qualities, and become the fruitful germ of infection. Therefore it would be desirable to distribute the antiseptics among the combatants only before battle. Part of the ammunition likewise is carried by the men, but the greater part is conveyed by the battalion cars and the vehicles of the artillery, and, if necessary, distributed during the fight.

Surgeon-General MOUAT, V.C., C.B., London: I shall confine myself entirely to the real question before the section—the difficulties of the antiseptic treatment known as Listerism in this country, in its application to the field. The antiseptic treatment of wounds is not new. It has been practised, although not systematically, for the last 40 years, and perhaps earlier. It was practised by myself in India in 1840, in a case of amputation of the fore-arm for a gun-shot injury. Twenty-four hours after this accident, the wound was infested with maggots. I found the application of a weak solution of creosote suspended in water at once destroy the maggots, and union by the first intention took place in eleven days, and three or four before the ligatures came away. This form of antiseptic treatment—that is, simply washing or irrigating the wound—was subsequently practised in the General Field Hospital for wounded in the Crimea, after the attack on the Redan, on the 18th of June, when all the large wounds and amputations became infested with maggots, and the mosquito gauze was found useless in preventing their reproduction. I claim for the creosote solution (one drachm to a gallon of water) the property of preventing the common fly depositing its larvæ on the wounds and dressings. It is inexpensive, portable, and easily made. The antiseptic treatment with permanganate of potass (Condy's fluid) was practised extensively in the New Zealand War of 1864–65, with the best results. Nine cases of excision of the

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shoulder-joint (all secondary operations) recovered under its use, without bad symptoms. No case of pyæmia, erysipelas, hospital gangrene, tetanus, or secondary hæmorrhage occurred. The wounded were treated in wooden huts and tents. So far as I am able to follow Professor Esmarch, I entirely concur with him, that the system called Listerism is not applicable to the field, and I think this will be found to be the experience of most military surgeons who have attempted it.

Mr. J. H. Casson, London: from observations in the Armenian campaign of 1879, believed that antiseptic treatment (strict Listerism) was impossible in the first lines of fighting, from (1.) Want of time. (2.) Impossibility of transporting *matériel* to the actual spot. A modification is possible and advisable. He recommended carbolized glycerine for first dressings, picked oakum as envelopes.

Dr. C. Reÿher, St. Petersburg: remarked that operations analogous to the *débridement primaire* (laying open gunshot wounds and fractures by incision for the purpose of exploring, treating, and asepticising them) had been executed by him at the dressing-station with good results, such as were previously unknown in field practice. For instance, seven primary excisions of the shoulder-joint all succeeded, whilst in the Franco-German war of 1870–71, amongst 31 cases the mortality amounted to 41·93 per cent. Amongst 9 primary excisions of the elbow he had but one fatal case, which expresses a mortality of 11·1 per cent., against 28·0 per cent observed amongst 45 cases in 1870–71. Nearly the same good results have been obtained by *débridement* in cases of injury of the bones of the upper limbs and of the lower below the knee-joint Higher up on the thigh, the primary *débridement* proved always fatal. Now, although Dr. Reÿher likewise had a great number of good results in wounds healed under the scab, he believes it still an open question if the scabbing treatment should be adopted in front of the field hospitals without exception, or if it is, on the contrary, permitted to perform *débridement*, or even excisions of joints, earlier at the dressing-stations, and, if so, in which cases, and in the injuries of which part of the body, these operations should be performed. Dr. Reÿher would like to see that question discussed, not with regard to the possibility of carrying out one or the other method, but from the broader point of view, whether the scabbing treatment should be adopted under all conditions, or the *débridement*.

The President then summed up the principal arguments of the debate, and pointed out the general agreement which existed as to the impracticability of using the antiseptic spray under the ordinary circumstances of warfare before the arrival of the wounded in a field hospital. Even then it had been generally felt that it could only be employed in a few special cases, owing to the urgent demands on the time and attention of the surgeons on such occasions. Although Dr. Reÿher had been able to use the spray at a dressing-station, and to turn it to very valuable account, it was not to be forgotten that the peculiar advantages enjoyed by Dr. Reÿher were such as could scarcely ever fall to the lot of military surgeons in the ordinary discharge of their functions. By employing antiseptic first dressings, which Professor Esmarch had done so much to perfect, and by due attention to the general principles of aseptic surgery, there ought not now, however, to be any serious impediments in the way of carrying out the antiseptic treatment of wounds in field practice.

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On the Treatment of Injuries of Bloodvessels in the Field.

Professor F. Esmarch, Kiel.

1.—The indications in the treatment of injuries of the larger arterial trunks, and of traumatic hæmorrhage, have been considerably simplified by the introduction of "antiseptics" and of Esmarch's bandage.

2.—The practice, formerly in use, of ligaturing arteries above the wound is uncertain, and ought, therefore, to be entirely discarded.

3.—The application of styptics is equally objectionable, not only because of their uncertain action, but because they foul a wound and interfere with its healing.

4.—In all cases of hæmorrhage threatening life, the wounded vessel must, if possible be exposed at the seat of injury, and ligatured above and below this point, either with catgut or antisepticized silk.

5.—This operation must be carried out under the most rigid antiseptic precautions; if in one of the extremities, an Esmarch's bandage must be applied. As it is necessary in all such cases to disinfect the wound, even to its innermost recesses, the disinfection and the operation can be done at the same time.

6.—The most effectual method to render such operations easy, rapid, and thorough, is to make a free incision parallel with the axis of the limb. When life is at stake, it matters little whether such an incision is one inch or one foot in length, provided that the hæmorrhage is arrested, and that the wound is kept sweet; the large wound heals just as well as the small one.

7.—After freely incising the skin, the operator inserts his left index finger deep into the wound, and, with a button-pointed bistoury, opens up, just as freely, the deeper layers of connective tissue, fasciæ, and muscles, while an assistant now separates the parts with retractors.

8.—The blood-clot, which is generally found to fill the wound, and to have infiltrated the surrounding cellular tissue, is now rapidly turned out, either with the fingers, or sponges, or raspatories, partly because it hides everything, and partly to avoid subsequent decomposition. It is only under these circumstances that any operation can be carried out with anything like exactitude.

9.—As soon as this is done, the operator feels with his finger for the vascular and nerve trunks, and endeavours, with the aid of a clean sponge, to learn the exact nature of the injury.

10.—When the large veins are empty and collapsed, it is sometimes difficult to distinguish them from strands of connective tissue. On this account it is advisable to provide a little reserve of blood, which may be done in this manner: For the arm, a cord might be fastened round the wrist below the wound, before the elastic (Esmarch) bandage was applied; then, on loosening this and raising the arm, the blood shut up in the hand would, if the vein was injured, flow into the wound, and would so make it manifest.

11.—As soon as the injured spot of the artery or vein has been laid bare, so that its full extent can be clearly seen, the vessel must be isolated and then securely ligatured, either with catgut or carbolized silk, above and below the injury. If the continuity of the vessel has not already been destroyed by the injury, the vessel must be cut between the two ligatures. The operator should convince himself that no lateral or deep branches are given off to the injured part of the trunk. Should any small branches be

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found they ought to be carefully isolated, ligatured, and then separated from the parent trunk.

12.—The Esmarch bandage should now be removed, and all bleeding vessels carefully tied, the limbs being raised, as after amputations.

13.—Should any nerve trunks or tendons have been divided, their extremities must be sought for and sewn together, either with fine catgut or carbolized silk.

14.—Any foreign bodies (bullets, bits of bone, or fragments of clothes) which may be found should be removed.

15.—The whole wound must now be thoroughly disinfected, either by irrigation, or rubbing in chloride of zinc, or strong carbolic solution, taking care that the fluid gets into every little recess.

16.—After counter-openings have been made in suitable places, and drainage tubes put in, the wound must be closed with antiseptic sutures, and finally the antiseptic compressive-dressing applied.

17.—This operation can hardly be carried out on the battle-field, or in a first field-hospital, because it requires both time and care, and because the necessary antiseptic precautions are only to be had in a thoroughly well organized lazaretto (hospital).

18.—The elastic bandage can alone be recommended for the provisional arrest of hæmorrhage on the battle field.

19.—The use of styptics is very objectionable; all such remedies ought therefore to be banished from the surgeon's field-case.

20.—The application of tourniquets, formerly so much in vogue, is likewise to be deprecated as uncertain and dangerous, not only because their application requires precise anatomical knowledge, but, and chiefly because, however accurately the pad may be adjusted, it is almost sure to slip aside during transport; the binder then interferes not only with arterial but also with the venous circulation. As a necessary consequence either the hæmorrhage recurs, or, if an external wound is closed, dangerous blood infiltration of the limb takes place.

21.—An effectual and permanent compression of the vessel can be secured by means of an elastic tube, applied, while extended, two or three times round the limb. The soft structures are in this way compressed on all sides, and the circulation is entirely arrested.

22.—As this compression is efficient to whichever part of the limb it is applied, no anatomical knowledge is necessary. It is in no way interfered with by transport, and it cannot slip if the extremities of the tube are carefully fastened.

23.—On this account all tourniquets should be removed from the armamentaria of field hospitals, and be replaced by elastic tubes.

24.—Every wounded man, on whom hæmorrhage has been provisionally arrested by an elastic tube, ought of course to be removed as rapidly as possible to a hospital, in order that the compression may be removed, and the ligature of the wounded vessel immediately undertaken.

25.—It is very advantageous, before putting on the elastic tube, to apply a bandage to the limb while in a raised position. If the bone is fractured, a fixative bandage of some kind ought to be applied for transport.

26.—As india-rubber, when it has been long laid aside, disused, loses its elasticity and becomes spoiled, suitable bands and tubing cannot be preserved for years in military storehouses, certainly not in sufficient quantity for the event of a great war. Moreover the making of contracts for their sudden supply would be useless, as in the occurrence of a mobilization of the army, at any rate in Germany, the greater number of the work-people in india-rubber factories would be drawn away for military service, so that, in the time of greatest need, the manufacturers would be unable to fulfil their contracts.

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27.—For these reasons I have had a necessary article of dress—a man's trouser-braces—so made that it can be employed for elastic compression in traumatic bleedings.

28.—These "tourniquet braces" are composed of a band of india-rubber, 160 cents. long, strong enough to serve to compress securely every vessel in every part of a limb.

29.—As every soldier requires to wear braces, and as these tourniquet braces are not more expensive than others, the desire that every soldier should in war time carry about with him the means of stopping bleeding can now be realized.

30.—Should this be carried out, every soldier would possess the means of arresting dangerous bleeding in his own case and that of others. If required for himself, he can spare his own braces, and on the field of battle the proposed elastic braces of the dead and wounded would be available in a larger quantity.

31.—Another circumstance not to be lightly valued, is that these elastic bands or braces can be applied to several other uses; to surgical blood emptying at operations, to arresting absorption in poisoned wounds (for instance snake-bites, poisoned arrows, &c.), and to the re-animation of those apparently dead through loss of blood.

DISCUSSION.

Surgeon-Major STARCKE, Berlin: I made different experiments with the elastic bandage, and I came to the conclusion that if Esmarch's bandage is the best apparatus for arresting bleeding, it is by the compression itself, not only by elastic, but also by any other linen or cotton bandages. I am convinced that the general use of india-rubber, in the form of braces, or otherwise, is not necessary, and fear that the sweat, or the extreme movements of soldiers during drill and exercise will often destroy the elasticity of india-rubber braces. In the beginning of a war it is not possible to inspect many thousands of soldiers to see if they have the braces in useful and safe condition. So we must lay the principal stress on the knowledge of arresting the blood not by tourniquets, but by the strict compression of a surrounding bandage in the centripetal direction. If all men of Army Hospital Corps are properly instructed they will bring sure help and succour to bleeding wounds. All surgeons are of opinion that gunshot wounds do not often bleed in any large degree. I believe that official distribution of braces is not of much moment, but in the stores of army field hospitals the elastic Esmarch bandages should take the first place.

Professor ESMARCH, Kiel: It is important that the braces are made of good india-rubber. Bad india-rubber soon deteriorates in its elasticity, but good elastic braces may be worn for years without losing their resilience, and the evaporation of the human body seems more calculated to preserve than to destroy the india-rubber.

Professor MACLEAN, Surgeon-General MOUAT, Dr. BILLINGS, U.S.A., Surgeon-General HUNTER, Surgeon-General FAYRER, expressed their decided conviction that instruments and appliances in which india-rubber forms an essential part cannot be preserved in an efficient state for any considerable time in tropical climates, and various substitutes for this material were referred to.

Surgeon-Major WHITE, Professor SIBTHORPE, and Surgeon MYERS, speaking from personal experience, had not found india-rubber appliances deteriorate so quickly in tropical climates as had been described, and considered that with improved methods of manufacture they might be rendered still more durable. This view was supported by Professor PARTRIDGE and Surgeon TSCHUDI.

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The PRESIDENT summarized the discussion in some concluding remarks, observing that it would probably repay makers of india-rubber articles if they would try to discover some sure method of rendering their quality of elasticity permanent in hot climates.

Transport of Sick and Wounded in Uncivilized Countries, bearing especially on the late Kaffir Campaigns.

By Surgeon R. VACY ASH, A.M.D.

The Zulu, Transvaal, Afghan, and more recently the Tunis war have brought the subject of transport once more prominently to the front. That it is a puzzle even in civilized warfare can be testified to by many; but in half-civilized countries the sparse population is hostile to a degree, and ready not only to impede one's efforts, but to massacre on the slightest provocation. Such were the conditions with which the Zulu war was fought. Picture to yourselves an undulating landscape, with ranges of hills intersecting it, and projecting spurs breaking up the country in a chaotic mass, the whole devoid of foliage except in the secluded ravines where the sugar bush and tree-fern thrive, and you have Natal and Transvaal. Over this kind of country, five and twenty years ago, the old Dutch voretreker (a species of wandering shepherd) drove his lumbering waggon and long span of oxen. Tracks left by these adventurers were soon followed by others, and so on, till a path or road was formed. Up and down hill, as straight as the precipitous mountain spurs would allow, the sturdy oxen wended their way. These paths are the roads of the present day, little altered or improved; a few heaps of stones here and there, where a deep rut or a deep mud pit had nearly broken the heart of many a driver, is all that can be said to have been done in road making. This greatly added to our difficulties in the transport of sick and wounded. The Traansvaal, barring its mineral aspects, is essentially a grazing country, mile after mile of veldt or natural grass-covered land is met with, and few houses are to be seen as the high road is traversed. Six thousand acres is an average farm, and many are double and treble that size. This accounts for the scattered population, and, combined with the love of seclusion and isolation so common to the Boers, accounts for the few houses or inhabited villages in the colony. The absence of railways and, what is ten times worse, navigable rivers, is an almost insurmountable obstacle to transport in times of peace, and doubly felt, of course, in time of war, when the people are unwilling to assist, even for the dearly loved coin of the realm. The heavy ox-waggon, drawn by fourteen or sixteen oxen, usually at the rate of 2½ miles an hour, is the only means of transport at the disposal of the authorities; and this, from its primitive construction, is ill adapted to the requirements of the sick, being shaky and wearisome even to the healthy.

The Boer is a rough, uncouth white, of dirty habits and tastes. A travelling schoolmaster remaining three months at a farm completes his education, if it can be so called. To spell out a few sentences, a rudimentary knowledge of arithmetic, and to write his name is all that is deemed necessary by the aristocrats; the commoners do without even this. They intermarry among themselves. It is no uncommon thing to see four or five families living in a small community of their own. During the summer months they migrate to the Bush lands, taking their homes with them, and living in tents; hence arose the habit of sleeping in their clothes, men and women alike, which habit is common to them at the present day. Their ignorance of the world, together with their isolated life, makes them proud, domineering, and cruel to their inferiors; un-

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willing to brook the restraint of civilized laws, or interference with their so-called liberty, it will not surprise anyone, therefore, that opposition on every hand was found as we traversed the Transvaal. Some families thought it a sin, or expected some supernatural manifestation, if they helped the cursed Englishman, even to assist in the transport of the sick. Many would not open their doors to us as we drew near to purchase milk or other necessaries at a farm where plenty abounded. Tobacco they had in abundance, yet I have known them charge a poor soldier half-a-crown for what was not worth a penny in the market; and so on throughout the category of necessaries. It was this antagonism which rendered our task so difficult. But the sick had to be brought down; and, thanks to the Commissariat and Transport Department, were brought down in spite of all, each convoy being independent of incidental supplies from the district, from starting to end of perhaps a four hundred mile journey. The Kaffirs—always our friends—could do little to help us. Waggons they had few or none, but labour was plentiful. A corps of bearers was formed, and did some good service on one battle-field, Ulundi, in the removal of wounded, but they could seldom be relied upon, frequently running away at the sight of the enemy, and sometimes without this stimulus. I think I have shown, from the description of the country and people, the unsuitableness of the cumbersome ox-waggon for the transport of sick. Transport by hand was equally objectionable, nay, even impossible, except for a limited distance. The roads precluded the possibility of utilizing the numberless appliances suggested and described by Professor Longmore and Dr. Otis, and for reasons which I will hereafter show. Next I come to the horse of the country. The horse used by the Boer or Zulu is a small undersized animal of wiry constitution and indomitable pluck, capable of doing his sixty or eighty miles a day. Stunted in growth and falling away in his hind quarters, he is unsuited to the transport of sick on his back and for draught; the ox, although slower, is much preferable. The mule is by far the most profitable means of draught, combining the endurance of the ox with the hardiness of the donkey and the pace of the horse, and is, to my mind, best adapted to the requirements of the sick and wounded.

Professor Longmore has struck the key-note when he says that it is the inability to secure trained animals which renders all modern appliances inapplicable in uncivilized, or half-civilized, warfare. You may send out your perfectly trained animals, who, at a field day in Aldershot valley would work almost as if possessed of human intellect, and horse sickness, that curse of South Africa, on the one hand, and want of suitable food, on the other, will render you powerless by the loss of your highly prized cattle before your campaign has begun. It is without hesitation, therefore, that I say any new method for the transport of sick and wounded, which depends upon trained animals, is unsuitable in half-civilized warfare. Unfortunately the food of the ox is not always suitable. On the high lands the sweet grass differs from the sour grass of the low lands and along the coast line, and oxen accustomed to one will not thrive on the other. This was found a difficulty in the late campaign. Almost every kind of animal in South Africa has some fatal disease peculiar to itself. The horse and mule are subject to horse-sickness, a rapidly fatal disease, supposed by some veterinarians to be a species of anthrax; but my own post-mortem observations lead me to think it is a blood-poisoning, attended by acute inflammation of the whole mucous and respiratory tracts. This dreadful disease is one of the most fatal obstacles in the Transvaal to transport by horse or mule during the summer months. The ox also is subject to an equally fatal disease, pleuro-pneumonia killing them off by thousands.

I have dwelt on the habits and peculiarities of these half-civilized people and on the description of their cattle to bring home to all the difficulties which we had to

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encounter, which may occur again at any moment, and, in fact, are far more likely to occur again than the civilized warfare we are all trained to take a part in. Bearing the half-civilized nature of the country in mind, let us now consider the kind of transport required, the few rough and ready materials which were at hand, and then draw a few conclusions as to what would be best should any of us be placed in a similar position.

First, then, let us consider the removal of the sick from the battle-field, for removed they must be to escape mutilation, or what would appear to us mutilation, but, with justice to the Zulu, is not meant as such by them. From the battle of Sekukuni's Kop, which was fought round a hill, the sides of which were charged over and over again by our men, to the battle of Ulundi, fought on a plain, every conceivable kind of undulating ground, sometimes intersected by nullahs (as at Isandula), was the scene of conflict.

In all battles, except of course in self-defence, as at Kambula, certain columns marched from their temporary halting place, generally at early dawn, with a given object to attain. Nothing but ammunition accompanied the troops, and the medical staff took only the ordinary but valuable stretcher. Numbers of these were carried by the bearer columns, when such were present, either native or A.H.C., or in their absence by orderlies told off for the purpose. As soon as a wounded man was found, he was attended to, removed out of the line of fire, and possibly taken to the temporary camp from which the column emerged in the morning. No improvement or suggestion is necessary for this part of the transport of wounded, except that there be plenty of these useful contrivances.

A much more serious problem is the conveyance of sick or wounded on patrol duty or rapid marches in light order. Frequently a three days' patrol either on reconnaissance, or with some set purpose to destroy kraals, &c., was organized. The men took nothing but a waterproof sheet and a blanket on their horses, with 70 rounds of ammunition, and, of course, their three days' cooked food on themselves.

No conveyance of any kind was allowed, and, in fact, would have been frequently impracticable, for occasionally over hill and dale, as the crow flies, without road or path, was the only means of attaining the desired object for which the patrol was organized. And, again, a sudden call for a rapid movement when on this duty would, had a conveyance been with them, have necessitated its abandonment and probable loss.

If a man, therefore, fell sick, or was wounded, there was no help for it; he had to be conveyed on his own horse, or on that of a friend, till he could be safely dropped at some friendly outpost or camp on the road. A doctor always accompanied these patrols, but his services were seldom required, and when they were, his resources were so limited that he could do little to help a sufferer. I have nothing to suggest for this problem. I see no help for it, but in such cases to make the best of it, and convey the sick or wounded man with the patrol, as was then done, till help is met with in a camp or post. I need scarcely say it would have been waste of time, and worse than useless, to have asked for his admission during the Zulu war to a kraal, or to a farm during the Transvaal war; it would have simply been to court ill-treatment as soon as your back was turned.

The next kind of transport required would be for flying columns, such as the one that marched to Ekowe to the relief of Colonel Pearson, or to the attacks on Sekukuni. In both these instances provision had to be made not only for the sick on the line of march, but for the return of the wounded from the battle-field. At first we could only get the cumbersome ox-waggon, previously described; but eventually we secured as complete a contrivance as could have been desired, the American waggonette—a species of covered waggon built on the well-known "spider" system. Its advantages were soon apparent. First of all, its lightness enabled

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four or six mules, or as many oxen, to run away with it. Secondly, its elasticity caused it to yield to large obstacles such as ant heaps and huge boulders; and, lastly, its airy springs made it a most comfortable conveyance for the sick. Each would accommodate six or eight sitting, or two recumbent and four sitting, the light hood protecting them from the rays of the sun, and side flaps having a similar object in view on each side. By their means the sick and wounded could be conveyed for scores of miles either in company with a column, or a convoy; at night, the sick occupied a bell tent, which could be carried strapped under the waggonette, together with the cooking utensils or on an ox or separate waggon. The same transport was found most suitable when the sick or wounded had to be removed from the front station hospitals to the nearest base hospital, such as to Pretoria from the Sekukuni wars; and to Newcastle in the north, and Tugela in the south, from the Zulu wars.

Lastly, we have to consider the transport of the sick from the hospitals in front to the port of embarkation. Bearing in mind the nature of the country to be traversed and the means at disposal, I think the safest and surest means is by ox-waggon. For be it remembered now a friendly district was being traversed, help was always at hand, and supplies procurable, and fuel in the form of dried manure, making a lovely fire, was in abundance, and, in addition, the sick and wounded were so far convalescent, as a rule, to be able to help themselves. The waggon generally used was the ox waggon of the country converted by local labour into an ambulance waggon. On the bed of the waggon was placed a platform on four or six carriage springs. In the rear of this were fixed two swing cots for wounded in the recumbent position, and in front were three or four seats with leather strap backs running transversely across the platform. The whole would accommodate two lying and some ten or twelve sitting, besides ample room between the spring platform and the true bed of the waggon for tent poles, stretchers, &c., and by the side and underneath could be strapped on plenty of cooking utensils. Over the whole was stretched a canvas hood running the full length of the waggon, and outside, attached to the top, and also running along on each side, was a long flap which, when unrolled, could be pegged down at an angle say, of 45°, forming a good shelter for the convalescent when halting for the night.

Such were the contrivances used in the wars in South Africa for the conveyance of sick to the ports of embarkation, traversing in some cases as much as four hundred miles (from Pretoria to Durban), arriving without accident and the convoy in better health than when starting.

In conclusion I would point out the almost total absence of wood prevented the adoption of impromptu litters as suggested by the late Dr. Otis of the U.S. Army; and the impossibility of getting trained animals barred the use of the admirable contrivances suggested by Professor Longmore. As I have before said, had such trained animals been sent out from home, horse sickness on the one hand and mealie food on the other would have rendered the transport department powerless before they had begun the campaign, and as they could not have been replaced, the last state of that department would have been worse than the first.

Comment devons-nous Transporter nos Blessés en Campagne?

Dr. Da Cunha Bellem, Lisbon.

Messieurs et très-honorables collègues,—La guerre! Malgré tous les efforts de la diplomatie, elle est, et elle sera pour longtemps encore la dernière ressource des nations, lorsqu'elles veulent démêler leurs querelles!

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La philosophie aura beau la blâmer !

La philosophie ne pourra entraver l'élan des armées, et elle n'aura dans son impuissance qu'à en pleurer les épouvantables résultats.

La religion la condamnera de toutes ses forces, et cependant ce ne seront que des vœux agréables au ciel, mais que les hommes, le cœur inflammé par les passions, ne sauront entendre.

La douce fraternité cherchera en vain à déployer partout ses ailes protectrices, pour en abriter tout le genre humain. Les armées, nonobstant, ne manqueront pas d'épuiser les trésors des nations, d'arracher aux travaux paisibles de l'agriculture, du commerce, de l'industrie, des arts et des sciences, les meilleurs enfants de la nation pour les envoyer à la mort, aussitôt que le feu des sentiments haineux éclatera contre les voisins, les amis de la veille, les compagnons et associés de la sainte croisade de la civilisation et du progrès.

Le droit de la force, ce droit dont l'ancienneté vient du moment où deux hommes ont cöexisté à la surface de la terre, sera le dernier mot des plaidoyers entre les nations ; et le fer, ce précieux métal des bêches et des charrues qui fouillent la terre, et lui procurent l'abondance des moissons, ce précieux métal des outils et des machines, qui font la richesse des industries, le fer deviendra l'épée au tranchant impitoyable, le fusil qui vomit la mort, le canon grondant à l'égal du tonnerre.

Rien ne reste aux hommes de bonne volonté, à ceux qui ne se laissent plus éblouir par de vaines théories, que de chercher à amoindrir les malheurs de la guerre, en lui supprimant son caractère farouche, en lui modifiant le penchant des cruautés inouïes qui ont fait l'épouvante et le déshonneur des générations passées.

Le soldat qui tombe n'est plus un ennemi, il n'est qu'un frère souffrant, auquel il faut venir en aide quelque soit le camp auquel il soit tombé. Les médecins militaires, ainsi que les infirmiers et les brancardiers, n'ont plus de nationalité après le carnage. Les vainqueurs et les vaincus se donnent les mains pour s'entr'aider au service de panser les blessés, de soulager les douleurs des mourants, d'entreprendre un glorieux combat contre la mort aux froides mains ! Tel est l'éclatant triomphe de la philanthropie, voilà la victoire splendide, dont le meilleur partage en honneurs revient à la médecine militaire. La convention de Genève, ce glorieux événement, prévu par le cœur généreux du baron de Percy, n'est que la consécration internationale des sentiments philanthropiques des médecins militaires du monde entier. Depuis lors, on a vu se développer partout l'intérêt et le dévouement pour les malheureux qui ont été atteints par le fer ou par le plomb meurtrier de n'importe quels rangs ennemis. Partout on cherche à leur porter des soulagements, à leur épargner des douleurs, à leur procurer des secours, à leur fournir des moyens de transport aussi commodes que possible. La bête farouche que l'on appelle la guerre est à demi apprivoisée.

Mais il n'en fut pas toujours ainsi pour ce qui concerne ce généreux empressement de secourir et de transporter les blessés.

La France seule s'occupait au dix-huitième siècle de l'organisation des moyens de transport pour les blessés militaires ; et l'ordonnance du 20 juin 1788 fut la première voix de ralliement à l'égard de l'adoption des voitures spéciales pour le service des ambulances.

Jusqu'à cette époque, en France, ainsi qu'en Angleterre selon l'opinion autorisée de M. le professeur Longmore, les blessés militaires étaient chargés sur les chariots des paysans que les commandants trouvaient sur leur route, ou qu'ils faisaient venir des villages les plus rapprochés du théâtre de la guerre.

Le premier transport roué mis en usage à l'armée française et qui a devancé celui du baron Larrey, c'était une caisse semblable à celle des fourgons, convenablement suspendue, et destinée à transporter trois blessés couchés et superposés.

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Mais cette voiture était tellement pesante qu'elle ne pouvait se mouvoir parmi les rangs des combattants; et, ce qui est incroyable, les brancards, le moyen le plus simple, le plus intuitif de transporter les blessés, faisaient tout-à-fait défaut, de sorte que les pauvres soldats souffraient horriblement, lorsqu'ils étaient conduits jusqu'à la voiture, sur des couvertures de lit, sur des planches de bois, sur des échelles, sur des portes, sur des fusils même, par leurs camarades, tantôt par un sentiment de condoléance, et tantôt par l'égoïsme de se soustraire eux-mêmes au danger auquel ils étaient exposés sur la ligne de feu.

Le souffle ardent de la Révolution française, de cet événement qui a bouleversé la société toute entière, donna naissance à deux glorieux innovateurs en fait de service d'ambulance. Parmi les flots de sang répandu, entre la fumée des canons et des fusils des soldats victorieux de la république, les noms de deux illustres chirurgiens militaires, les barons Larrey et Percy, ont brillé d'un vif éclat.

Leur apparition sur le champ de bataille marque une nouvelle époque à la chirurgie militaire; au services des ambulances surtout, et si, comme M. le professeur Longmore le fait remarquer dans son ouvrage "Treatise on Ambulances," les circonstances ont contribué à la gloire des deux célèbres innovateurs, leur énergie personnelle, leur talent supérieur, leur coup-d'œil prévoyant, contribuèrent bien mieux encore à la réussite de leurs entreprises si hardies, à la grandiose renommée qu'ils ont acquise.

A l'armée du Rhin, sous le commandement du général Custine, le baron Larrey conçoit l'idée de son ambulance volante, qu'il perfectionne quelques années plus tard; tandis que sous les ordres de Moreau, à l'armée du nord, le baron Percy parvient à réaliser un système d'ambulance tout-à-fait différent.

Celui-ci cherche à porter les secours partout, celui-là s'occupe du moyen de conduire les blessés jusqu'au point où l'on peut donner les secours, l'un de ces systèmes complètant l'autre, ou les deux constituant le système complet de porter les secours aux blessés et de porter les blessés à l'endroit des secours. Les infirmiers et les brancardiers militaires attachés à ce service reçoivent une organisation régulière. Le brancard portatif et réglementaire est adopté. Le décret du 9 frimaire de l'an XII., donne à chaque régiment un fourgon de chirurgie et de pansement.

Pendant les guerres de Russie et d'Espagne, on adopte les cantines d'ambulance. Le baron Larrey, en Egypte, conçoit l'idée des cacolets pour le transport des blessés.

Toute l'initiative était française; les autres pays, l'Angleterre même, malgré son progrès intellectuel, restaient en arrière en fait d'organisation du service d'ambulances.

Avant la guerre de la Péninsule, selon l'autorité de M. le professeur Longmore, il n'existait pas de transports spéciaux pour les blessés des armées britanniques; et même dans les guerres soutenues à cette époque sur le continent européen, les autorités militaires mettaient à profit les chariots du pays où les Anglais faisaient la guerre, pour transporter leurs blessés aux hôpitaux, ou aux ports d'embarquement.

Le chirurgien-en-chef Sir James M'Grigor tâcha en vain de créer un service d'ambulance analogue à celui des armées françaises. Ses efforts échouèrent. A la fin de la guerre de la Péninsule les blessés anglais étaient encore transportés sur les voitures du corps nommé Royal Waggon Train; mais les imperfections du système ne tardèrent pas d'être reconnues, et les voitures montées sur des ressorts furent exclusivement destinées au transport des blessés. Le corps d'infirmiers militaires a reçu une nouvelle organisation, quoique très-éloignée encore de celle du Army Hospital Corps ou de celle du personnel du corps des brancardiers français.

Les brancards adoptés dans l'armée anglaise étaient on ne peut plus défectueux d'après ce qu'en rapporte Millingen dans son ouvrage "Army Surgeon's Manual;" et ce chirurgien fut le premier qui présenta un modèle de brancard qui porte son nom et qui était une heureuse modification de celui de Percy.

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Ce brancard fut adopté à l'armée portugaise, où il devint réglementaire jusqu'en 1874.

Les "Notes and Recollections of a Professional Life" de l'inspecteur-général William Fergusson; les "Rough Notes by an Old Soldier" du major-général Bell; les "Observations on the Defective State of Army Transport, with Suggestions for its Improvement," de Cherry, sont des documents authentiques de l'imperfection du système des moyens de transport pour les blessés dans l'armée anglaise au commencement de ce siècle; et ce ne fut qu'en 1835 qu'on essaya sur le champ de bataille les avantages de la voiture proposée par Cherry, lorsqu'une légion anglaise est venue en Espagne sous le commandement de Sir de Lacy Evans, d'après ce que nous rapporte Sir Rutherford Alcock dans ses "Notes on the Medical History and Statistics of the British Legion in Spain."

Vient ensuite une période où tout mouvement de perfectionnement du matériel d'ambulance s'est partout arrêté. La France, le premier pays qui avait adopté les voitures pour le transport des blessés, les avait laissées tomber dans l'oubli juste au moment où les autres pays les adoptaient. L'Angleterre ne faisait qu'approprier les *kujawahs* au transport de ses blessés dans les guerres des Indes. Toutes les autres nations de l'Europe ne se préoccupaient guère de la guerre, et on parvint à croire que le temple de Janus aurait ses portes à tout jamais fermées dans les pays civilisés.

Nous ne connaissons que la Suisse, en 1839, et la Suède, en 1845, qui aient organisé leur matériel de transport des blessés militaires, en adoptant des modèles très-remarquables.

Mais la guerre de Crimée fut le cri d'alarme dans cette paisible insouciance de l'Europe entière. On s'aperçut alors, de ce qu'on n'avait pas songé au transport des blessés, et l'on s'empressa partout de regagner le temps perdu. L'Angleterre en donna l'exemple. Les modèles de voitures spéciales sont devenus innombrables depuis lors, et chaque inventeur s'imagine avoir atteint le but et avoir dépassé ses devanciers. Les modèles de brancards varient aussi à l'infini. On cherche partout à approprier les nouveaux moyens de transport au service des blessés, d'après les moyens de transport en usage dans chaque pays. Depuis le hammock et le dhooley jusqu'au vélocipède prôné par M. le professeur Neudörfer, tout s'est mis à profit.

Le brancard à roues, dont l'origine remonte à la bataille de Bautzen, reparaît pendant la guerre de Crimée; il est d'abord accueilli avec un grand enthousiasme, mais bientôt on s'aperçoit qu'il n'atteint pas non plus son but, malgré tous les modèles présentés. Le tablier de Landa, la chaise de Locati échouent tout de même.

Dans les voies ferrées on invente des wagons, des moyens de suspension, des brancards spéciaux.

Mais enfin on reconnaît que tout est inutile, que tout est insuffisant, que rien ne peut satisfaire aux besoins de la guerre moderne pour le transport des blessés du champ de bataille aux places de secours, des places de secours aux hôpitaux, des hôpitaux siègeant près le champ de bataille à ceux qui se disséminent à l'intérieur du pays.

On revient sur ses pas. L'appropriation des moyens de transport ordinaires vient substituer tout cet empressement d'inventer des modèles spéciaux. On tâche de suppléer à l'insuffisance des voitures qui encombraient les derrières des armées en inventant des moyens simples et faciles d'appropriation des chariots des paysans. M. le professeur Léon Le Fort invente son crochet à ressort élastique; M. le docteur Wywodzoff invente son étau. Les inventions se multiplient aussi pour l'appropriation des voitures ou des fourgons de chemin de fer.

Mais le médecin allemand au service des ambulances de la Roumanie invente encore quelque chose de bien mieux. Il invente la paille et le foin comme le meilleur des coussins pour transporter les blessés, pour donner de l'élasticité à n'importe quels moyens

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de transport roulants, pour assoupir la trépidation et les secousses des voitures. Il reconnaît dans le char pesant trainé par des bœufs une précieuse ressource pour conduire les blessés, quand même ces chars seraient, comme l'*araba* de la Dobrutscha, montés sur des roues octogones!

La paille et le foin comptent encore d'autres partisans; et tandis qu'on blâmait les trains luxurieux construits en Russie pour la dotation du service sanitaire sur les voies ferrées, on faisait l'éloge des moyens primitifs dont on profitait pour transporter les blessés de Plevna aux ambulances de Turnu-Magurelli, sur la rive septentrionale du Danube! A quoi bon avoir dépensé tant de fantaisie pour créer des modèles spéciaux de transports roulants? La paille, voilà le grand ressort! le foin, voilà le meilleur des coussins!

Nous voilà au point de départ, après avoir fait de longs détours dans les régions de l'éblouissante théorie!

Le char à bœufs et la paille! Nous sommes en arrière des barons Larrey et Percy.

L'histoire des moyens de transport, c'est comme l'histoire de l'humanité. Après avoir parcouru le cercle formé par des aspirations irréalisables, elle arrive aussi, toute haletante et toute découragée, au point de départ pour recommencer encore une fois à courir après son idéal insaisissable.

Cependant, si les constructions spéciales, très-coûteuses et très-difficiles à mettre en route sur les derrières d'une armée, ne sont qu'une théorie séduisante; si elles représentent des difficultés insurmontables; si l'appropriation des moyens de transport ordinaires est ce qu'il y a de plus pratique et de plus facile; si elle représente, dans sa simplicité même, un véritable progrès, il ne faut pas aller à nier toute prévoyance, et à se croire tout-à-fait pourvu du nécessaire, lorsqu'on s'est faite une bonne provision de paille ou de foin, lorsqu'on compte sur nombre de chariots de paysans trainés par n'importe quelle espèce d'animaux. Et, s'il n'y a point de règles générales à établir pour ce qui concerne le système d'appropriations à adopter, ce système devant être subordonné aux moyens de transport en usage dans la contrée où l'on fait la guerre, il faut néanmoins avouer que le crochet à ressort de M. le professeur Léon Le Fort a des avantages très-sérieux et bien connus en plusieurs cas, et que l'étau inventé par M. le docteur Wywodzoff semble destiné à remplir très-raisonnablement le but d'une appropriation facile en bien des circonstances.

L'approvisionnement de ces deux ressources, en nombre suffisant pour les besoins de la guerre, nous semble donc une mesure d'une grande portée, lorsqu'on veut être préparé pour toutes éventualités.

D'après le principe des appropriations, il ne faut pas se préoccuper trop d'avance des moyens de transport à obtenir dans les pays civilisés; la connaissance parfaite des ressources de la contrée où l'on porte la guerre, et l'emmagasinement préalable des moyens les plus convenables à obtenir l'appropriation, suffisant en règle.

Mais, lorsqu'on va porter la guerre dans des pays non civilisés ou à demi-civilisés, il faut que la prévoyance la plus rigoureuse devance de beaucoup le commencement des opérations militaires, car on ne peut s'y confier qu'à ses propres ressources, car on ne peut rien attendre du pays même. Mais cette prévoyance jusqu'à quel point peut-elle aller? Faudrait-il, lorsqu'on va faire la guerre chez les peuples non civilisés ou à demi-civilisés, y porter de pesants équipages, encombrant les derrières des armées, difficultant leurs mouvements, surtout si l'on est forcé de faire une retraite? Peut-on s'attendre à y trouver des routes frayées, des chemins pratiquables pour le roulement des voitures? Doit-on, par contre, s'abstenir tout-à-fait des transports roulants, et ne confier qu'aux efforts des brancardiers le soin de transporter les blessés du champ de bataille aux hôpitaux, des hôpitaux aux ports d'embarquement? Voilà la question. Pour nous,

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nous croyons bien que le juste milieu est le seul expédient acceptable en de telles circonstances. Certes, on ne doit pas proscrire absolument les transports roulants, surtout s'il s'agit d'un pays à demi civilisé, où il y aura, tant peu que ce soit, des routes, mais on doit déposer une plus grande confiance au transport à bras. C'est-à-dire, le brancard doit être la principale ressource sur laquelle une armée doit compter, lorsqu'elle va faire la guerre en des pays où la lumière de la civilisation n'a pas encore pénétré. Mais un modèle simple et transportable est à créer pour la dotation des armées européennes qui font la guerre dans les contrées lointaines, non civilisées ou à demi civilisées; tous les brancards réglementaires dont nous avons connaissance étant trop lourds, très-compliqués, fort difficiles à transporter, lorsqu'il s'agit de la guerre en des pays sauvages, lorsqu'on ne peut les faire transporter sur des chars ou sur des fourgons du service sanitaire, lorsque les brancardiers sont forcés de les porter eux-mêmes à travers des sentiers hérissés de scabrosités, à travers des bruyères où le pied de l'homme n'a jamais laissé son empreinte.

Néanmoins, comme à la guerre il faut tout mettre à profit, ce brancard doit répondre aux indications d'être utilisé pour le transport des blessés à bras, par les brancardiers réguliers, ou porté en suspension sur l'épaule par les gens du pays, sans laisser d'être accommodable aux moyens de transport roulant, que par hasard on trouverait en usage au pays, qu'ils soient trainés par des chevaux, par des bœufs ou par n'importe quels autres animaux.

De cette façon le brancard sera toujours profitable, et l'on n'aura qu'à s'approvisionner des moyens les plus simples pour l'appropriation, s'il y a lieu.

Il ne faut pas cependant mépriser tout-à-fait l'adoption de chariots pour le service sanitaire des armées faisant la guerre en des pays non civilisés ou à demi civilisés, surtout si l'on a le droit de s'attendre à ce qu'il y existe quelque route pratiquable où l'on puisse faire rouler ces moyens de transport. Leur modèle doit être le plus simple et le plus rudimentaire. Un plancher monté sur un essieu à ressort, et deux roues assez grandes et assez solides, le tout pouvant se démonter avec une extrême facilité et se monter de même. Sur le plancher, des tiges dressées, que l'on puisse y fixer assez solidement, pour y faire la suspension de deux ou de trois brancards; une couverture de toile protégeant les blessés transportés. Toutes ces pièces des chariots doivent s'accommoder très-facilement, de telle façon que leur emmagasinage dans les navires destinés à conduire les troupes au théâtre de la guerre soit on ne peut plus facile. Cependant, pour mettre à profit toutes les ressources qu'on peut se procurer en de pareilles circonstances, il faut que tous les chariots d'équipages ou de munitions que l'on aura à mettre à la suite de l'armée soient construits en sorte qu'ils puissent servir au transport des blessés.

Malgré l'enthousiasme de notre honorable ami et très-savant confrère M. le baron Larrey pour les moyens de transport des blessés sur le dos des bêtes, nous nous obstinons à croire que ce n'est qu'une mauvaise ressource que celle-là, et que ces moyens de transport ne doivent être admis que faute de mieux. Entre le cacolet et le char trainé par des bœufs, quand même il ne soit suspendu ni pourvu de ressorts, quand même il faudrait y mettre de la paille ou du foin pour adoucir les secousses, nous donnons la préférence à celui-ci; et dans la guerre des montagnes, entre le cacolet et le brancard porté par deux brancardiers, nous croyons que les avantages sont pour le brancard.

Nous savons bien que c'est beaucoup que d'occuper deux hommes valides pour transporter chaque blessé; mais il ne peut pas en être autrement, un homme seul ne suffisant point pour la conduction d'un blessé, pas même dans les brancards-à-roues.

Moyennant les cacolets et les litières, un homme et un animal conduisent deux blessés.

C'est vrai! Mais d'abord rien de plus difficile que d'obtenir des animaux assez

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vigoureux pour supporter la pesanteur de deux blessés; ensuite, si l'animal vient à tomber, ou rien qu'à broncher ou à faire un faux-pas, ce qui est d'autant plus facile que les routes frayées dans les montagnes sont très-dangereuses à parcourir, voilà que les deux blessés tomberont par terre; et s'ils ne meurent pas de la chute, ils auront besoin de quatre hommes au moins pour les remettre à leur place sur le dos de l'animal.

Le cacolet donc ne saurait être admissible qu'à deux conditions impossibles de remplir: la vigueur hors ligne de l'animal, et l'assurance de ce qu'il ne viendrait jamais à tomber ou à broncher!

Pour ce qui concerne les constructions de luxe, les voitures spéciales surtout, on ne peut songer à en profiter pour la guerre chez les peuples non civilisés ou à demi civilisés.

Leur adoption ne peut qu'être limitée au service de la guerre faite au pays même ou dans les contrées des pays voisins, occupées militairement et avec des communications assurées.

Messieurs, avant de finir, permettez-nous de former des vœux pour que la guerre, ce fléau terrible, ne puisse jamais troubler la vie paisible du laboureur, de l'artiste ou de l'ouvrier; que les sentiments de la philanthropie triomphent partout, et que le service sanitaire sur le champ de bataille puisse devenir inutile au long jamais!

Medical and Surgical Notes on South African Warfare.

Fleet Surgeon Henry F. Norbury, C.B., R.N.

In the present paper I propose to confine myself strictly and briefly to a few simple practical items which have occurred to me during a somewhat extensive experience in South African warfare.

In the campaigns of South Africa, the transport is conducted by means of waggons drawn by a number of oxen, but, owing to the rugged and muddy "drifts" or fords which have to be crossed, and from which the waggons have frequently to be extricated with much difficulty, they must necessarily be laden lightly; consequently everything that can be done to lessen weight and economize space becomes of special importance. One of the most undesirable articles of military field equipment is the ordinary board and trestle bedstead; it is heavy, cumbersome, and awkward in the extreme, and I would strongly recommend as a substitute for these Pocock's tubular air beds, which are light, very portable, durable, waterproof, and comfortable to the patient; they are also strong, and might easily be made specially so for campaigning. I do not think that half-a-dozen of them would weigh so much as one board and trestle bedstead.

Again, in the hurry of marches, especially with flying columns, the compounding of medicines is both difficult and uncertain; nearly every drug necessary might be issued already dispensed, in the form of pilules or pills, and what are known as lamels, which occupy so small a space that they can be carried in practically inexhaustible quantities, and thus the transport relieved of the conveyance of heavy medicine chests.

Every surgeon will recognize the desirability, both for surgical and sanitary reasons, of having the beds of his patients raised to a comfortable height above the ground. In camps of some permanency, where there are always plenty of empty commissariat packing cases available, I found that by having six stakes driven firmly into the ground, and nailing on these two cases sawn across parallel to the cover, so as to leave the sides only some seven inches high, and then removing the two ends, a capital bedstead was formed without any trouble, the bottom of this when filled with dry grass, and covered with a

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sheet, really left nothing to be desired; these were peculiarly adapted for fracture beds, and a hospital marquee can be fitted with such bedsteads in a very short space of time.

The dried strips of raw hide known as "reims" may be utilized in many ways. Should it be necessary to irrigate a part for a severe contusion, wound, or other injury—a leg, for instance—a piece of the bottom of the lower box can be removed and a few reims fastened across; then, by suspending a soldier's canteen from a stick above the limb, filled with the irrigating fluid, it drips from the limb at once on the ground where a hole can be dug to receive it. A similar contrivance higher up in the bed renders a bed-pan unnecessary; in cases attended with diarrhœa any receptacle can be placed on the ground beneath, and kept earthed as occasion requires.

Again, in cases of serious fracture and wherever it is imperative that the transport of a patient should be conducted without jolting, reims can be stretched across the summit of the sides of a waggon, and then interlaced. This arrangement almost entirely prevents the motion of the waggon being felt, and is, in my opinion, superior to a vehicle with springs; the mattrasses of the patients can be placed on this, and the journey is one of comparative comfort. When we evacuated Fort Ekowe there was a large number of sick, and there was a "trek" of some forty miles to the Lower Tugela, which occupied three days, and I had the most serious cases placed on these constructions with great advantage.

A large proportion of the waggons in South Africa are provided with arched coverings known as "tilts," the frames of which are generally very strong, and can be easily rendered additionally so by placing a rough stanchion in the centre by way of support; this admits of a limited number of cots or other contrivances being slung from the roofs. The waggons can generally carry two cots each, and nothing can be more simple or complete than this method of transport, because it admits, further, of the bottom of the the waggon being laden.

I would suggest that the floors of ambulances should be placed as low as possible, so as to prevent their becoming top-heavy when full, for from the high construction of a military ambulance I once had a load of recently wounded men overturned at a sloping spot on the road, which upwards of a hundred heavily laden waggons passed without any mishap. In addition to the springs which are placed upon the axles, springs might also be fastened beneath the stretchers with which ambulances are furnished, and much greater freedom from both oscillation and vibration would accrue.

Besides the above devices for affording rest during transport to an injured part, such as a serious fracture of either trunk or extremity, the part may advantageously be enveloped in a firm casing, partial or complete, of some substance moulded to it. The carriage of large supplies of chalk or plaster of Paris for this purpose is cumbrous and unnecessary, as what answers equally well and forms quite a firm support, is the cement used by the Kaffirs for forming the floors of their huts, and which can be procured anywhere; it is termed by the natives "Udaka," and consists of the earthy, mortar-like matter of which the ant hills are formed, beaten up and made into a thick paste with water. The method which I was in the habit of employing was, to cover the skin with a single layer of any old sheeting or calico, then to lay on the Udaka an inch thick, then to apply another piece of old linen, and finally a bandage. This in a short time became perfectly hard, and formed a mould of the part, and could always be removed by pulling at the inner piece of sheeting. With reference to bullet wounds in South African warfare: the missiles of the natives are made of various substances, short pieces of galvanized iron wire such as is used in the country for fences, pieces of rough iron, metal buttons, and often of very soft lead; these latter bullets flatten on meeting with even the slightest obstacle, and the consequence is that they leave an unusually large

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and lacerated channel, which is especially the case with the huge bullets of the elephant guns used by the Zulus, which wounds are followed by long and tedious suppuration. In cases of penetrating wounds, extraction through this channel generally leads to much further injury, and it is advisable to remove the bullet, if possible, by cutting down to it from without, even if one has to cut deeply, provided, of course, there is no anatomical reason to the contrary. I wish to recommend the somewhat general employment of drainage in bullet wounds. Some hours after the bullet and any foreign bodies carried with it have been extracted, and as soon as any moisture appears from the openings, a length of drainage material should be inserted; this is not difficult, unless the track of the bullet is unusually tortuous, and can easily be done by fastening it to the end of a bullet probe or catheter. If a tube is used it may be syringed out once or twice daily with some mild antiseptic lotion, and a horse-hair may be passed through it for the purpose of preventing its becoming blocked. This allows a free exit for all discharges, especially for the dead and blackened shreds of tissue covering the sides of the canal; it also prevents the subsequent burrowing of matter, and traumatic fever and septicæmia will, I believe, be found to be much more rare under this treatment. As soon as the healthy suppuration, which shortly becomes established, begins to subside, and granulations are evident at the mouth of the canal, the drainage material may be removed, and firm bandaging generally completes the cure. The drainage substances I have employed have been of necessity rough ones, either of india-rubber tubing self manufactured, by cutting numerous small apertures in he smallest tubes used for pocket filters, or horse-hairs, purified, after being plucked from the tails, by immersion in carbolic acid. Some of the prepared animal tubes, which, after having served their purpose, undergo absorption, would doubtless be a vast improvement, as they would not continue to act as foreign bodies.

Another practical point to which I wish to allude, where Listerism, of which I have had no experience in the field, is not had recourse to, is a more open treatment of wounds. On long marches, with bullock waggons especially, in conveying large numbers of wounded to the rear, it is almost impossible to pay adequate attention to the wounds where bandages have to be removed, lint bathed off a part, and fresh applications made. I have often been disgusted at the fœtid discharges and maggots which soon accumulate in a hot climate. In my opinion the majority of wounds do much better if the air is allowed free access from the very commencement; but, as a certain amount of protection is always advisable, a thin layer of oakum may be thrown over the part, no bandage whatever being used. When a halt occurs, all that is necessary is for the oakum to be removed, the wound to be syringed over with a little weak carbolic or other antiseptic lotion, and a fresh layer of oakum thrown on the wound. In this way it is possible to take better care of a large number of wounded than a very few could receive, after a great deal of labour, under the usual system. There is little or no fœtor, no larvæ of flies, as there is little of that decomposition in which they flourish; and because, as the capillary vessels nearest the surface of the wound contract rather than dilate, as they invariably do when kept warm and moist and soaking in excretion, from being covered up, the walls of these vessels become less permeable to the migration of white blood cells, consequently there is much less pus formed, and a greater tendency to healing by the first intention.

For handy splints I would call attention to the bark of trees cut from the trunks according to the required size and shape. When taken from somewhat young trees abounding in sap, especially from the mimosa, which grows in every part of South Africa, they soon become moulded more or less to the limb. Astringent medicines are of the first importance in South African campaigns, and from experience I can strongly

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recommend an infusion of the bark of the "water boom" tree. At Ekowe, when the supply of medicines failed, I ascertained this bark to be rich in astringent matter, and used this infusion extensively and with much success. These trees are very generally found growing near water. The principal affections met with in South African warfare are dysentery and diarrhœa; remittent, typho-malarial, and enteric fevers; and the so-called "veldt sore." This ulcer is a pest to those who have to sleep in the veldt, or long grass; it mostly arises primarily from the bites of ticks, which swarm in the grass; these cause a troublesome itching, and a small sore is formed, which henceforth spreads in a circular manner, and is generally seated on the dorsum of the hand and forearm or on the front of the leg, and sometimes produces an ulcer several inches in circumference. My own observations with the microscope led me to conclude that their spreading action depends on the rapid growth of a physomycetous fungus of the Mucorini family, which I likewise found growing luxuriantly on the dead or decaying veldt. I was led in consequence to dress them with ung. sulphuris spread on lint, and so successfully that I recommended its use to several other medical officers, who gave me very favourable reports of it. After this dressing has been applied for two or three days the fungus is evidently destroyed, and any simple application will cause the sores to heal.

With regard to dysentery, enteric and typho-malarial fevers, I feel convinced that the principal agent in their production is the somewhat prolonged occupation of a camping ground, and I base my opinion upon two significant facts: (1.) Nearly all the cases of these affections which I met with, and they were extremely numerous, had their origin in permanent camps, where filth, decomposition, and malarial emanation existed in a sub-tropical heat. I can scarcely remember a case that occurred in a flying column, or for a considerable time after such a column had become stationary. (2.) Enteric fever is by no means a common disease in Natal; indeed, the Colonial Surgeon of the Lower Tugela Division (Dr. Jones) informed me that he never saw a case of it in his district until the troops arrived in 1879, and he informs me (February, 1881) that since the troops left in the latter part of 1879 he has never seen another case. The so-called typho-malarial fever is, in my opinion, simply enteric fever, and nothing else. It is quite possible that malarial emanations influence the symptoms to a certain extent; for instance, I have noticed an especially marked periodic diminution and increase of temperature, but this may also be sometimes observed in ordinary enteric; otherwise neither in the usual symptoms nor in the necroscopies could I ever see the slightest difference. There was, however, a peculiar symptom observed in some of these cases—viz., a cutaneous redness larger than a man's hand, which appeared in various localities, but especially on the side of the thorax and outer side of the upper arm. When first visible it possessed a pinkish hue, becoming in the space of a few hours of a deep crimson; it did not disappear on pressure, was very tender at first, soon becoming painless, the centre was slightly œdematous, and the margins were lost imperceptibly in the surrounding skin. It was always a fatal sign, no case in which it was present recovered to my knowledge, and on incision post mortem small blood coagula were found near the centre, but no pus, this centre being of a dusky hue. At Fort Ekowe some of these cases were present, and with regard to the etiology and period of incubation of enteric fever, I would remark that before our arrival it was utterly unknown there, as I ascertained from a medical missionary who was an old resident. We arrived on January 23rd, 1879, and left it on April 4th, during which time we were closely invested; also when our column marched from the Lower Tugela in January no case had been heard of there. The first case occurred in Ekowe about the end of the first week in March, and no communication whatever took place with the outer world except that very rarely

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a Kaffir runner managed to get through from the Tugela some forty miles off, but these ceased to arrive after February 12th, and it was a considerable time after that date that a case of enteric was heard of at the Lower Tugela.

The sanitary regulations in camps cannot be too rigid, especially where, as in South Africa, a field force invariably includes a native contingent, and to compel these latter men to use latrines is impossible. A high wind springs up nearly every afternoon, and the dust in permanent camps which sweeps in clouds among the tents contains a large proportion of organic matter, quickly pervading the fluid and covering the solid food, besides being inhaled or swallowed with the saliva. The general presence of animal matter is often evidenced by the vast numbers of carnivorous beetles which make their burrows in the soil, as well as by the myriads of flies; these are besides especially abundant in the latrines, from which they emerge with their plumose antennæ and hairy tarsi too often clogged with fæculent matter, to be shortly deposited on articles of diet or to be mingled with the liquids in which they are so often drowned. Of all things belonging to a camp the latrines and cess-pits require the strictest attention. I had an opportunity when at the Lower Tugela of making microscopic observations on these matters, and, without touching on the significance of certain microscopic organisms beyond their partiality for developing in decomposing matters, I found that, as a rule, in from two to four hours micrococci, vibriones and bacteria were numerous, and increased with great rapidity. In latrines earthed at sunset maggots abounded at sunrise. The fæcal matter in latrines should be covered with a good layer of earth about every three hours.

Description of Transport Splint for Immobilizing and Transporting cases of Gunshot and other Injuries of Spine, Pelvis, and Lower Extremities.

Mr. John Fagan, Belfast.

As a general hospital surgeon I have had occasion to treat many cases of fracture and other serious injuries of the spine, pelvis, and lower extremities. I have frequently witnessed a great amount of unnecessary suffering endured by such while being conveyed to hospital on some rude form or litter by kind but untutored assistants, and it occurred to me that if some simple and effectual contrivance could be devised to meet such emergencies, and kept ready for use in our dockyards, foundries, railway centres, and such busy resorts, it would tend to do away with a great amount of suffering and perhaps save many valuable lives.

The element I believe that contributes most to the great mortality attending conservative surgery in gunshot injuries of the thigh is the damage sustained by the parts, owing to the imperfect means employed in immobilizing and transporting such cases. Hence the widely different views held by eminent surgeons as to their treatment, and the marked contrast in the results obtained, as the surrounding circumstances were favourable or otherwise.

I will briefly consider the subject under discussion, under the three following heads:—

1st. The principles involved. 2nd. Some of the means hitherto adopted for carrying out these principles. 3rd. The method I propose as an improvement on those already in practice. Under the first head will come:—(1) rest; (2) freedom from constricting causes; and (3) facility for dressing without disturbing the parts.

The first grand principle to be observed in the treatment of all injuries is rest. I will only notice a few of the contrivances employed which aim at immobilizing the injured parts, and in the case of fractured thigh extending the fragments and keeping

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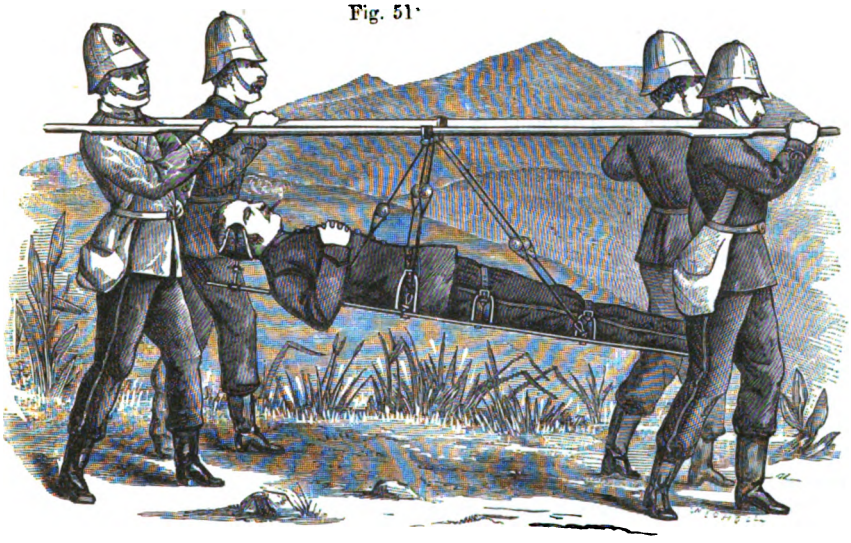
them in apposition. Some of these are good, while others are, to my mind, very injurious. As an instance of the latter I may mention the plaster of Paris splint. Whatever its merits as a means of treating simple fractures it is not for me here to inquire, but as a means of immobilizing and treating gunshot fractures of the thigh, I consider it most objectionable. It forms, no doubt, a solid support, and if applied while the patient is under chloroform will succeed in keeping up extension and the bone in apposition, but the more effectually it accomplishes this object, the more injurious will it be as a constricting agent, while the process of dressing cannot be effectually accomplished without considerable movement and injury to the limb. Seeing the advantage of the application of the principle first in acute disease of joints, it occurred to me that it might with equal reason be extended to the treatment of serious injuries of the trunk and lower extremities, where rest constitutes so important a factor in their treatment.

Fig. 51.



Represents the patient secured for carrying on the splint. Both poles should rest on the shoulders next the patient. The artist, through oversight, represents one of the poles on the outside shoulder.

This splint which you see here, and which I propose naming the "Transport-splint," aims at securing the trunk as well as the limb by a method known as "posterior fixation." Its novelty consists in this, that it serves the double purpose of immobilizing in the one fixture the trunk and extremities, while, at the same time, it acts as a most effectual means of carrying the patient. It is a simple contrivance, consisting of two narrow bars, five padded hoops, a head and foot piece, a small transverse bar, and two carrying poles. The bars are six feet long, three-eighths of an inch square, and weigh between six and seven pounds, they form the basis of the whole fixture. The bands are made of hoop-iron, $1\frac{1}{2}$ by $\frac{1}{8}$ inch. There are five of these, one for thorax, one for pelvis, one for thigh, one for leg, and one for the sound limb. The thoracic and pelvic bands are broadest, having oval plates of sheet-iron attached. The head-rest, which is a broad padded hoop, can be raised and lowered at will, and allows also rotating movements. The foot piece, which has a sling for the heel, permits of flexion and extension, with right and left lateral movement. All these points of support are attached to the bars (which are maintained one inch apart) by a mortised brass slide and cap tightened by a screw, they can be run on and off the bars at will, and placed at any point on them to suit

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different sizes. The whole is swung to a cross-bar by means of cords attached to the sides of the thoracic, pelvic, and leg bands, and these cords are adjustable by modified pulleys or "lock-balls." The cross-bar is eighteen inches long, with six hooks on its lower border for the rings attached to the cords, and at either end is a square socket for the carrying poles. The poles can be doubled up by a joint in their centre when they will serve the purpose of a support for the splint. The whole can be taken to pieces in a minute, put into a very narrow space, or carried over a man's shoulder.

When adjusting the splint for use, the pelvic band should be placed from 9 to 10 inches, above the centre of the bars. The patient should be placed in it, so that the centre of the band should correspond to a point about half an inch above the crest of the ilium. The leg band should be opposite the tuberosity of the tibia, and the thoracic band close to the border of the axilla. After measuring from half an inch above crest of ilium to these points, the bands can be adjusted accordingly. After the patient is swung the head-piece and foot-piece can be accurately adjusted.

If this splint is used for no other purpose than that of immobilizing and transporting cases of serious injuries, gunshot or otherwise, of the trunk and lower extremities, it will, I think, serve a most useful purpose.

As I stated at the commencement of this paper, what puzzles the military surgeon most, is, what is best to be done with a case of compound fracture of the thigh when he has to deal with it in field practice. If he considers it a fit case for conservative surgery he knows it may be so injured by transport that a secondary amputation may have to be performed; and if it is a case for primary amputation, the immediate surroundings are so unfavourable that it will be done at great risk to the patient. With one of these appliances at hand the difficulty disappears. When the patient is placed in it, the limb can suffer no further injury from transport, for it lies evenly balanced, supported at every point, free from constriction, and secure from all jarring influences; in fact, it is placed in a condition of absolute rest.

As a means of immobilizing and transporting gunshot injuries of the spine, pelvis, and femur, this splint, being constructed on the true principle, will, I expect, prove more effectual for such purposes than any other contrivance yet devised.

Let us now for a moment see how far it will meet the requirements necessary for gunshot injuries of the thigh. Hamilton says:—"The limb must not be confined by tight bandages or forcibly extended by apparatus." And Stromeyer, that the great principles to be followed are:—

1. Dressing the wounds without lifting the limb. 2. Avoiding constriction. 3. Not irritating the muscles by straining them by mechanical contrivances.

An examination of this splint will show that it meets all these requirements. With it there is no necessity for tight bandages or straps, the padded hoops being sufficient to steady the limb on the bars underneath; these hoops can be expanded and closed at will, and shifted up or down as the size of the patient or the condition of the wounded part may require.

For purposes of dressing it is peculiarly well adapted, for it comes in contact with only a small area of the limb, which lies fully exposed, and gives the greatest facility for carrying out in the most complete manner the antiseptic method of treatment.

I will make a few suggestions as to the condition under which this splint may be found useful in the practice of military surgery. One man, supposed to know the working of it, carries the whole contrivance over his shoulder; he meets with, say, a case of gunshot fracture of the thigh, he places this splint by the side of the wounded man, takes a few rough measurements, adjusts the padded bands on the bars to suit the size of the person and the part injured. With the aid of a couple of assistants, the patient is placed in the splint, the straps are then buckled, the cords adjusted, the poles put to, and he is carried away to the nearest point of surgical assistance.

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If the surgeon cannot give immediate attention to the patient, he can rest at ease in his splint till his turn comes round. The poles that are used for carrying him are constructed so as to double up and support the splint fully off the ground. A piece of canvas or macintosh thrown over all forms a useful tent, under which, if necessary, he may securely pass the night. The clothes can be cut and removed while examining the limb, without in the least way disturbing it; and, if the case is considered suitable for conservative surgery, antiseptic treatment can be there and then adopted, after which the patient may be dispatched by bearers, ambulance waggon, or railway train, to the place selected for permanent treatment. The excellent code of rules laid down by our respected President, Professor Longmore, is in some respects unnecessary if this splint be used for transport, for it does not matter as regards the wounded man whether the bearers be of equal height or not, whether they march in step or out of step, whether up hill or down hill, on level or rugged ground, the patient maintains all through the same level he had when starting. Other occasions appear to me when this splint might be used with advantage; for instance, in the trenches during siege operations, where men are liable to suffer from injuries of the nature above mentioned, the wounded man could be slung in one of these, where he lies secure till he can be removed under cover of night; or in sending such cases by ambulance waggon, rail, or sea.

I have lain suspended in it for hours together; I have been carried in it by bearers over rough ground; I have made a railway journey of nearly twenty miles in it; and the sense of security given when properly placed and suspended in it must be experienced to be appreciated.

In conclusion, I will say that if this splint of mine be never adopted, being considered perhaps too cumbrous, complicated, and expensive, I will still feel that I have not worked in vain, for some one happier in carrying out the principles involved in its construction will devise a means more perfect in its application.

Sur le Transport des Soldats Malades et Blessés en temps de guerre.

Professor Marinus W. C. Gori, Amsterdam.

"Dès le moment où un soldat tombe sous le plomb ennemi il devient un être sacré, auquel la charité chrétienne et la dignité nationale commandent de donner toute sorte de secours, et le premier et le plus urgent de tous est celui de l'enlever du lieu dangereux qu'il arrose de son sang. Et pourtant, cette partie, si intéressante du service sanitaire n'est pas encore organisée d'une manière capable de satisfaire ni aux hommes de guerre ni aux amis de l'humanité."

Depuis que notre illustre collègue et noble ami le docteur Landa a écrit ces paroles éloquentes, l'intérêt au sort des malheureuses victimes de la guerre est devenu de plus en plus général. Ceux qui ont eu l'occasion d'étudier le matériel sanitaire aux grandes Expositions de Paris, de Vienne, de Bruxelles et de Philadelphie devront reconnaître qu'il est largement dévéloppé dans les dernières années. Il existe, en effet, déjà à un certain degré l'embarras du choix. Par contre, il est généralement reconnu que la critique est devenue de plus en plus difficile. Elle doit être, cela va sans dire, dans la première place scientifique, mais répondre en même temps aux besoins du service militaire. Cette vérité est malheureusement trop souvent oubliée. Des personnes, sans doute très-bien intentionnées, mais tout-à-fait étrangères dans les études de la médecine, s'en sont occupées. Elles se garderont bien de faire des propositions regardant des instruments ou des appareils appartenant à la pratique de l'art de guérir. Elles les trouvent justement très-difficiles, mais se sentent, au contraire, parfaitement en état de juger ou d'améliorer un

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brancard ou une voiture d'ambulance, oubliant trop souvent que ces moyens de transport, par leur construction et plus encore par l'usage qu'on en fait, décident sur le sort des nombreuses victimes de la guerre.

Permettez-moi, messieurs, de le dire sans détours : ce sont les associations sous la Croix Rouge qui ont nourri ce traitement superficiel. Sous ce signe glorieux de l'amour fraternel sur le champ de bataille, l'esprit inventif et surtout la fantaisie des hommes pouvaient trouver beaucoup de terrain pour leur activité souvent trop zélée.

Permettez-moi de le dire, si on pourrait permettre d'un côté à ces soi-disant inventeurs le plaisir calme et l'honneur passager de leurs œuvres, on agirait de l'autre côté contre l'intérêt de la vie des milliers de ces nobles défenseurs de l'indépendance, de l'ordre et de la liberté.

Heureusement, messieurs, nous pouvons fixer l'attention sur différents exemples d'une étude approfondie de cette importante question. C'est à votre exemple, monsieur le président, notre excellent et digne maître, que différentes personnes se sont occupées de l'étude sérieuse des moyens de transport des malades et blessés. Votre livre est étudié partout. A côté de votre nom illustre nous prononçons, avec le plus grand respect, celui de notre collègue tant regretté, Georges A. Otis, médecin militaire au service des Etats-Unis de l'Amérique. En visitant l'Exposition universelle de Philadelphie, comme membre de la commission royale des Pays-Bas, j'ai eu l'occasion de faire la connaissance de cet homme éminent. Je laisse à d'autres juges plus compétents de parler de ses ouvrages, qui sont la gloire de l'armée à laquelle il a appartenu, en répétant seulement vos paroles, écrites le lendemain de sa mort, que les médecins militaires de tous les pays devraient porter le deuil à l'occasion de cette grande perte. Mais il y a encore plus. Longmore et Otis ne sont pas seulement écoutés et suivis par nous tous, messieurs, mais leurs gouvernements respectifs ont heureusement pris leurs conseils au sérieux.

Aprés avoir étudié les meilleurs types des moyens de transport, on veut savoir le nombre dont on aura besoin en temps de guerre. Les chiffres, qui sont les produits de cette calculation, ont conduit des juges compétents à la conclusion qu'on n'aura jamais assez de voitures d'ambulance, et qu'à l'avenir on sera obligé aussi de faire usage de tous les véhicules disponibles, voitures de réquisition, chariots de paysan, etc., tant bien que mal disposés pour ce but. Sortant de là on a instruit le personnel médical et auxiliaire.

Nous avons déjà à notre disposition différents traités et manuels pour nous faciliter cette institution. Mais en même temps on est allé plus loin en préparant d'avance the différents moyens pour l'arrangement des voitures d'ambulance auxiliaires. C'est d'après ma conviction, messieurs, un pas dans une mauvaise direction contre laquelle on ne peut pas assez prévenir les administrations de la guerre. Il est facile de préparer d'avance, à cet effet, des crochets à ressorts, des étaux, etc., et de les mettre en magasin, mais ces moyens temporaires ne seront pas présents au moment où l'on en aura besoin. Différents spécialistes ont prononcé à peu près la même opinion.

Par tout ce qui précède j'ai tâché de vous donner la conviction, messieurs, que cette importante question doit être régardée de toutes ses faces. Elle forme, en effet, une partie souvent trop négligée de la chirurgie militaire. Comme tous les arts soumis à l'inspiration cette branche de l'art de guérir exige des dons naturels que l'étude développe et que l'expérience mûrit. Elle supplée quelquefois à la science, mais elle n'obtient des succès importants que lorsqu'elle s'appuye sur elle. C'est ce qu'un poète a fort bien exprimé en disant: "Pour savoir quelque chose il faut l'avoir appris." Je ne vous fatiguerai pas, messieurs, avec une description des meilleurs type des moyens de transport pour malades et blessés, ou avec l'énumération des conditions auxquelles il doivent répondre. Votre traité "On Ambulances," monsieur le président, est, comme je viens

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de le dire, dans toutes les mains. C'est à votre exemple que je me mis à l'œuvre après le congrès sur cette matière importante, tenu à Paris en 1878.

J'aurai tantôt l'avantage de vous montrer une partie des planches de mon ouvrage qui ne tardera pas, je l'espère vivement, à paraître sous le titre : "On Ambulance Service, together with a Précis of Army Medical Tactics."

Je prends à présent la liberté, messieurs, de répéter la proposition que j'ai faite, il y a déjà trois ans à Paris, d'adopter les résolutions prises à la Conférence au Pavillon Sanitaire de l'Exposition de Vienne en 1873, avec les modifications et les améliorations que l'expérience des derniers temps pourrait rendre nécessaires.

En suivant l'ordre dans lequel les moyens de transport ont été traités à Vienne, nous commençons par les trains sanitaires. Eh bien! Comme je l'ai dit ailleurs, après tout ce qui a été écrit dans ces derniers temps sur les trains sanitaires, on peut dire que la question est épuisée. Ce sujet fut examiné sous toutes ses faces. Les publications de Billroth, Mundy, Longmore et Otis se trouvent entre les mains de toutes les autorités compétentes. Cependant, les résolutions des dites conférences sont très-mal interprétées, et on entend répéter sans cesse cette erreur si grave : Vous voulez l'impossible, des trains sanitaires constitués par des voitures de construction spéciale. Mais, j'ose demander, qui les a défendues ou proposées, messieurs? Personne. Les trains sanitaires exposés à Vienne, à Bruxelles et à Paris ne sont pas destinés à être emmagasinés en temps de paix pour en faire directement usage en temps de guerre. Non, messieurs, les wagons à marchandises destinés à être changés en voitures à malades et blessés, forment et formeront dans l'avenir, en temps de paix, une partie du matériel roulant des compagnies. La conférence a déclaré que du point de vue du secours volontaire, l'équipement des trains sanitaires complètement aménagés est superflu et trop coûteux en temps de paix ; mais en même temps elle a pris des résolutions d'une grande utilité pour l'arrangement et l'usage des trains sanitaires en temps de guerre ou de mobilisation.

Ces mêmes résolutions comprises et acceptées par tous donneront de plus en plus, j'en ai la conviction intime, aux trains sanitaires le caractère d'hôpitaux roulants, répondant aux progrès de l'hygiène hospitalière moderne. Nous ne trouvons pas la moindre difficulté, quant aux brancards de campagne, qu'ils doivent être légers et d'une solidité à toute épreuve. Par contre, un seul modèle de brancards n'est pas nécessaire. En adoptant un modéle unique, tel qu'il puisse être employé sur le champ de bataille, comme dans les voitures d'ambulance et sur les convois de chemins de fer, on a dépassé le but. Eh bien! on peut facilement établir en principe qu'un homme gravement blessé doit être transporté sur le même brancard du champ de bataille jusqu'à l'hôpital, mais dans la pratique on sera souvent forcé d'en agir autrement. Le très-grand nombre de brancards exigés et la difficulté du transport nous rendront impossible de laisser sur les mêmes brancards les blessés très-graves.

Mais, peut-être, monsieur le président, il n'est pas inutile de le rappeler encore une fois, quel que soit le résultat des investigations sur cette matière importante, tous les moyens matériels inventés pour prévenir et adoucir les souffrances de la guerre, n'ont aucune valeur sans l'intervention des personnes capables de s'en servir.

Le matériel d'une ambulance doit être envisagé dans son rapport avec le personnel appelé à en faire usage, et avant tout avec l'organisation de ce personnel. Pour pouvoir juger d'un matériel de transport et de traitement des malades et blessés dans une armée, il faut avoir une connaissance approfondie de son organisation médicale envisagée dans ses rapports avec les autres institutions militaires du pays.

Surgeon-General LONGMORE, President of the Section : The meeting has decided to postpone the discussion on questions connected with the transport of sick and wounded

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in the field until all the papers bearing on the subject have been read. Two papers on this subject still remain; but in the course of the observations which have just been brought to our notice by Dr. Gori, of Amsterdam, that gentleman has made reference to a calamity about which there can be no discussion. I allude to the eloquent tribute which Dr. Gori just now paid to the services and untimely death of that learned and most distinguished American surgeon, Dr. Otis. All of us military surgeons who are in this room, to whatever nation we may belong—and every leading nation is represented here at this moment—all of us have greatly benefited and, so long as we are able to pursue our profession, must still be benefited by those grand, impartial, and comprehensive volumes of the surgical history of the great war of the Rebellion in the United States, which Dr. Otis was spared to complete, and which the Government of the United States have so largely and so liberally distributed among military surgeons in Europe. This seems to be a very fitting occasion, representatives as we are of the science and practice of military surgery in all countries, for us to express our profound regret at Surgeon Otis having been taken away from us before he was able to complete the greatest of all his many valuable professional works as he had hoped to do; and it seems also to be a fitting opportunity to convey to Surgeon-General Barnes and, through him, to all the medical officers of the United States army, our heartfelt sympathy with them on the great loss their medical service, in particular, and, at the same time, military surgical science in all parts of the world, has sustained in the death of their great colleague. I say these few words in the presence of an eminent friend and fellow-labourer of Dr. Otis—Dr. Billings—who occupies an important post in the Surgeon-General's Office at Washington; and I beg to propose to the meeting that Dr. Billings be asked kindly to allow himself to be made the medium of communicating this, I may truly say, international expression of feeling—for I see plainly you all share with me the sentiments which I have tried to express—to the distinguished chief at the head of his department and to his colleagues on his return to Washington.

The proposal was carried by universal assent.

Dr. Billings: In behalf of the medical department of the United States' army, of the Surgeon-General, and of the colleagues and personal friends of Dr. Otis, I desire to return thanks for, and to express the highest appreciation of the eloquent tribute which Surgeon-General Longmore has paid to the memory of Dr. Otis. I shall not attempt to add to the eulogy which Surgeon-General Longmore has pronounced upon my friend and colleague; I can only say that I want words to express the emotion with which I have listened to it, and that I shall convey the message with which he has charged me to the best of my ability. You will all, I am sure, be glad to know that before his death Dr. Otis had completed so much of the surgical history of the war upon which he was engaged as relates to wounds of the extremities. There remains yet to be completed the account of the complications of wounds, such as gangrene, tetanus, septicæmia, &c. Another surgeon of the army will be assigned to complete this history, and you can readily conceive how difficult he will find it to prepare a report which will be the continuation of, and be constantly compared with, the work of Dr. Otis.

La Désinfection du Champ de Bataille.

Médecin Major Guilherme José Ennes, Lisbon.

Suivant pas à pas la mission de la science et de la charité dans leur rôle de dévouement bienfaisant sur les champs de bataille, M. le comte de Beaufort, dans son livre

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"Questions Philanthropiques," ne tient pas grand compte que l'œuvre de la guerre ne devienne plus désastreuse encore pour l'humanité, et il examine simplement le sujet par le côté du sentiment et de la poésie.

Dans l'intérêt des nations civilisées et dans celui de la science, il faut songer à quelque chose de plus important, il faut prendre des précautions extrêmes contre les germes atmosphériques, et il faut, malgré la poésie et le sentiment, les prendre contre le transport direct du germe-ferment et infectieux. En effet, si l'on pense à un énorme chiffre de cadavres de chevaux étendus sur le sol, en putréfaction dans le terrain, dans les rivières, et même altérant le fil d'eau qu'approvisionne souvent et les champs et les habitants, et à des centaines de cadavres humains enterrés avec trop de précipitation, en grandes masses, soit pour cacher le prix de la victoire, soit pour dissimuler l'extension de la déroute, empoisonnant l'air après les batailles à distance même, on déduira qu'un pays dans ces conditions est funeste pour ses habitants, pour l'armée, et encore pour les pauvres blessés, qui ont extrêmement besoin d'un air non vicié et susceptible de remettre en bon état leur sang devenu physiologiquement pauvre.

Emus de ces redoutables causes de maladie, cherchons à en préserver les populations, et cherchons aussi à empêcher l'empoisonnement de l'atmosphère après les batailles. Il y a fatalement trop de désastres à côté du soldat; évitons donc l'agrandissement de l'œuvre de destruction par des mesures préventives sages et énergiques.

II.

La gradation putride des cadavres est changeante selon les variations de la température et selon les conditions et la nature du sol; et, d'après cette spéciale influence, on divise les terrains en trois catégories:—1. Terrain à décomposition de courte durée des matières animales; 2. Terres mixtes; 3. Terrains à décomposition lente ou incertaine. Dans les premiers sont compris les terrains silicieux et calcaires; dans la deuxième catégorie sont admis les terrains schisteux, calco-schisteux et schisteux à fond granitique; et dans la troisième classe on doit inclure les terrains argileux, d'alluvion, et les terres argilo-calcaires.

Or, comme on ne se donne pas toujours, après les batailles, la peine même de creuser une fosse, et on se contente souvent de jeter les cadavres humains et d'animaux pêle-mêle dans des fossés, sur des chemins, en les recouvrant de terre, quelquefois pas trop abondante; et comme, d'une autre part, on ne fait pas certainement l'étude de la nature du terrain et de son action plus ou moins grande sur la conservation des cadavres, il s'en suit logiquement que des masses énormes de vibrions, dans la phrase d'Ehrenberg, et de sporules vivants ou de cellules vivantes, selon le mot consacré par Dumas, se répandront dans l'air avec leur activité toxique, et qu'il faut en finir, sur le champ de bataille, avec les inhumations, source certaine des plus funestes émanations.

III.

Pour l'action conservatrice des vêtements sur la décomposition des cadavres, écoutons Louis Crémous, un illustre chimiste belge, qui connaît la question pratiquement, ayant été délégué par le gouvernement de son pays pour l'assainissement des champs de bataille de Sedan et des environs, et qui insiste, avec la plus grande rigueur, sur les grandes précautions hygiéniques indispensables après les batailles:

"Les vêtements, selon leur nature, ont une action sur la putréfaction des cadavres. Partout j'ai observé que le drap conserve les cadavres d'une façon complète. Ainsi dans des terrains perméables mêmes, tandis que les figures et les mains étaient fortement altérées ou décomposées, les corps vêtus de drap étaient parfaitement intacts. J'ai vu

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des officiers allemands, qui presque tous portaient des gilets de flanelle et des bas de laine, dans un état de parfaite conservation. Souvent, pour ceux-ci, on avait eu soin de placer leur képi de drap sur la figure, et alors cette partie du corps était aussi intacte que le reste. Je pus aussi remarquer, dans quelques fosses de soldats morts aux ambulances, que ceux enveloppés dans des suaires de coton étaient aussi plus décomposés que ceux enveloppés dans la toile. Plusieurs soldats ayant été enterrés sans bottines, je pus observer que ceux qui portaient des chaussettes en coton avaient les pieds altérés, au lieu que ceux qui portaient de la laine les avaient parfaitement conservés. Une particularité qui mérite d'être notée, c'est l'action de la poudre sur les chairs des cadavres. Plusieurs fois j'ai vu des soldats sur lesquels on avait jeté des débris de guerre, et, entre autres, des gibernes détruites et remplies de cartouches. Les chairs étaient entièrement détériorées et en pleine putréfaction.

"On excite ainsi aussi la cupidité des gens pauvres, qui vont fouiller dans les fosses pour s'emparer du peu de butin qu'ils croient y retrouver, tout en s'exposant et en exposant les autres."

L'hygiène conseille donc l'inhumation des soldats déjà privés de leur vêtements, et la prudence le recommande surtout pour ne pas exciter à la violation des cadavres, à la cupidité et au vol.

IV.

Dans les plaines du champs de bataille, il y a trois indications capitales:

1.—Désinfecter l'atmosphère;
2.—Désinfecter le terrain imprégné;
3.—Détruire la source même des agents producteurs des émanations.

Mais, qu'on s'imagine un grand champ de bataille, des chemins, des cours, des jardins, de vastes espaces couverts de corps inanimés! Qu'on se fasse une idée de tout cela, et qu'on nous dise si on arrivera à obtenir un bon et prompt résultat avec les feux allumés en plein air; avec les fumigations acides et alcalines; avec l'emploi copieux de l'acide phénique; avec les fumigations d'acide muriatique; avec les fumigations de chlore ou les fumigations aromatiques? Si c'était pour un espace restreint, on serait pleinement d'accord, d'autant plus que les agents désinfectants, presque toujours d'odeur forte, forcent à ouvrir portes et fenêtres, ce qui est un excellent procédé: mais pour la désinfection des plaines d'un champ de bataille, tout cela n'est pas évidemment le moyen énergique, prompt et donnant toutes les garanties à l'avenir.

V.

Une fois posée l'impossibilité pratique de la désinfection du champ de bataille par les désinfectants chimiques; une fois établi aussi son inefficacité, dans ces conditions, comme nous le dirons dans les conclusions de ce modeste travail, à l'égard de tous les agents d'assainissement, et inclusivement à l'égard de l'acide phénique; et encore vu le peu de stabilité des opinions scientifiques, on n'a qu'à recourir au système de la crémation sur place, comme remplaçant définitif de l'inhumation sur le champ de bataille, applicable par tous les temps et dans toutes les localités, et réalisable, à l'aide d'un bon agent de combustion, économiquement et sans danger pour les opérateurs.

Et, quant aux objections de Lacassagne, qui hésite sur le mode d'obtenir facilement le combustible et de le transporter, et encore sur l'impossibilité morale pour l'armée de se faire accompagner d'un cortége de funèbres appareils; et pour répondre encore aux opinions du professeur Vallin, qui conseille toujours le procédé commun, parce qu'il le trouve plus simple, plus pratique et plus d'accord avec les habitudes des populations, en dédaignant de la préférence de l'incinération, comme suprême moyen hygiénique

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applicable aux plaines du champ de bataille, citons une fois de plus les expériences de Créteur, qui affirme le principe de la crémation, ses avantages, et l'agent propre à pouvoir réduire les cadavres directement sur place.

" La crémation faite à l'aide du goudron de houille, après une bataille, a, me semble-t-il, plusieurs raisons d'être.

" 1.—La fumée, en se répandant, purifierait l'air vicié par les produits de la combustion de la poudre, soit en contrariant la formation de ces produits, soit en les entraînant, et sauvegarderait ainsi la santé des blessés dans les ambulances, et des soldats bivaquant dans les lieux mêmes du combat;

" 2.—On empêcherait ainsi la formation de ces charniers appelés à produire les plus graves maladies parmi les populations entières;

" 3.—On ne forcerait pas les habitants ruinés et ravagés à des expropriations arbitraires;

" 4.—On empêcherait la violation des tombeaux des victimes;

" Enfin les parties intéressées y trouveraient l'immense avantage de réunir en un seul endroit les corps des leurs, dont l'incinération, faite immédiatement après les batailles, à l'aide du goudron de houille, d'après mes calculs, ne coûterait pas quinze centimes par homme.

" Ces résultats et ces observations, résultant d'un travail qui pour la première fois s'accomplissait dans ces conditions, et qui été appelé à rendre les plus grands services à l'humanité, je les livre, autant dans l'intérêt des nations civilisées que dans celui des sciences, afin que chacune d'elles en prenne sa part et en fasse son profit."

Guidé par ces idées, je m'attache au procédé crématoire, comme le moyen hygiénique sûr pour rétablir et pour assurer l'assainissement du champ de bataille.

CONCLUSIONS.

1.—Il est indispensable, pour plusieurs raisons, de prendre les plus sérieuses précautions hygiéniques après les batailles; l'intérêt de l'armée même, celui des habitants des localités avoisinantes du théâtre de la lutte, et des blessés surtout, exige dans ces circonstances, le sévère respect des lois de l'hygiène;

2.—L'établissement des ambulances dans des endroits où des batailles ont été livrées place les blessés dans les plus nuisibles conditions hygiéniques;

3.—L'influence du sol, aidant ou empêchant la décomposition, plus ou moins rapide, des cadavres, selon la composition chimique des terrains dans lesquels les corps sont déposés, ne peut pas être méprisée dans l'œuvre hygiénique du champ de bataille;

4.—L'hygiène sur les champs de bataille implique aussi la connaissance exacte de l'action de la température sur les matières animales.

5.—L'hygiène prescrit l'inhumation des soldats, après les avoir privé de leurs vêtements;

6.—La désinfection des plaines d'un champ de bataille embrasse l'assainissement de 'air ambiant et du sol impregné, et l'indication de frapper de mort la source des agents producteurs des miasmes;

7.—Il paraît acquis pour la science:—

(*a*) Que l'acide phénique est simplement un agent insecticide, et qu'il n'agit pas chimiquement sur les gaz méphitiques;

(*b*) Que le chlore gazeux, l'acide azotique, le sulfate de fer, et le chlorure de chaux doivent être appréciés comme de véritables agents désinfectants pour l'odeur cadavérique et pour les organismes vivants;

(*c*) Que l'acide hydrochlorique fait recroître l'odeur cadavérique sur le point de se dissiper.

8.—La crémation est le seul moyen absolument certain pour désinfecter le champ de bataille.

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Sur le Transport des Malades et des Blessés dans les pays non-Civilisés.

Don Nicasio de Landa y Alvarez, Pampeluna.

Messieurs et très-honorables confrères,—Ce n'est pas sans émotion que j'ose élever ma voix dans cette assemblée où je vois réunis tous les grands noms qui honorent la chirurgie militaire de nos jours, tous ceux qui ont employé leurs talents, leur force, leur vie au progrès de la science, à la diffusion de la lumière, et, ce qui est encore plus beau, au service de l'humanité.

Dans nos jours, on peut dire que la médecine militaire, ou la chirurgie d'armée, constitue une branche spéciale de la science qui a ses ancêtres dans les noms vénérés de Ambroise Paré, de Cristobal Perez de Herrera, du Chevalier Pringle, des Larrey et de Guthrie, et dont les champions actuels se trouvent presque tous dans cette enceinte et devant lesquels je m'incline avec le plus profond respect.

Cette science spéciale va faire encore un pas par la discussion des thèmes qui nous sont soumis et qui offrent tous le plus vif intérêt; permettez-moi, messieurs, d'avoir l'honneur d'y prendre part, en vous soumettant, dans la mesure de mes forces, quelques observations sur le quatrième des thèmes qui nous ont été soumis, c'est-à-dire sur les améliorations dans le matériel d'hôpital et de transport dans les campagnes qui ont lieu dans les contrées non civilisées.

Précisément j'ai reçu le baptême de feu à la campagne du Maroc, et là j'ai eu la douleur de voir déposer dans nos ambulances des corps décapités par nos sauvages ennemis, et quelquefois on m'a apporté aussi les têtes coupées achetées par nos soldats. Plus récemment dans la guerre que l'Espagne a dû soutenir contre les séparatistes dans l'île de Cuba, les mêmes outrages à l'humanité ont été réproduits : nos blessés étaient écharpés et nos médecins aussi, lorsqu'ils tombaient au pouvoir des insurgés; nous avons ainsi perdu sept collègues. Le corps médical espagnol, lui aussi, a donc la triste expérience des guerres sans merci, et comme autrefois le Dr Perez de Herrera, le bouclier et l'épée à la main, protégeait l'embarquement de ses blessés sur la côte d'Afrique, à l'île de Cuba aussi on a trouvé un des mes collègues qui, faisant une forteresse de l'hôpital a repoussé l'ennemi avec le feu nourri de ses convalescents.

Nous trouvons donc très-important le thème soumis à la discussion, et nous comptons bieu profiter des lumières acquises, à leur tour, par nos collègues de l'armée anglaise dans ses récentes et victorieuses campagnes du Zululand et de l'Afghanistan.

Les modifications à introduire dans pareils cas dans le matériel sanitaire de l'armée d'opérations sont de deux ordres que spécifie le thème : (1.) Dans le matériel de pansement; (2.) Dans le matériel de transport.

Pour le matériel des ambulances, comme les colonnes d'opérations ne peuvent dans ces guerres avoir derrière eux un long convoi, il faut le réduire au strict nécessaire: très-peu de médicaments; une bonne provision de sulfate de quinine, d'acide phénique, d'extrait de viande, de lait condensé et de *lime juice* pourra suffire; mais il faut beaucoup de matériel de pansement, beaucoup de coton et de gaze, parce qu'il faudra renouveler souvent les pansements des blessés qui suivront la colonne. On a vu à Cuba rentrer quelques colonnes d'opérations où non seulement les soldats, mais les officiers et les chefs revenaient sans chemise: c'est que tous avaient donné leur linge pour le pansement des blessés qu'ils trainaient avec eux.

Un des moyens d'assurer cette provision sans surcharger les ambulances c'est de faire porter à chaque homme son premier pansement : ce moyen employé en Allemagne l'a été aussi à Cuba où le Depôt sanitaire de Madrid a envoyé plusieurs milliers de petites poches avec du linge et de la charpie, qui ont été distribués à chaque soldat.

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Pendant la guerre civile contre les Carlistes on a répandu dans l'armée libérale le mouchoir de notre éminent collègue le Dr Esmarch de Kiel, qui a été reproduit par le Zomento Nacional de Barcelone; et le Comité des Dames de la Croix Rouge, que présidait Madame la Duchesse de Medinacœli, a répandu aussi par milliers dans les deux camps le pansement tout fait qui porte mon nom, et dont j'ai l'honneur de mettre sous vos yeux un exemplaire. Vous y verrez que les diverses pièces constituent le pansement; soie, gaze, coton et bande en tarlatane sont cousues ensemble et ne forment qu'une seule pièce: c'est pour cela que je l'ai nommé "pansement rapide."

Mais la plus grave difficulté se soulève au sujet du deuxième point, c'est-à-dire, sur le matériel de transport. La meilleure manière de la lever serait d'opérer toujours à proximité de la côte, qui serait allongée par des bateaux hôpitaux toujours prêts à récevoir les blessés et les porter soit sur la mère patrie, soit dans un endroit ou neutre ou ami. C'est ce que nous avons fait pendant la campagne du Maroc; jusqu'à la prise de Tétouan, le maréchal O'Donnel portait son armée près de la côte et de grands bateaux hôpitaux reçurent les blessés de Castillijos, de Montenegron, de Djilitli et de Tétouan.

Mais ce ne sera pas toujours possible; là aussi il a fallut s'interner, et les blessés de Quadras dûrent être portés en convoi.

Pour les convois il ne faut pas compter avec les belles voitures d'ambulance que nous devons aux constructeurs Locati de Turin, Kellner de Paris, et Lhoner de Vienne, parce que dans les pays peu civilisés il n'y a pas de routes.

Porter les blessés sur des civières à bras d'homme, c'est une corvée écrasante pour les soldats, lorsqu'il faut le prolonger pendant beaucoup de jours et à longues distances et avec des probabilités de combat.

Pour emporter une vingtaine de blessés graves, après le combat d'Oroquieta il me fallut racoler une centaine de paysans pour leur faire faire un trajet de quatre lieues. Si on ajoute les ardeurs d'un climat intertropical qui ne permet pas de continuer ce travail pour plus d'une demi-heure, on voit qu'il faudrait trente-deux hommes pour porter chaque brancard dans chaque étape. C'est ce que m'assure un de mes collègues, qui s'est beaucoup distingué dans la campagne de Cuba, le Dr Tusto Gonzalès, et c'est lui aussi qui m'a expliqué le moyen de transport employé dans cette guerre par nos colonnes, pour emporter ses blessés, et qui consiste à suspendre le brancard entre deux petits chevaux, dans la forme représentée dans les croquis que j'ai l'honneur de mettre sous vos yeux, en vous priant de me pardonner les incorrections de mon dessin.

Vous voyez, messieurs, que s'il s'agit de substituer les chevaux aux hommes, il faut pour cela prolonger les bâtons du brancard d'un mètre à peu près; généralement on improvisait le brancard en découpant dans le bois deux longues branches qu'on passait dans des fourreaux de toile des dimensions de la toile du brancard, qu'on emportait preparés pour la circonstance; une simple corde suffit pour assujetir les extrémités des bâtons entre eux et pour le passer sous le ventre de l'animal; il ne faut pas de bât spécial, on peut même s'en passer de bât. C'est par ce moyen qu'on a fait le transport des blessés dans les colonnes d'opérations qui parcouraient les savanes de l'île de Cuba, sans qu'on ait eu à regretter aucun inconvénient, et ceux qui ont été transportés ainsi, pendant plusieurs jours, s'accordent à dire qu'ils ne ressentaient aucune incommodité dans ces vèhicules.

Cette expérience de mon armée m'autorise à recommander à votre bienveillante attention cette solution du problème qui nous occupe. Je la trouve préférable aux cacolets litières employés par l'armée française en Algérie, et que je crois ont été adoptés aussi dans l'armée anglaise, mais je doute beaucoup qu'il survivent à l'épreuve de l'expérience.

Il y a bien longtemps, lorsque dans le congrès de Genève de 1864, j'avais l'honneur

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de siéger près de notre honorable président, le Dr Longmore, j'ai dû faire un peu la guerre aux cacolets que notre excellent collègue, l'intendant délégué de la France, nous présentait comme une solution du problème du transport pour la grande guerre; aujourd'hui je dois dire qu'ils ne sont acceptables non plus dans la petite guerre ou guerre de montagne.

Je n'accepte pas même le cacolet ordinaire, parce que les blessés qui peuvent y aller assis, pourront aussi bien être montés sur un petit cheval du convoi. Les difficultés du chargement pour niveler le poids, l'irrégularité de la marche, le besoin d'avoir de forts mulets pour supporter le poids de deux hommes, tandis qu'on trouve plus facilement des petits chevaux et même des ânes, me font trouver ce moyen de transport très-peu pratique. Quant aux cacolets litières les difficultés augmentent dans une forte proportion, ainsi que les risques du pauvre blessé; nos confrères de France ont constaté la mort d'un brave colonel porté dans un de ces appareils et lancé sur les pavés de Metz par un soubresaut de son mulet.

Ainsi je crois que le matériel de transport sanitaire pour les armées qui opèrent dans des pays non civilisés doivent consister en une bonne provision de brancards et de toiles de brancard doubles, et beaucoup de petits chevaux qu'il sera préférable de prendre dans le pays même où l'on fait la guerre, tant qu'il sera possible.

Messieurs, en vous remerciant de votre bienveillante attention dont vous m'avez honoré, je ne puis finir sans vous exprimer un vœu qui j'espère trouvera de l'écho dans vos cœurs. Représentants de toutes les armées civilisées, Dieu veuille que désormais ce soit toujours dans des pays non civilisés que nous portions nos armes victorieuses. On a bien dit que dans notre temps toute guerre entre états européens est une guerre civile. La guerre contre un état civilisé est un retour à la barbarie, tandis que les baïonnettes portées dans un pays sauvage peuvent y être les conducteurs d'acier de la lumière du progrès et de la civilisation.

DISCUSSION.

Surgeon-General Mouat, V.C., C.B., London: Notwithstanding the valuable collection of sick carriages exhibited in London, Paris, Brussels, Vienna, &c., a perfect ambulance conveyance on four wheels, that can go anywhere, and combining all the requisite qualities of simplicity of construction, cheapness, lightness, portability, and capability of repair by any workmen; a vehicle that can be taken to pieces and packed for board-ship, is still a desideratum for all armies. Ours have nearly all proved useless in savage warfare; they all attempt too much, and are too lumbering and complicated. The American models fulfil these indications nearer than any other. I had myself an opportunity of testing the value of a cheap kind of ambulance waggon, extemporized from an ordinary express American waggon in the last New Zealand war. I was able to have these simple waggons (little more than a tray on wheels) converted into useful efficient transport with stretchers, water barrels, and a hood, at a trifling cost, in forty-eight hours; and they proved most useful and valuable in passing rivers and other obstacles.

Surgeon-Major Christen Smith, Christiania: I think that the necessity of having recourse to what is commonly termed improvisations does not apply only to a warfare in wild countries and amongst uncivilized men. There are small nations, who for a long time may live in peace with their neighbours, and who wish always to do so But it has happened, and it may happen again, that such small States, against their will, become involved in the contest of greater conflicting powers. It is unlikely that such

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a nation will continually provide a complete outfit for the medical department of costly carriages of approved construction. And so it may come to pass, that, when the calamity of war breaks out, this nation may stand wholly unprepared for the transport of its wounded defenders, and in its hour of need be left to the most miserable shifts. Revolving this point seriously with regard to my own country, I thought it might be possible and right to press into the service of the medical department for the transport of sick and wounded soldiers such common and strong waggons as serve the farmer and the peasant in removing the produce of their fields, and which are to be found everywhere in great numbers. But, to make them really serviceable for such a purpose, it is necessary to make several preparations beforehand, and in fact to leave nothing to improvisation. I found it necessary to relieve the shocks and shakings in passing over a rough country, as well as to protect the wounded against the inclemencies of weather, by using such simple means as can be found and applied almost everywhere. I find it further necessary to let the waggon, after it has been prepared in this way, undergo the most severe trials to prove how far it may be trusted. And, lastly, I think it to be of the greatest moment to instruct the bearer companies—officers and men—thoroughly in its construction and use, so that after a certain time men may be found spread over the whole country completely familiar with it, and able to give proper instructions to others.

Surgeon BILLINGS, Washington, D.C. : spoke of some extemporized methods of transporting wounded men employed on the frontier in the United States where the use of wheeled vehicles is out of the question. The method used by the Indians is to carry the wounded man in a blanket, swung hammock fashion between two long, slender elastic poles. One end of each of these poles drags on the ground, the other ends are fastened at the sides of the saddle of a horse or mule. The comfort thus secured for a wounded man is much greater than might be supposed from mere *à priori* consideration. A case was also mentioned in which a wounded man was carried several hundred miles seated in a large trunk strapped on the packsaddle of a mule, one end of this trunk having been removed to permit the adjustment of the splints for the injured extremity. Alluding to the paper of Dr. Janes on gunshot fractures of the femur, attention was called to the fact that mere statistics of the comparative mortality in amputation, as compared with the so called conservative treatment of such injuries, are not sufficient to decide the question of their relative merits. The subsequent suffering and deformity where an attempt is made to preserve the limb in cases of gunshot fracture of the femur are often very great, and many of those who have escaped with their lives and limbs after such an injury bitterly regret that they had not taken the risks of an amputation. While conservative surgery is the highest triumph of the art, there is a conservative surgery which is born of timidity, and the results of which are, in the long run, pernicious.

Surgeon-General ROTH, Dresden: The questions which have been discussed to-day concerning the transport of wounded, in my opinion, all rest upon the same basis. As to the main points the same institutions exist in different armies, since the transport of wounded men generally must follow the same principles. The experiences of the last great wars, particularly the Franco-German and Russo-Turkish ones, have proved that the difference of the means of transport matters little. Stretchers and railroads finally are for large numbers of wounded the only means of conveyance to be relied upon, because carriages (or waggons) scarcely ever can be concentrated in sufficient number at the places of most importance. The general disposition remains always the main thing, and from this point of view Pirogoff is quite right in asserting that the

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fate of the wounded soldier depends more upon the administration than upon the medical man. The most important point is the *personnel* of the army hospital corps. Well-drilled bearer companies will make the best of every kind of material, therefore the object must be the education of these bearers, who ought to be at the complete disposal of the army medical departments. For the inauguration of such an organization we are indebted to America, from whence issued a model of sanitary service formerly unknown in Europe. The development of the sanitary service after the model of the so-called technical troops, the application of medical knowledge to the largest extent in the interest of the army, combined with the greatest possible independence of the sanitary service, will be better able to secure successful transport of wounded than the development of mere technical questions, which will follow by itself. This ought to be the scope of our exertions.

The PRESIDENT made some concluding remarks and the discussion terminated.

On the Prevalence of Enteric Fever among Young Soldiers in India; its Causes, and the most Rational Means of Prevention.

Surgeon General Professor MACLEAN, C.B., Netley.

Mr. CHAIRMAN AND GENTLEMEN,—The subject to which for a brief space of time I am about to invite the attention of this section of the Congress is one that of late years has much occupied the attention of military medical officers in India. It is the prevalence of enteric fever among young soldiers during the early years of their service; the nature of this fever; its causes; and the most rational means of prevention. It is not in India only that this subject has awakened attention, as I shall presently show; the French army serving in Algeria has in like manner suffered, and very notably also during the occupation of Rome.

It was Dr. Bryden who first seriously awakened the attention of the profession in India to the wide-spread distribution of enteric fever in that country, and brought out the fact that youth and recent arrival there are intimately related as predisposing causes of this disease.

M. Léon Colin, one of the most distinguished physicians in the army of France, and professor in the great military hospital of the Val-de-grâce, has referred me to a valuable *brochure*, originally published by him in the March and April numbers for the year 1878, of the *Archives Générales de Médicine*. It appears that enteric fever was fatal to the French army in Algeria to the extent of 4·63 per 1,000 in the year 1868, and that the death rate in that country from the same disease was in 1872, 2·2; in 1873, 2·23; 1874, 3·2 per 1,000 effectives. As in India, so in Algeria, the young and the newly arrived were the victims. When the Pontifical States were occupied by the French army the mortality attained in the year 1868, from the same fever, to the enormous figure of 20·3 per 1000 effectives. It is a notable fact, to be afterwards referred to, that along with this great mortality from enteric fever, the same regiments serving in Rome and Algeria simultaneously lost severely from pernicious malarial fevers, and from that fatal malarial form of pneumonia from which the French army, during its stay in Italy, suffered so severely. For example, in 1868 the 35th regiment of the line lost in Rome 70 men, a mortality of 41·15 per 1,000 present; of these 48

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died from enteric fever, 8 from intermittent, and 10 from pneumonia; while another regiment serving at the same time in Algeria lost in one year 13 men from enteric, and 15 from malarial, fever. I may be permitted to say with reference to pneumonia, as seen in malaria poisoned men, that the fatal rapidity with which consolidation of the lungs takes place in this disease in malaria-struck subjects is a fact with which at Netley we are quite familiar.

Up to the year 1861 enteric fever never appeared in the medical statistics of the army of India, European or native. Fevers were registered under three heads—intermittent, remittent, and continued. The two first were regarded as without question due to malaria; under the heading *Febris communis continua*, were classed all fevers not distinctly paroxysmal. Few now doubt that hundreds of cases, formerly regarded as the adynamic form of remittent, would now be classed as enteric, and still fewer that an enormous proportion of those entered in the returns as common continued fever, more particularly those with a history of bowel complication, as the diarrhœa of such cases was often phrased, were nothing else than enteric fever, as we now understand that term.

We must bear in mind that it was only about the year 1861, or thereabouts, that clinical thermometry in India was brought to aid in the diagnosis of fevers. In the General Hospital at Madras, so far back as the year 1838, the year of my first connection with India, in the old infantry barracks of Secunderabad, in the Deccan, of dysenteric notoriety, and on service in China, as far north as Nankin, I saw and treated cases of continued fever, extending over twenty days in duration, with the bowel complication above referred to, that, without hesitation, if I saw them now, I would diagnose as enteric fever. The symptoms were the same, the mortality exceeded that of fevers distinctly malarial, they were not amenable to quinine freely given, and death from hæmorrhage from the bowels was frequent, and the intestinal lesions were what we now recognize as characteristic of enteric fever. It was on my return to India after two years' residence in Europe that I first satisfied myself that such cases, in all their essential particulars, were none other than enteric fever, as that term is understood among you here. I then saw, treated, and dissected cases as distinctly enteric in their symptoms, progress, and in their post-mortem lesions, as any to be seen in the London Fever Hospital. It is useless to add that all the attempts since made to persuade the public and the profession that enteric fever is not a disease of India have had no weight with me. I now beg to hand round drawings from the facile pencil of my friend, Surgeon-Major Gillespie, made in the dead-house of an Indian hospital, from the intestines of men dead of fever of a distinctly non-malarious type, and I request you to examine the preparation in this jar, taken from the body of a young soldier who died in India, of a disease not in any essential particular to be distinguished from the fever with which in your hospitals and various fields of practice you are so familiar.

The question has arisen, Is enteric fever a new or an old disease in India? I regard this as an unprofitable discussion, and believe we might as well ask, Does the existence of enteric fever in Great Britain date only from the researches of Dr. A. P. Stewart and Sir William Jenner? No attentive student of Indian medical literature can doubt that Clark, Annesley, Twining, and many others, saw, treated, and dissected cases of enteric fever. There are more Europeans now in India of the enteric-fever-liability age than in the days of the men I have named; we study disease from better stand-points, with more light, and better means of research, and, as a natural result, our diagnosis is better. This, I believe, in few words, is the whole matter.

And now as to the all-important question of causation. It has been said—I maintain on very insufficient data—that enteric fever is not a disease from which the natives of

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India suffer. We know that "fevers" kill more of the native races than all other diseases put together. I have just seen the annual report of the health of Calcutta for the year 1880. I observe that 441 deaths among the native population are put down to enteric fever; the health officer doubts whether all were in reality due to that disease, but I notice a significant entry—viz., 2,072 fatal cases of continued fever, many of them with diarrhœa. How many of these were cases of enteric fever? The deaths of thousands of natives all over India are put down as due to fevers, but the exact nature of the fever is known only in the comparatively small number of cases diagnosed by competent observers. I am not here to dogmatize, and to assert that enteric fever in India has no other factors than those with which sanitarians have made us familiar at home; but this I do say, that those who assert that the fever, so fatal to our young soldiers in India, cannot in any case be traced to pythogenic origin, have a difficult thesis to maintain. Few in this country doubt that this fever, in the language of my colleague, Professor Aitken, is generated by a specific morbid cause, fostered by, if not actually born of, decomposing nitrogenous matter. I ask, is there any country in the world in which decomposing nitrogenous matter is more abundant than in India? If so, what is there in the nature of the climate, in the various soils, to hinder the birth of the *contagium* of this disease, that elsewhere owns decomposing nitrogenous matter for its parent? It is said, and truly said, that the present sanitary condition of our barracks in India and their surroundings is such as to forbid the origin of such a *contagium* in or about them. On this point I observe that this admitted improvement is, comparatively speaking, a thing of yesterday. I have seen, and very notably in the barracks of Fort St. George, in Madras, and the old infantry barracks of Secunderabad, of dysenteric notoriety, the latrines so placed, and the conservancy of them so bad, that a privy atmosphere pervaded the dormitories of those placed near them. When a very young man, in temporary charge of the Queen's Garrison Hospital, of Fort St. George, in Madras, I placed it on record, that the majority of cases brought into that hospital of dysentery, diarrhœa, fever, cholera, came from the barrack rooms under this influence; and Surgeon-General Cornish has noted that when the dry earth system of conservancy was substituted for the old latrines, and when the sea wall of the fortress was thrown down admitting the free perflation of air, an immediate diminution in the number of fevers took place. The same authority notes that when typhoid fever prevailed in the artillery barracks at St. Thomas' Mount, near Madras, "local nuisances were abundant, in the shape of foul drains and cesspools in the neighbourhood of barracks and hospitals. As for the old privies of the Secunderabad barracks—I quote from the official report of Superintending Surgeon Cole—" they were positively pestiferous, the buildings and their neighbourhood were sodden and saturated with the foul imbibitions of a long series of years, and emitted most offensive odours." When this description was written, the regiment then in barracks was suffering from a non-malarial fever of continued type, and extremely fatal, and Mr. Cole was clearly of opinion that the disease was propagated by a direct contagium from the morbid alvine discharges. I pray my audience to note this significant fact. The 17th Lancers occupied these barracks after those dreadful latrines had been abolished, and the dry earth system substituted, when the walls were purified, and overcrowding abolished, and the result was that, in three years that fine regiment, with a strength of 576, lost only 37 men from all causes. The Royal Scots in the pre-sanitary era, having, in the same barracks, lost 104 men in one year from dysentery alone, out of a strength of 1,098 men. But, admitting, as I do, the immense improvements in the sanitary conditions of the barracks in India of the present day, I ask do our soldiers confine themselves to barracks? Noto-

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riously they do not. The native bazaars and villages, within easy walking distance of most of our cantonments, stand on soil for ages sodden with excremental matter, and the tanks and wells of such places are befouled with like impurities. This being so, we naturally turn to the media by which such impurities may be conveyed into the system. Are these not the same there as here? Organic laden air, water, milk, or other beverages, articles of food, and, as Dr. Kerr Innes has suggested, even clothes washed in tainted water. We know, on authority, that few English sanitarians, at all events, will contest, that, in one or other of the ways mentioned, cholera has, in examples without number, been propagated both here and in India, and why not the contagium of enteric fever. It cannot be carried into our barracks by sewer air for we have no sewers there; but will it be gravely argued that in the places of which I have spoken there is no such thing as organic laden air? I know the pains taken in India to procure for our soldiers pure, or at least relatively pure water; but, once more, is it only in barracks that our soldiers drink? The fact is, and must be, they drink when thirsty, and they drink what comes first to hand, partaking of beverages made with water taken from the sources already mentioned. But is the water supplied to barracks always so unexceptionable as has often been said? In the barracks at Rawal Pindi some years ago, an outbreak of enteric fever occurred, and several men died; one of the drawings handed round was taken from the intestine of one of the victims. Now, one of the wells of supply was found to contain more than twelve grains of organic matter per gallon, and was thick and offensive. Dr. Gillespie declines to say positively that this was the cause of the outbreak, but he notes the significant fact that, after that well was closed, no more cases of enteric fever presented themselves. I repeat, I do not wish to dogmatize. But this I do say, that our soldiers are exposed to the same causes we know to be powerful in the genesis and propagation of the *contagium* of enteric fever in this and other countries in Europe, and that I know no reason why what operates here should have no effect in India.

I have already quoted some statistics from M. Léon Colin's brochure, entitled "De la Fièvre Typhoïde Palustre." This military physician does not admit the opinion put forth by M. Boudin, that there is a complete antagonism between malarial and enteric fever. On the contrary, he holds that there is a certain affinity between the two affections. According to M. Léon Colin, typhoid fever is a disease that may spontaneously arise under all the conditions where the organism falls under the influence of putrid miasms of different origins, whether urban or paludal. Every febrile movement, accompanied by great alterations in the secretions and intense gastro-intestinal alterations, as in those of continued or paludal remittent fever, places the organism in a condition to suffer from typhoid, by what M. Léon Colin calls auto-infection. This author has, in the work indicated, demonstrated by numerous observations the frequency of enteric fever in the most intensely malarial places; and he is of opinion that pernicious remittent fevers of malarial origin frequently pass, by what he calls a process of "transformation," into typhoid; moreover, that it is in such places that typhoid fever strikes especially fresh arrivals, just as it does fresh arrivals in crowded cities. In the French army if soldiers are suddenly landed in Algeria, they are attacked in great numbers by typhoid fever; the same happens as surely when suddenly transferred to the Agro Romano. M. Léon Colin, writing to me on the subject of this paper, is of opinion that the prevalence of typhoid fever among British soldiers freshly landed in India, has malaria (*impaludisme*) for one of its causes. According to this view the typhoid of malarial localities is a hybrid affection—in other words, the typho-malarial fever of some Indian and many American authors. I would observe on this point that the author bases this opinion very much on the fact that the morbid anatomy,

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in a case of typhoid fever, often presents the characteristic lesions of malarial as well as typhoid fever; the pigmented spleen and liver as well as the intestinal lesions of typhoid, but it is not always safe to draw the conclusion that both enteric and malarial poisons were operating on the organism at the same time, as the malarial lesions may be due to attacks of that peculiar fever antecedent to the attack of typhoid. At Netley we are familiar with the fact that, no matter what the disease may be which has proved fatal in an Indian invalid, we are almost certain to see evidence of previous malarial poisoning in the organism.

I cannot in this connection pass without notice the opinions of so high an authority as the illustrious Pettenkofer, of Munich, however much they are opposed to those of the highest authorities in this country and in India. At one time Pettenkofer believed in the water theory of transmission both of cholera and enteric fever. This belief he has now finally abandoned, apparently by the simple and easy process of throwing overboard the evidence so patiently accumulated by English observers of the highest reputation—a process, it appears to me, more convenient than convincing. As the views of Pettenkofer with regard to cholera and enteric fever are identical, the statement given in a paper read before the Medical Society of Munich on the 28th of April, 1880, on cholera and its relation to the parasitic theory, thus expresses them. He insists upon the localist origin of disease, which he thus defines:—"There is a germ of a nature unknown to us at present, but which is in all likelihood a minute vegetable organism, belonging to the order of fungi. This germ may be brought into a locality by the medium of persons or things, but the persons may be healthy or sick themselves, and the transportation of the germ in no way depends upon their actual attack of disease. The germ must then find a congenial soil, nutriment, or heat, for its development. This development is probably accompanied by the production of a third substance, just as alcohol is the result of fermentation, which is the active cause of an outbreak. Thus each case of disease is the result of the individual inhaling this substance from the emanation of the soil, and the case depends upon the quantity of the disease poison so injected. But this active poison is no more capable of reproducing itself or being communicated to others, than alcohol is capable of being reproduced, or drunkenness communicated by contact with an individual."

The congenial soil may be found in the individual, but Pettenkofer thinks it more likely to be in the ground. The conditions most favourable are, a previous high ground water in a porous soil; a dry season with a falling ground water, and high temperature of ground atmosphere.

This view is strongly upheld by Dr. Soyka, one of Pettenkofer's assistants, who denies the transmission of cholera or enteric fever by means of drinking water, or by the foul emanations from rivers, drains, cesspools, or the like.

Those who accept this theory, which, as I have said, is in Pettenkofer's estimation as applicable to enteric fever as it is to cholera, must follow the example of its eminent parent and shut their eyes to the immense accumulation of facts, not theories, collected in this country and in India, pointing unequivocally to what is now regarded in both countries as a fundamental principle in sanitation—viz., that the contagium of both diseases is in the excreta, after they have gone through certain putrefactive changes, and that this *contagium* can be, and has again and again been proved capable of being not only inhaled from a tainted air, but also of being carried into the system through the medium of drinking water.

When opinions so various and so opposed to each other are held by men of eminence as to the etiology of this disease, the consideration of means of prevention is difficult indeed. One thing is certain, no hope can be entertained that the soldiers sent

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to India are likely, save in exceptional cases, to be past the enteric fever age. If there be any truth in the main contention of this paper, that enteric fever, however its symptoms may be masked and complicated by the influence of another and all-pervading poison, is due in the main to the same cause as here—viz., the poisoning of air, water, or food, with some of the products of decomposed nitrogenous matter—the remedy in India must be the same as here, protection from such sources of impurity. How imperfectly this protection is afforded I need not say. One poisoned well may cause a dozen deaths in a small community before a suspicion arises as to the source of the mischief. Or an epidemic of milk typhoid may affect a fashionable quarter of London, supplied with milk from one tainted dairy—a fact requiring the aid of an exhaustive inquiry by a qualified expert to reveal. If this be so at home, let those who know India, with its fæcal sodden soil, its people, their customs and habits, and the nature of the water supply they use, say how difficult, how almost impossible, is the task of protecting our young soldiers from this destructive disease. My conviction is, that success in this direction will be the measure of our success in sanitary reform, not in barracks and their immediate surroundings only, but also in the places beyond such limits, a reform only to be achieved by efforts well directed by an enlightened and vigorous administration, and hardly to be looked for in the immediate future, or until the time comes when some knowledge of the laws of health has penetrated far below the slightly educated surface of native society.

In my evidence before the Royal Commission that inquired into the health of the army of India, I pressed on the Commissioners the importance of making a greater use of hill stations for young soldiers than was then the practice; that the proper use of such stations was to preserve health, not to cure disease. It unfortunately happened that when hill cantonments were first established sanitary precautions were neglected. It was thought that elevation above the heated plains, mountain air, and so on, would be enough; and that hygienic measures, the necessity for which on the plains was not denied, at least in words, might with safety be neglected. A fatal error, as now acknowledged. I do not say that hill stations alone, however well selected and cared for, will suffice entirely to protect our young soldiers in the first years of their Indian service from the disease that is so destructive to them; but I do say, that if, on first arrival, they are quartered on hill-stations properly selected, lodged in suitable houses, constructed on hygienic principles, and clothed in accordance with the demands of the climate and healthfully occupied, we may see a marked diminution in the prevalence of a disease that more than any other to which the human race is liable, may be classed among those that are distinctly preventible.

The Etiology of Enteric Fever.

Surgeon John Martin, A.M.D.

Gentlemen,—Some of you may remember that, four years ago, at the meeting of this Congress at Geneva, Dr. Bouchard laid before it certain conclusions he had arrived at respecting the etiology of typhoid or enteric fever. The views he put forward at that time were, briefly, that either of the three doctrines of (*a*) spontaneity, (*b*) infection, or (*c*) contagion, if taken separately, is unable to account for the varied phenomena of this disease: but that a combination of them is capable of giving a satisfactory explanation of these phenomena—that is, a belief in the occasional spontaneous origin of this disease, and at the same time in the occasional propagation of it.

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I do not purpose to answer here the objections of those who consider such a combination of opinions impossible, who believe that, if enteric fever is *ever* propagated, it must be in *every case* propagated and never originated, or the reverse; these objections have been, in the opinion of many of us, fully overborne. I would now merely take up this subject where Dr. Bouchard left it on the occasion referred to, and endeavour to lay before you some explanation of the manner in which putridity originates enteric fever in connection with the practical question of its prevention which is before us to-day.

Few can doubt that the phenomena of enteric fever mark this disease as an effect of septic poisoning. Nine years ago, at a debate on enteric fever at the Medical Society of Munich, Professor v. Gietl distinctly stated this as his opinion, which he upheld by strong evidence and arguments. A careful consideration of the phenomena of enteric fever, viewed as a form of septic poisoning, leads to the belief that the inauguration of this morbid process is due to, or determined by three factors—namely,

1.—Hepatic insufficiency.

2.—An abnormal functional activity of the intestinal glands, consequent on, and vicarious or supplemental to, this hepatic insufficiency; and,

3.—An idiosyncrasial proneness of these glands to assume this abnormal function.

On this view the steps of the enteric fever process, from its inauguration to its close, would seem to be somewhat as follows:—

The absorption of putrid matters, and their circulation in the venous blood; the partial and temporary elimination of this septic poison by the lungs, and subsequently its presence in the arterial blood when its amount becomes too great for elimination by the lungs; then the strain on the lymphatic system to eliminate the poison, and its failure to do so completely (this failure being the inauguration or determination of the specific enteric fever process), which results in an attempt at further elimination by the glands of the intestinal tract in the form of a suppurative enteritis; and the ultimate elimination here of the septic poison in a virulent form, or as bioplasm endowed with a specific modification of the vital process, and capable also of communicating this specific process to healthy blood bioplasm.

This seems to be the most satisfactory view of the nature of the enteric process, the initial disturbance being, according to it, an excess of work thrown on the hepatic system; and possibly such a disturbance, under certain circumstances, such as residence in tropical climates, may even, without the putrid element, initiate this enteric fever process. At first the increased demand on the hepatic function is replied to by an increased activity; but, as this demand on its activity goes on increasing, the liver seems unable to come up to time (so to speak), and is no longer able to bear the strain of eliminative work thus thown upon it. Coincidently with these conditions, certain of the intestinal glands take on an abnormal functional activity, and this new and abnormal function of the intestinal glands is believed to be vicarious or supplemental to, or consequent on, the hepatic functional insufficiency.

This view of the nature of the enteric fever process seems to explain, more satisfactorily than any other, the following groups of phenomena which go to make up the natural history of enteric fever:—1, the occasional spontaneous origin of enteric fever; 2, its occasional pythogenic origin; 3, its origination as a result of increased temperature; 4, its non-occurrence or rarity among natives of tropical climates; 5, its occurrence chiefly during the age of functional activity of the intestinal glands; 6, its very infrequent occurrence after the age at which these glands are known to atrophy; 7, the unmistakable symptoms of hepatic and splenic insufficiency which accompany it; 8, the true first stage of it, which is only an apparent period of incubation; 9, the occasional observance of bacteria in enteric fever blood, this being a result of the septic transformation of nitro-

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genous matter; and, 10, its occasional propagation by contagion, which is the original poison, or the patient's bioplasm in which the septic transformation is developed, and which is capable of communicating the same process to a healthy organism.

I would specially direct your attention to two of these groups of phenomena—namely (those above, 7 and 8), the hepatic insufficiency which mark the enteric fever process, and the way in which this pythogenic view of its causation explains the so-called "first stage," or "incubation period," of enteric fever. First, that there really does exist hepatic insufficiency in the later stages of this disease is apparent from clinical observation, more especially the condition of the urine and the body temperature.

The observations of Gubler, Jaccoud, Parrot, Robin, Sigmund, Vogel, and others go to prove that the proportion of urea is much increased during the early stage of enteric fever but that as the disease advances the amount of urea falls below the normal standard of health; and, according to Robin, there is, in fatal cases, an excessive diminution in its amount.

This points to the hepatic break-down I have described. The observations of Cloetta, Meissner, Bullard, Stokvis, Cyon and others show that urea is formed by the liver. M. Cyon, as you know, carefully examined the blood of the portal and hepatic veins, and found that blood, after passing through the liver, contained more urea than it did before entering that organ; and his experiments also demonstrated that blood which was three or four times passed through the liver contained more urea than that passed through only once. Then, again, the occurrence of albumen in enteric fever urine is explained by this incompetence of the liver to further eliminate urea, and the consequent elimination of the albuminous waste of the tissues by the kidneys as albumen and albuminose. The colouring matters give the same indications; urochrome, which occurs in excessive quantity in the early stage of this disease, indicates increased hepatic function, and the presence of hæmapheine and uroerythrine at the decline of the disease indicates (according to Gulber's observations) insufficient hepatic function.

The body temperature in enteric fever indicates the different stages of the disease no less definitely than the urine analysis. At the end of the third or beginning of the fourth week, the difference in the kind of temperature variation is well marked. This is the period when, according to the urine analysis, the hepatic insufficiency occurs, and the great differences between the morning and evening temperatures, and, altogether, the general likeness of the temperature of the fourth week of a severe case of enteric fever to that of the milder forms of pyæmia, taken with other clinical similarities of these diseases, cannot fail to suggest an etiological relationship between them. During the first three weeks we have the temperature of febrile disturbance, consequent on blood poisoning; and immediately following comes the peculiar temperature variation so characteristic of local suppurative inflammations, marking this enteric fever process as a suppurative enteritis: as closely allied to the family *metastatical dyscrasiæ*, in which the blood is altered, becoming abnormally coagulable by the presence of putrid animal substances.

Now, with regard to the so-called "incubation period" in enteric fever, the indefiniteness of this first stage is explained by the pythogenic view of its causation here advanced. The experiments of Claude Bernard on the results of injecting the volatile products of putrefaction into the systemic veins prove that such an indefinite period between the entrance of the poison and the exhibition of its results is absolutely necessary. In other words, that this indefiniteness of the incubation period is in itself, so far as it goes, a strong piece of evidence in favour of the pythogenic origin of enteric fever, of its being caused sometimes by the ingestion of putrescent organic matter. Bernard has pointed out that the poisonous effects of such putridinous matters are produced only when the toxic agent is brought into direct contact with the tissue elements by the

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arterial blood; now, when a volatile gas, such as sulphuretted hydrogen, is introduced into the systemic veins, whether by injection or osmosis, it will never enter the arterial system so long as the rate of its introduction does not exceed that of its elimination by the lungs. It is only when the gas enters the blood faster that it can be got rid of into the expired air that poison symptoms ensue, proportional to the amount of gas which penetrates into the left heart. In some cases this occurs sooner than in others and (may we not say *consequently?*) in some cases the specific process of enteric fever is set up sooner after the absorption of putrescence than in others—that is, the first stage of the disease (the incubation period as it is by some erroneously called) is indefinite.

My purpose in bringing this matter before you to-day is to point out how these theoretical considerations point to the same measures of prevention as those advocated by the most practical observers of enteric fever as it occurs in tropical climates. In summing up the results of the special inquiries for some years conducted in India, Surgeon-General Jones, C.B., reports to the Government of India that this disease "may develop itself in the European as the direct result of a consensus of causes, the chief of which seems to be the prolonged exposure of an alien and unripe constitution to an intensified and unaccustomed temperature, to which certain other more recondite climatic conditions may probably be added."

And the same officer, in the "Army Medical Report for 1878," says, with reference to enteric fever:—"What I would suggest is a practical experiment by way of testing the efficacy of hill stations on the health of newly arrived corps. Let a small station, such as Chakrata, for example, be selected, to which a regiment should at once proceed on its arrival at Bombay in the winter season; provide good water supply, and ensure that its purity is maintained, and see that the conservancy, drainage, washing, and cooking arrangements are practically as good as they can be; enlist the co-operation of the commanding and other officers in carrying out such experiment by explaining to them the object of it, and place the corps in medical charge of an officer specially selected for his intelligence and practical acquaintance with the fever in question."

The late Professor Parkes sought to impress the adoption of the same course. "If it were made a rule to send every fresh corps at once to the hills for two years after landing, it would probably be the means of saving many lives. The advantages of the hills are not merely the avoidance of malaria and the excessive high temperature of sea-level or inland plains, but the fact that exercise can be freely taken in a temperature not so very different from England." ("Hygiene.") Time does not permit a further consideration of the subject, but those who esteem the views I have advanced as important in their bearings on the prevention of enteric fever will find the subject treated in detail in the first volume of my "Contributions to Military and State Medicine," published by Messrs. Churchill, London.

DISCUSSION.

Deputy Surgeon General NORMAN CHEVERS, C.B., London: gave facts towards proving that true enteric fever has been of gradually progressive prevalence during the last forty or fifty years in Europe, and during the last thirty-five years in India, that the disease is everywhere of atmospheric, probably of epidemic origin; and that the introduction of a poison, by milk, water, &c., by the mouth is to be regarded only as one of the most prevalent of its *exciting* causes.

Surgeon BILLINGS, U.S.A.: remarked that, for medical officers of the army, the practical question as regards typhoid fever is, how to prevent it. For this purpose we must consider probabilities, seeing that there is no certainty as yet as to the nature of the disease.

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It is probable that many cases of typhoid fever are due to something which has been derived from the body of a person affected with the disease, and that this something in most cases is conveyed by means of water or food. Upon this probability are to be based the methods of prevention of the disease. The term "epidemic condition" is simply another name for total ignorance. Dr. Billings asked whether any of the medical officers present who had seen service in India had observed cases of what is known in America as the typho-malarial fever.

Surgeon Major J. B. SCRIVEN, Lahore: confined himself to remarks on points that had been raised in the papers already read. He could vouch for the disease being as old as the year 1851. He thought post-mortem examinations before that time were often imperfectly conducted. As to its affinity with malarious fever, he had found that persons weakened by malarious fever were particularly liable to typhoid. Intermittent also often supervenes during convalescence from typhoid. Enlarged spleen is common to both diseases. With reference to causation, Mr. Scriven had in India generally found the cause impossible to trace, but he had accepted what had been satisfactorily proved in England—viz., that in most cases the poison was conveyed through the mouth, and he thought it unlikely that the most frequent cause in England should have no effect in India. He did not, however, exclude communication through the air. His practice had of late years been civil practice, and he had observed that enteric fever in the Punjab did not occur in outbreaks affecting several persons, but in isolated cases. The cause of this was probably the system of filtering drinking water through sand picked up promiscuously; the supposed purifying process thus became really the mode of contamination. He knew of one case in which enteric fever had rapidly supervened on exposure to the effluvia from an exhumed body in a state of advanced decomposition. He only remembered direct communication from person to person in two instances, both instances being sisters. In one case the sisters had certainly slept together, in the other, probably so. It has been said that natives are not very liable to typhoid, but this is not proved. Mr. Scriven had seen cases in natives; Dr. Ewart has published some. Mr. Scriven had attended many cases in half castes. Hill stations he considered most useful in preserving the health of European soldiers, but the causes of typhoid exist there in abundance. The water is polluted by flowing over the sides of the hills, and is collected in reservoirs below. He thought the disease would be most effectively prevented by furnishing pure water not only to barracks but throughout the stations where European troops are located, as their drinking is not confined to barracks. Also he thought that whatever tended to promote temperance among soldiers would also promote immunity from typhoid; for alcohol is the great cause of thirst, and the more alcohol they imbibe the more water they require, and the more promiscuously they will drink.

Inspector-General J. D. MACDONALD, F.R.S., R.N., Netley: spoke of air and water as media of communication, referring to the case of Eastney barracks as proof of the former, and that of Haslar barracks as illustrating the latter. 1. In reference to Eastney barracks cases constantly occurred every autumn until it was found necessary to investigate their cause and suggest a remedy. Professor de Chaumont and myself recommended the proper ventilation of the drainage system, and especially the provision for the free escape of refuse gas, and with complete success. 2. In the case of Haslar barracks about 70 cases had been sent for treatment to Haslar Hospital, and it was clearly proved that they had been drinking water from an old well, with which the latrine was in immediate communication. The remedy was found by closing the foul well and using the pure one which had been newly opened for general use.

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Sir J. FAYRER, London: said that the existence of enteric fever—the same disease as that of England, whatever may be its cause—in India, was not to be doubted; but that all the cases of continued fever in India or the tropics, with enteric complications, diarrhœa, a certain range of temperature, &c. &c., were due to a specific poison, generated in the bowels of one individual, and transmitted through water, air, or other channel to the bowels of another, and so on to others, he could not, and did not believe; that there was a large amount of continued fever in India, often severe and even fatal, attended with the phenomena of the enteric fever of England, but that it is not due to fæcal poisoning, and that to attribute it to such a cause alone would in his opinion be erroneous and dangerous; that there is no possibility at present of dogmatizing on the causation; that due allowance has not been made for geographical position and climatic influences; heat, moisture, organic decomposition, miasmata, and a variety of aerial and telluric conditions are to be considered, and that in such, more probably than in a specific poison developed in fæcal matter, were the causes of continued fever in India and the tropics, and subtropical regions, to be sought for; that it is stating more than we can prove, to assert that the phenomena and pathological changes seen in enteric fever, even as it occurs in this country, are due to one specific poison only. The causation, as far as other climates are concerned, must be looked for in other directions than in the drain, cesspool, and water-closet; in general and not in specific causes alone. Freely admitting, he repeated, that the disease may in some cases arise in India as it does in England, he had heard Mr. Martin's paper with great interest, as also the remarks by Dr. Chevers and Dr. Billings. The discussion was most important and interesting, and would do much good, as it would direct attention again to the necessity for reconsidering the question of the etiology of Indian and tropical continued fevers, and it was interesting to notice how many observers now were recognizing other explanations of this form of fever than the specific fæcal theory. As to malarial influences (an expression of ignorance) in remittent and low forms of fever, it was often very difficult to say to what exact type of fever certain cases should be referred; and in a country where malaria, whatever that may be, prevails universally, it is only too probable that any or all cases of disease may be modified by its operation. He would repeat that to refer all the cases of continued fever with enteric complications in India to one specific origin was a dangerous theory. Every one recognized the importance of pure water, good drainage, and ventilation, and sanitation generally; so far there was no difference of opinion. With reference to the case of Eastney barracks no one could doubt that all Dr. Macdonald said on the subject was of the highest importance, but to argue from that to India was dangerous. The conditions were different; fevers occurred where no such conditions as those described at Eastney existed; and, on the other hand, it would not be difficult to find instances where the state of things was even infinitely worse, and yet fever was not the result.

Professor MACLEAN, Netley: Before this discussion closes I wish to make a remark on what fell from my friend, Dr. Norman Chevers, on the subject of atmospheric waves. He tells us that typhus was seen in almost every wave, and that such is not common now. When I was a student in Edinburgh typhus raged. I could reckon up at least a dozen cases of my fellow students who perished from it. That disease is unknown now in Edinburgh, or nearly so. Why? Is it due to the absence of any atmospheric wave? No; it is due entirely to the great sanitary movement that has taken place in that city. Professor Maclean then gave some instances from his Netley experience, to the effect that the disease has never spread in that hospital, where care is taken not to allow the stools of those affected to pass into the drainage of the house; and that, on the

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other hand, in a house near to the hospital, where no such care was taken, the disease spread in the house and caused three deaths.

Deputy Inspector General EWART, Brighton: said he had always recognized the fact that, in malarious localities, typhoid fever was greatly modified by the existence of malarious poisoning of the system. This compound might be fairly regarded as the typho-malarial fever of America In India—and probably elsewhere—the cases are frequently ushered in by paroxysms of ague, and analogous attacks sometimes supervene during the protracted period of convalescence. These complications induced the older authors to include the remittents of tropical and tropoidal regions with bowel complaint in the malarious group of diseases. Indeed, even now, with all our additional advantages of an improved pathology and the introduction of thermometry, the diagnosis is attended with much difficulty until the disease has advanced. He prefers the old name of typhoid, or the new one of enteric fever, and looks upon the malarial element simply as a complication, important no doubt, but not sufficiently so to warrant its being raised in our nomenclature to an equal share of causation as might be implied by the adoption of the term typho-malarial fever. As to the question of etiology, the true position of climatic influences is that of a predisponent. Of their widespread operation there cannot be much doubt. They probably stand in a somewhat similar relation to the infectious and contagious diseases as they palpably do to animated nature generally. But that these influences, potent as they are, can generate enteric fever, is just as reasonable as that they can singly give origin—in the absence of the specific germs or seeds—to plants or animal organisms. It frequently happens that the rose-coloured spots so common and characteristic in temperate latitudes are altogether unnoticed in India. At first he thought that such cases might not be true enteric fever. He had, however, had the opportunity of examining the ilium in two or three cases of this kind, and had found the pathognomonic ulceration of Peyer's patches. Another point of difficulty is the modified temperature caused by malarious poisoning, giving a much greater expression to the remittent character of the disease. During these remissions the perspiration is more copious than in those met with in cases in cold and unmalarious countries. It is true that the disease has been most common among young European soldiers in India. But most of the troops are made up of men included in the ages at which enteric fever is witnessed in all countries and among all races where it has been proved to prevail. He had noted its greatest prevalence among youths of both sexes in Europeans, Eurasians, and natives. He would suggest an inquiry as to whether the disease was more frequent among the young soldiers serving in India than among their brethren of the same ages in England and the non-malarious colonies.

Inspector General LAWSON, London: When examining the records in the principal medical officer's office at the Cape, I found the description of several fatal cases of true typhoid in 1827. This description gave a most characteristic detail of the affection of the Peyer patches at the lower part of the ilium, and further the frequent occurrence of perforation of the intestines and peritonitis was remarked on. As to the so-called hybrid fever of malarial and enteric form in Jamaica, the military were always subject to malarial fever; and enteric fever was not a very rare one, but when it did appear, the resulting fever presented the hybrid appearance. The thermometer was not then in use there, so I cannot answer Dr. Billings' remark on that point, but the cases were distinctly intermittent for about a week, when diarrhœa appeared; the fever then became more continued, and after death the usual lesion was found in the ilium. The occurrence to which I more particularly refer was this: there were four

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barrack buildings in a line, each of them with a large latrine behind it, and one of these latrines was completely cleared out after being for many years but partially emptied, and shortly after the cases of this fever occurred among the men occupying the adjoining barrack. Some ten cases occurred among them. As soon as I procured this information, I had the men removed from the building, and the form of fever ceased, no other of the buildings having been affected. The water employed was from the same source for all, and was obtained from the works for the supply of Kingston. As to the inquiry by Dr. Chevers of there being a pandemic factor concerned, I am not able to give any specific facts on this subject. When I mention, however, that syphilis is largely affected by such an influence, as I will bring more particularly under your notice on Monday, it would be rash to say that enteric fever may not be.

Surgeon General HUNTER, London: It is difficult to conceive that the prevalence of enteric fever in India should be open to doubt, so overwhelming, in my opinion, is the evidence of its existence in that country. Typical specimens of its morbid anatomy are to be found in the museum of the Medical College in Bombay. The first case observed in Bombay occurred as far back as 1860–61. It occurred in a European, an assistant to a chemist. It was seen and attended by Dr. Peet, the then Principal of the Medical College, and myself. Since, numerous examples have been noticed, and the disease is found to be common, and has taken its place in the returns from all hospitals in the Presidency, whether military or civil. True typical cases come from time to time under observation, but are less frequently noticed than in this country. Malaria, that ever prevalent disorder, often masks and modifies its course, not in enteric fever alone, however, but in almost every disease which falls under observation. I do not know a more difficult matter of diagnosis than to differentiate a low remittent with typhoid symptoms, more especially if diarrhœa be present, and all history absent, as only too frequently happens, unfortunately, among the civil population in Bombay, from enteric fever. Often the only solution of the difficulty is to be found in the post-mortem room. I am inclined to the opinion that the form of low remittents I have just referred to is the same as the typho-malarial fever to which reference has been made by Surgeon Billings, as occurring in the United States. No race or sect is exempt. I have witnessed the disease in Europeans, Eurasians, Hindoos, Mussulmans, Parsees, Portuguese, and Jews. That filth is a cause of enteric fever is no more doubted in India than here. Much evidence in proof of this might be adduced if necessary, but it is needless to take up the time of the section by detailing it. Whether, however, it is the only cause, at least in India, seems to admit of doubt. Careful inquiry by competent observers has failed to trace outbreaks of the disease to filth. Were filth alone the cause, then in the grossly defective sanitary condition of Bombay, for example, enteric fever should be far more prevalent than it is. In the absence, therefore, of this cause, some other must be looked for. Heat has been advanced as a cause; so it is, but not in the sense its advocates would have us understand. The concurrence of outbreaks of the disease with a high temperature is rather, I think, to be attributed to the greater activity of the processes which give origin to the infective material.

Surgeon Major EVATT, Woolwich: An important item in the question of the character of the fevers in India is the careful thermometrical observations and the careful post-mortem observations made now-a-days. In the old days no thermometers were used, to-day they are; and post-mortems are now made very carefully indeed, and with special reference to the condition of the bowels. I consider that the fact of these fevers increasing so much in the hot season points very strongly to the necessity of sending the young

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soldiers at once from the troopship to the mountains. The young lads like the hills, the old soldiers dislike them; and on the hills the chances of this fatal fever are far less than in the plains for the young soldier. I am myself led to believe that heat and malaria develop a fever of a fatal form, and that in these cases the filth origin of the fever has not been clearly shown, although I do not doubt that filth fever can originate in India, as in any other country in the world.

Surgeon General MOUAT, V.C., C.B., London: It was my fate to commence my career in the medical service forty years ago in the presanitary days, long before the introduction of modern methods of diagnosis, at two of the most unhealthy stations in the whole of India —Bellary and Secunderabad. In India there are a few well-marked diseases; you may count them on the fingers of one hand: fevers, hepatic diseases, dysentery, and cholera. The former we generally (except in well-marked instances of intermission) returned as common continued fever. On looking at these drawings, I am satisfied that a large proportion of the fatal cases from dysentery would now be returned as from enteric fever, which, on account of the predominance of the dysenteric symptoms, were returned under that head. I am now also quite satisfied that enteric fever is not a new, but a very old disease imperfectly diagnosed. A well-marked case occurred in my practice, as far back as 1840, in a young officer of the 38th Regiment, which commenced as an ordinary attack of fever, ending in peritonitis, with perforation of the bowel. As there was a suspicion of poisoning in this case, I made a most careful post-mortem examination, and found the now well-known characteristic morbid signs of enteric fever. There was no eruption. With regard to the liability of young soldiers, which is a principal point insisted on by the author, they are no more liable to enteric fever in India than in Europe, further than young men on arriving in India at the hot season are more liable to all climatic diseases than the older and more seasoned. With regard to the cause of this disease, dismissing all speculative theories, I think there was at that time quite enough to account for it, more especially at the stations to which I have alluded, in the unsanitary state of the barracks and vicinity, so well known and described by Dr. Maclean, who, probably, was not aware of a very conclusive experiment made and carried out at my suggestion, a out the time when Dr. Maclean was at Hyderabad—viz., the removal of a strong company of the regiment to another barrack, two miles distant, on a slightly elevated plateau. The result at the end of twelve months was a mortality of 1 per cent., as compared to a previous one of 9 in the company and 7 in the regiment generally. This determined the Government to move the barracks, or rather to build new ones on the race course, with the best results. I believe the true cause of enteric fever to be pythogenic, transmissible by both air and water, but chiefly the latter, and so far capable of removal.

Dr. BECHER, London: remarked that his experience in China in 1857 proved that at Hong-Kong no cases of enteric fever came under his observation, although he made all post-mortem examinations, but after the occupation of Canton cases of typhoid appeared, first isolated, gradually increasing in numbers, till it might be called an epidemic. These cases—although the disease is one the post-mortem appearances of which are so well defined that it cannot be doubted when one has a case before one's eyes, provided the examination is made carefully—were not admitted by the principal medical officer to be cases of enteric fever, on the ground of his 15 years' experience in the tropics (India), according to which this disease did not exist in India! This example will show that the disease may have been prevalent in the tropics at previous times, but, owing to imperfect observation, it was not recognized. Post-mortem examinations I consider as alone decisive on the question. As to India, I have not had much experience;

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but one case occurred at Fort Gwalior, imported from outside by a young soldier, followed by another in the same room after some six weeks, both terminating fatally, but the carefully prepared specimens unfortunately perished, otherwise they would have proved the existence of the disease at that time (1864). A short remark as to prevalence of the disease in armies: in the Crimean War true enteric fever did not appear at the Scutari hospitals until the spring of 1856 (when it was introduced from the Crimea), although numerous cases had died from other fevers previously. The nature of this fever (in answer to Sir J. Fayrer) I cannot define—it was a shorter, more rapid fever, with vascular eruption of skin, enlargement of spleen, congestion of small intestines, and slight intumescence of patches of isolated glands. Similar observations I made at Versailles during the Franco-German War, where enteric fever also appeared late.

Surgeon ANDREW DUNCAN, Bengal: I rise to corroborate the statements made by Sir Joseph Fayrer, in stating that typhoid fever in sub-tropical climates does not arise solely from filth causes. During the war in Afghanistan I served in the Kurram valley during the first winter; we had no cases of typhoid in the winter, although the sanitary conditions were not then favourable. Directly the hot weather came, when the sanitary arrangements had become better, typhoid fever appeared. The same occurred, in a more marked degree, on the Candahar line. There, during the cold weather, there were very few cases. Directly the hot weather began, typhoid fever became rife. I myself had returned in a rude state of health from New Zealand, and was hastening up to join the army. I filtered my water, but was exposed to much heat; after marching for three weeks I was seized with typhoid. All these facts seem to me to show that there is some cause other than filth capable of giving rise to typhoid in India. Another point I wish to notice is that, as far as my experience goes, there is a more frequent absence of rose-coloured spots in India than in England. That the cases were really those of typhoid, although with an absence of spots, was proved by the post-mortem examination revealing the characteristic lesions. The typhoid fever in India frequently begins like remittent or intermittent. In fact, some of the cases were diagnosed as such, until two cases of sudden death occurred, and perforation of Peyer's patches was found.

The PRESIDENT closed the discussion by briefly summarizing the several points which had been advanced by the respective speakers.

On the Influence of the Contagious Diseases Acts on the prevalence of Venereal Affections among the Troops serving in the United Kingdom.

Inspector-General ROBERT LAWSON, London.

In 1859 the admissions to hospital for the various forms of venereal affections among the troops serving in the United Kingdom was 422 per 1,000 of strength, and the number constantly sick 26·8 per 1000. In 1860 the admissions were 369 per 1000, and the constantly sick 23·7; and in 1861 the corresponding ratios were 354 and 23·5. These enormous admission rates, leading as they did to the immediate loss of service of about every fortieth man, drew the attention of the authorities to the vast importance of affording the females with whom the troops associated, when diseased, hospital treatment until cured; and an Act of Parliament, sanctioning this, was passed in 1864. Another Act of Parliament was passed in 1866, which sanctioned the personal examination of the women practising prostitution within given areas, and gave power to retain them in hospital for six months. In 1869 a third Act was passed enlarging the areas

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within which prostitutes were liable to examination, and extending the period a woman might be detained in hospital if necessary for her cure. From 1866 to 1869, the measures sanctioned by the existing Act were being gradually applied, but it is only from 1870 that the full effect of the Act of 1869 is met with at all the stations under it. This state of things continued until the end of 1873, when an order, issued by Lord Cardwell, stopping the pay of soldiers under treatment for primary venereal sores, and gonorrhœa, led to much concealment of both forms of disease, and from that date, until the end of 1879, when it was set aside, the returns show a considerably smaller number of admissions for these diseases than there should have been, had all those affected reported themselves.

An examination of the returns of the health of the army serving in the United Kingdom, from 1830 to 1847, brings out the fact that there was great fluctuation in the frequency of venereal sores, presenting periods of maximum and minimum every few years. The returns for the troops from 1860 to 1878 present the same features, which are further borne out by the Registrar-General's returns of deaths from syphilis among the civil population in England and Wales; both show minima in 1866 and 1871, with maxima in 1868-9 and in 1875-6 (the last of secondary disease among the troops). These fluctuations are very remarkable, they show that venereal affections, like small-pox or measles, are subject to an epidemic element largely influencing their prevalence, and this seems—so far as the evidence at present available permits it to be traced—to show itself in a series of waves, which become sensible in the south of England up to a line from the Wash on the east coast to the Bristol Channel on the west, in the first year, and from that to a line from the mouth of the Tees to Morecambe Bay, in the second, and from that to a line from the Forth to the Clyde in Scotland, in the third. Traces of the same waves are found in Ireland, but are less distinctly marked. From all this, however, it is obvious that there is a great variation in the incidence of venereal disease over the country, from causes of which we have no cognizance, and, in any attempt to estimate the efficiency of restrictive legislation in reducing its prevalence, the difference in the incidence of the disease for the periods under comparison must be eliminated and the residue only attributed to the influence of the repressive measures employed.

An objection has been made to the grouping of all venereal sores in the Army Medical Returns under the head of "Primary Venereal Sores." This has proceeded from men in civil practice. Military surgeons, on the contrary, maintain there are no inconsiderable number followed by constitutional disease which they could not distinguish from non-infecting sores, until some trace of the implication of the system presented itself, and which, therefore, it was quite impossible for them to designate correctly within the time when their returns had to be made up. Under these circumstances, had the Army Medical Department attempted to separate the infecting from the non-infecting sores in their returns, they clearly would have introduced an error of uncertain amount which would have marred their value as statistical data, and there was no choice but to include both descriptions of sores under the general term of "Primary Venereal Sores," leaving it open for the infecting portion of the group being estimated by the number of cases of secondary syphilis which followed.

Variations in the incidence of venereal disease over the country have been alluded to already, but there is another variation of a very striking description, which may occur among different corps occupying contiguous barracks at the same station, at the same time, and all under the same general conditions. At Aldershot it became necessary for me to examine into the prevalence of primary venereal affections in four regiments quartered in the permanent barracks close to the town. In one of these bodies the

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ratio of admissions for primary venereal sores was 124 per 1,000 of strength per annum, in another in the next barrack the ratio was 42 only, in the regiment next to that it was 84, and in the fourth, in the adjacent barrack on the other side, it was 142. In the north camp, two miles off, and at the same time, one regiment had a ratio of admissions for primary venereal sores of 114 per 1,000, the second had one of 59, and the third one of 23 only. These facts show that extreme caution must be exercised in drawing inferences from returns from small bodies of men, or from small stations, and that trustworthy results can be obtained only by using as large numbers as can be obtained, and from as many different points as possible, so that the deviations from the mean in one direction at some may be neutralized, as far as can be, by deviations from it, in the opposite direction, at others.

To arrive at the influence of the measures taken under the Acts, the stations at which these were ultimately applied, 14 in number, were thrown into one group, and 14 other stations, each of which had a mean annual strength of 500 or upwards, at which no restrictive measures were enforced, were used to show the varying incidence of the disease over the country. Subsequently the whole of the stations in the United Kingdom which did not come under the Acts were substituted for the 14 selected stations; these give a wider basis for estimating the variations of the disease over the country than the 14 selected stations, though with lower ratios of prevalence, but every fluctuation observed at the 14 selected stations between 1860 and 1878, with one slight exception, were distinct in the more extensive group. The strength, with the admissions to hospital for primary venereal sores and gonorrhœa, with their ratios per 1,000 of strength, at these two groups, for each year from 1860 to 1878, are given in Table I.

TABLE I.

Showing the Strength and Admissions for Primary Venereal Sores and Gonorrhœa in the Stations which came under the Acts, and at all the remaining Stations never under the Acts.

Years	Stations which came under Acts.					All Stations never under Acts.				
	Strength	Admissions for		Ratios per 1000		Strength	Admissions for		Ratios per 1000	
		Primary Venereal Sores	Gonorrhœa	Primary Venereal Sores	Gonorrhœa		Primary Venereal Sores	Gonorrhœa	Primary Venereal Sores	Gonorrhœa
1860	57479	8405	7966	146	139	40224	5291	4998	132	132
1861	51328	7267	7133	142	139	37627	4577	4063	122	108
1862	45322	5314	6283	117	139	32851	3264	3925	99	120
1863	43419	4653	5202	107	120	32526	3521	3638	108	112
1864	40694	4135	4803	102	118	32558	3297	3252	101	100
1865	43078	4077	4937	95	115	29921	2956	3373	99	113
1866	39476	3444	4573	87	116	30816	2594	2993	84	97
1867	39911	3640	5274	91	132	33509	3367	3946	101	118
1868	42595	3533	5685	83	133	35666	3370	4061	95	114
1869	42017	2765	4466	66	106	31747	3366	3431	106	108
1870	41580	2268	4081	55	98	33734	3134	3195	93	95
1871	54096	2763	6254	51	116	38571	3122	3969	81	103
1872	50794	2752	5280	54	104	41424	3736	4023	90	97
1873	48039	2420	3946	50	82	40918	3307	3508	81	86
1874	48136	2039	2968	42	62	38701	2555	2596	66	67
1875	48606	1717	2825	35	58	39541	2324	2488	59	63
1876	48620	1622	3302	33	68	38073	2416	2642	64	69
1877	52422	1809	3585	35	68	39721	2690	3376	68	85
1878	55813	2235	4348	40	78	45316	3970	3144	88	69
Sums										
1861-66	263317	28890	32931	109·7	125·1	196299	20209	21244	103·0	108·2
1867-72	270993	17721	31040	65·4	114·5	214651	20095	22625	93·6	105·4
1873-78	301636	11842	20974	39·3	69·5	242270	17262	17754	71·2	73·3
1860-63	19 7548	25639	26584	129·8	134·6	143228	16653	16624	116·3	116·1
1870-73	194509	10203	19561	52·5	100·6	154647	13299	14695	86·0	95·0

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To admit of comparison the means of the numbers in this table have been taken for three periods of six years, 1861–66, 1867–72, and 1873–78; during the first scarcely any measures of repression were employed at the stations which subsequently came under the Acts, and the disease may be said to have had fair action at these as well as at those which never were under them; during the second, the Acts were gradually applied for the first three years, and were only in full operation during the last three; in the third period Lord Cardwell's order, which came into force late in October, 1873, deranged the returns for both groups, and they no longer show the true course of primary sores or gonorrhœa. Two other periods, of four years each, are given, 1860–63, before any restrictive measures were taken, and 1870–73, when these were in full force at the 14 stations which came under them, from which a fair idea of the full operation of the Acts may be obtained.

Taking the six yearly periods, it is found that the ratio of admissions for primary venereal sores for 1861–66, at the stations never under the Acts, was 103·0 per 1,000, that for 1867–72 was 93·6, a fall of 9·4, or 9 per cent. of the former ratio, indicating a reduction in the incidence of the disease over the country generally to that extent. At the stations which came under the Acts the ratio in 1861–66 was 109·7, and in 1867–72 65·4 only, showing a fall of 44·3, or 40 per cent. of the original ratio, and if this be diminished by 9, the reduced incidence of the disease, 31 per cent. remains as fairly due to the action of the means of prevention enforced at them, even when these were but partially applied. Turning now to the four yearly periods, at the stations never under the Acts, for 1861–63, the ratio for admissions for primary venereal sores was 116·3 per 1,000, and for 1870–73, when the incidence of the disease over the country was unusually low, it had fallen to 86·0, a decrease of 30·3, or 26 per cent., while at the stations which came under the Acts the ratio in the first period was 129·8, and in the last 52·5, a fall of 77·3, or 60 per cent., and deducting 26 for the reduced incidence of the disease the remainder, 34 per cent., shows the influence of the Acts when fully in force. Had the incidence of the disease been greater from 1870 to 1873 there is no reason to doubt that the stations under the Acts would have shown a considerably larger percentage of saving.

Turning now to gonorrhœa, at the stations never under the Acts, the ratio of admissions per 1,000 for 1861–66 was 109·2, and for 1867–72, 105·4, a fall of 2·8, or about 3 per cent., while at the stations under the Acts the ratio for the former period was 125·1, and for the latter 114·5, showing a decrease of 11·6 or 9 per cent., an excess in favour of the Acts of 6 per cent. At the stations never under the Acts, for 1860-63 the ratio of admissions was 116·1, and for 1870–73 it had fallen to 95·0, a diminution of 21·1 or 18 per cent., while at the stations under the Acts the ratio, which in the former period was 134·6, fell to 100·6 in the latter, a decrease of 34·0, or 25 per cent., showing an excess of 7 per cent. in favour of the Acts.

It is to be observed that the above results are obtained from the Returns for the stations under the Acts as they stand, but as these embrace a large number of cases of disease, which, though they came under treatment at these stations for the first time, were nevertheless ascertained to have been contracted elsewhere, such cases should be eliminated to admit of the full capability of the Acts for the repression of the disease being ascertained. Thus at Aldershot, from April, 1868, to 31st of December, 1871, the ratio of admissions for primary venereal sores was 66·8 per 1,000, but of these 18·8 per 1,000 were imported cases; deducting these from the observed ratio, and adding to the remainder one-fortieth for men who, on the average, were absent on furlough and otherwise, who had they been present would have presumably contracted disease at the same rate as their comrades, and again adding one twenty-sixth for men who had contracted the disease at Aldershot, but in whom it was not developed before leaving, the admission

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rate for that place is found to have been 51·1 per 1,000 only for the period. Similarly with gonorrhœa, the ratio of admission was 95·2 for the same period, and the imported cases were 23·4 per 1,000, and making the necessary corrections as in the previous case 71·8 per 1,000 is obtained as the ratio for gonorrhœa actually contracted at Aldershot.

The influence of the measures, taken under the Act, on the prevalence of constitutional syphilis, may now be examined. In 1859 and 1860 there seems to have been an unusual number of non-infecting sores met with, as in the whole force at home there were 4·81 cases of primary venereal sores to one case of secondary syphilis in the former year, and 4·18 in the latter; these diminished considerably in 1861, when there were only 3·72 cases of primary sores to one secondary case. From 1861 to 1872 inclusive, the admissions for primary venereal sores in the whole force at home were 86,915, and for secondary syphilis 28,807, being as 3 primary to ·99 secondary, or, to avoid fractions, for every 100 cases of primary venereal sores 33 cases of secondary syphilis were met with, and if these twelve years be divided into four portions of three each, the same proportion is maintained very closely, thus:—

	Primary Venereal Sores.	Secondary Syphilis.	Proportion of Secondary Cases to 100 Primary.
1861–63	28,596 ...	8,927 ...	31
1864–66	20,503 ...	6,937 ...	34
1867–69	20,041 ...	6,820 ...	34
1870–72	17,775 ...	6,123 ...	34

So that during these twelve years the secondary syphilis may be said to have occurred in exact proportion to the primary sores, notwithstanding the great diminution in the number of the latter, and the relative frequency of the infecting and non-infecting sores must have remained pretty constant; this analysis cannot be extended beyond 1872, as the stoppage of pay from the end of 1873 so interfered with the admission of primary venereal sores as to destroy the trustworthiness of any comparison based on them.

As is well known from one to twelve months, or more, may elapse between the date of a man's having contracted a primary venereal sore, and the appearance of the secondary disease, and during this period he may have left the station where he contracted the sore and have gone to another where the constitutional affection came under treatment; that this takes place to no inconsiderable extent there is no doubt, but the returns do not enable such cases to be referred back to the stations where the primary disease was contracted. With two groups of stations, one of which is not under the Acts, and has a high admission rate for primary venereal sores, and the other, under them, has a much smaller rate—between which frequent transfers of troops take place—the practical result is that much secondary syphilis becomes developed at the stations under the Acts due to primary disease contracted at stations never under them, while, at the latter, a smaller amount appears due to the lesser prevalence of the primary affection at the former, thus unduly lowering the ratio of the disease at the stations not under the Acts, and raising it at those which are under them. Notwithstanding this, however, the returns show a much greater reduction of secondary disease at stations under the Acts than at those not under them, as will be obvious from the following Table II.

As Lord Cardwell's order did not extend stoppage of pay to constitutional syphilis, the figures in this table, from 1873 to 1878, may be received with equal confidence as those for the other periods. It is obvious that while at the stations never under the Acts there was but little alteration in the three six-yearly periods, amounting to 2 per cent. only between the first and last, there was a great and progressive deduction in the ratio at the stations under the Acts as the primary sores at them diminished, ultimately reaching 41 per cent.; and in the two four-yearly periods, though there was a greater fall at

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TABLE II.

Periods	Stations which came under Acts. Secondary Syphilis		All Stations never under Acts. Secondary Syphilis	
	Admissions	Ratio per 1000	Admissions	Ratio per 1000
1861–66	9838	37·4	6026	30·7
1867–72	6677	24·6	6266	29·2
1873–78	6631	22·0	7317	30·2
1860–63	7897	40·0	4362	30·5
1870–73	3942	20·3	4246	27·5

the stations never under the Acts amounting to 10 per cent. of the first ratio, the fall at the stations under the Acts was 49 per cent. of the ratio in 1860-61, being still in excess of that at the other group by 39 per cent.

The saving of men's time effected by the smaller number of attacks of primary disease under the Acts, and the reduction of those constantly sick with these, and their consequences, and the resulting constitutional disease, may now be considered. Here however, as with the admissions for secondary syphilis, the numbers at the stations under the Acts are unduly raised after these were fully in operation by cases from the stations never under them, and at these, on the contrary, the numbers constantly sick are depressed, thus diminishing the real saving that would have been shown could these sources of disturbance be eliminated. The following Table III. gives the numbers daily in hospital for primary venereal sores, secondary syphilis, and gonorrhœa *et sequelæ*, including under the latter balanitis, orchitis, stricture of urethra, and bubo not connected with a sore :—

TABLE III.

Period	Stations which came under Acts. Constantly sick with			Ratio per 1000 constantly sick with				All Stations never under Acts. Constantly sick with			Ratio per 1000 constantly sick with			
	Primary Venereal Sores	Secondary Syphilis	Gonorrhœa et Sequelæ	Primary Venereal Sores	Secondary Syphilis	Gonorrhœa et Sequelæ	Sums of Ratios or actual daily loss of service	Primary Venereal Sores	Secondary Syphilis	Gonorrhœa et Sequelæ	Primary Venereal Sores	Secondary Syphilis	Gonorrhœa et Sequelæ	Sums of Ratios or actual daily loss of service
1861–66	2097	730	2748	7·96	2·77	10·44	21·17	1467	496	1640	7·47	2·53	8·35	18·35
1867–72	1396	504	1852	5·15	1·86	6·83	13·84	1603	471	1241	7·47	2·19	5·78	15·44
1873–78	910	538	1154	3·02	1·78	3·83	8·63	1258	524	809	5·19	2·16	3·34	10·69
1860–63	1834	582	2328	9·28	2·95	11·78	24·01	1161	374	1293	8·11	2·61	9·03	19·75
1870–73	845	316	1041	4·34	1·62	5·35	11·31	1093	323	706	7·07	2·09	4·57	13·73

To apply this table, the sum of the ratios at the stations never under the acts for a period before these came into force is to be subtracted from the sum of the ratios for the same period at the stations which came under them, and the difference, divided by the first mentioned sum, gives the fraction of it by which it differs from the second sum, As the sum of the ratios at the stations never under the Acts, for any other period, is taken to represent the incidence of the disease over the country for that period, to

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render it comparable with the corresponding sum under the acts it must be increased by a portion of its amount indicated by the fraction just mentioned, and on subtracting the sum of the ratios at the stations under the acts from this the actual saving of time is obtained. Thus for the period 1861–66 18·35 being subtracted from 21·17 leaves 2·82 and $\frac{2 \cdot 82}{18 \cdot 35}$ equal to $\frac{1}{6 \cdot 5}$, which is the fraction of 15·44 the sum for 1867–72, at the stations never under the Acts, which is to be added to it to make it comparable with the stations under the acts; this fraction is 2·38, which added to 15·44 makes 17·82, and the sum at the stations under the Acts taken from this leaves 3·98 as the daily saving of service per 1000 men under the Acts during this transition period. Similarly, using the two four-yearly periods, the saving of time is found to be 5·40 for the four years 1870–73, when the Acts were fully in force; the incidence of the disease over the country during this period was unusually light, and it is probable that on its increasing the actual saving will be greater. The number of primary sores and gonorrhœa cases under treatment during 1873–8 are too much affected by the stoppage of pay to afford a correct result.

The benefit of these acts has not been confined to the troops, for the mortality from syphilis among the civil population, in the districts where they have been enforced, has been materially reduced since 1870, while, with the exception of London, it has increased to an equal or greater extent in the remainder of England and Wales. Ten of the twelve protected stations in England are in the counties south of the Thames and Bristol Channel, constituting the second and fifth divisions of the Registrar-General. One station, Woolwich, is in the first or metropolitan division, and one, Colchester, in the fourth division. Taking the quinquenniads 1865–9, 1870–4, 1875–9, the mortality from syphilis for all ages in the second and fifth divisions was 69 per million living in the first period, declining to 62 in the second, and to 59 in the third, a reduction of 14 per cent. In the third, fourth, and sixth divisions extending from the east coast to the Welsh border, immediately to the north of the second and fifth divisions, the mortality from syphilis in 1865–9 was 67 per million, almost the same as in the two southern divisions, but instead of falling in the next quinquenniad it rose to 72, and in 1875-9 rose still further to 78, an increase of 16 per cent. In the next group to the north, comprising the seventh and eleventh divisions, the mortality from syphilis in 1865–9, was 43 per million living, rising to 49 in 1870–74, and to 59 in 1875–9, an increase of 37 per cent. In the remainder of England to the north the deaths rose from 85 per million in 1865–9, to 88 in the next quinquenniad, and to 98 in the following one, an increase of 15 per cent. London presented the only exception to the general rise, this, with the neighbouring districts comprised in the first division (embracing Woolwich, where the Acts are in force), had a mortality of 144 per million from syphilis in 1865–9, which fell to 129 in 1870–4, but with a slight rise to 131 in 1875-9. Some of the increase here shown is due, no doubt, to the advancing knowledge of the medical profession as to the forms of constitutional syphilis enabling them to refer cases to that disease which were formerly included under some other designation, but as this would apply to the practitioners in the two southern divisions as well as to those in the remaining nine, the facts point clearly to the influence of the acts having diminished the mortality from syphilis not only in the immediate localities where they are enforced, but to a large distance around them, while in London there has been a smaller reduction and n the rest of the country a marked increase.

DISCUSSION.

Dr. Birkbeck Nevins, Liverpool: maintained from the evidence of Mr. Lawson, and the evidence before the Select Committee, that there had been an increase of secondary disease

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in the army at large during the whole period of the Acts. That an extraordinary precaution has been adopted in the stations under the Acts—viz., the re-examination of every soldier on entering a subjected station to prevent his spreading disease, while no such precaution is adopted in the unsubjected stations. That in spite of this precaution, the proportion of secondary syphilis, compared with primary, has risen in the stations under the Acts from 34 per cent., before the Acts, to 56·0 per cent. under the Aets, while it has only risen to 42·4 per cent. in the stations not under the Acts. That Mr. Lawson stated to the Select Committee there was a greater danger of contracting true syphilis in the stations under the Acts than in those not under them, because the women were most diseased in the subjected stations. The returns laid before the Select Committee showed that disease had risen among these women by 24 per cent. since the Act had been put in force, and as they associate with an almost incredible number of men, they naturally impart an increased amount of severity and disease.

Dr. Routh, London: said that opponents to these Acts were under a disadvantage, because these statistics are prepared by our enemies. 1st. The figures, as stated by Mr. Lawson himself, in answer to question 2162 before the Committee, "that they are only given to you to compare the stations as far as you can do so legitimately." So if you hold a different opinion from Mr. Lawson, your conclusion is not legitimate. Again, in reply to question 2163, then the figures are of no use? He replies, "without proper explanations of what the effect of the figures is, *they would not be of much use*, and I apprehend that I come here to *state what the figures teach statistically*." 2nd. Mr. Lawson also is opposed to other army authorities, for he himself tells us there was a mistake of 600 cases in return A, being 8,774 in lieu of 8,174. This mistake runs through the whole table, and makes the number for gonorrhœa and its sequelæ too far by 600 (1260). The table called Mr. Lefevre's table has been gone over, and *several errors* have been found in it (1825). Some of them are *considerable* (1826). 3rd. The figures put down on the table to-day by Mr. Lawson were apt to delude. Why include any returns after '73? when you know they are affected by Lord Cardwell's hospital stoppage order, it being admitted on all hands that hundreds, if not thousands, have hidden their diseases from their officers. As I read the Army Reports gonorrhœa gave the mean of 123·7 per 1000, not under the Acts, 110·6; and I find 27 syphilis, which in 1866 was 24·77 per 1,000 entire home army, and in 1872 24·26; but if I take '78 I make it 26·64. I conclude therefore the Acts have not diminished constitutional syphilis.

Dr. Whittle, Liverpool: maintained that, since Lord Cardwell's order came into effect, the Government returns are valueless. Further, it has been admitted by the medical officers examined by the Parliamentary Committee, that the Acts are of no use *quoad* gonorrhœa. Now, if gonorrhœa, so easily recognized in the female, cannot be kept in check, how can true syphilis, which is comparatively more difficult to diagnose? In Liverpool there was much less disease in her Majesty's ships than at Portsmouth. From the neglect of sanitary consideration shown to the class among whom prostitutes are most found, the improvement wrought in the army by Sidney Herbert's sanitary measures might well be absent among the civil population.

Surgeon Myers (Coldstream Guards): Our great object is to find out whether the Contagious Diseases Acts have or have not diminished the prevalence of venereal disease in our army and navy. I feel inclined to disagree with Inspector General Lawson when he states that when dealing with this subject error is less likely to occur in large than in small statistics, for I maintain that the statistics of the Guards, though small, are

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those of all troops the most trustworthy, owing to the fact of their being chiefly moved between two stations, London and Windsor, the latter only having the Acts in force. What is our invariable experience of these two stations? This, that whilst in London our hospitals are full of syphilitic cases, in Windsor, after a short residence, there is little or none. I will not repeat the various statistics I have drawn up in proof of my statement, as they can be found in the Blue Book of the present Parliamentary Commission. The opponents to these Acts take, it seems to me, as their standpoint, the statistical returns of secondary syphilis, whereas, of all returns, these are the most liable to error, so many circumstances occurring to prevent the full benefit of the Acts being shown in this respect. Let me mention some as they occur to me. 1. In former years, before great attention was drawn to constitutional syphilis, many cases of this disease were without doubt returned by medical officers under other headings. 2. When the pay of the soldier in 1873 was stopped during his residence in hospital for primary and not for secondary disease, it was only just to the man that the change should be recorded in the returns, whereas prior to this time the disease might inadvertently or otherwise have remained recorded only under its first condition. 3. The comparison of secondary affections in protected and unprotected stations is most fallacious, owing to the time which frequently elapses before the constitutional disease becomes developed, and the consequent prior departure of the man from one to the other station. With regard to gonorrhœa, I will only say that whatever may be thought of the statistics, the disease is of minor importance, as it is generally so local in its injurious effects. In conclusion, I maintain that if we really want to learn whether the Contagious Diseases Acts are beneficial or otherwise, we should limit our statistical observations to primary syphilis, as this is what we wish to combat, and in checking this, it is my firm opinion, based on a long experience of the beneficial nature of the Acts, there cannot be a doubt.

Mr. Carson, Liverpool: These Acts were called by Mr. Lawson an experiment, but this experiment was completely vitiated by the fact that men are examined when entering the protected districts, but are not examined when leaving them. Thus the first condition of an experiment—namely, that other conditions should remain unchanged—is not observed. We cannot accept Mr. Lawson's statistics as final, seeing that errors have been discovered by the department themselves, one table having been absolutely withdrawn by Mr. Lawson because [see his answers (No. 1568 to 1572) before the Common's Committee] he had based it on the *assumption* (sic) that the proportion of primary to secondary was the same in the subjected and unsubjected stations, which he found to be erroneous. We do not know now if these latest statements of Mr. Lawson are not also liable to revision. He also points out a very serious error in the Return A, put in by Sir W. Muir, a mistake of no less than six hundred cases. Also Mr. Lawson, in explaining a difference in ratio given by himself, and one given by Sir W. Muir, says, "I presume that is one of the slips in this paper. We have found many in them." All arguments in favour of the Acts are statistical, and these statistics are really not very valuable.

Dr. William Carter, Liverpool: desired to draw attention to certain facts: Firstly, after certain Acts intended to diminish or stamp out venereal diseases had been in operation for several years, an army order was issued imposing penalties on men who acquired such affections. Now, why was such an order issued if these Acts were satisfactory, or, in the judgment of the authorities, likely to be satisfactory? There are several necessary inferences from this fact: Firstly, the fall in the number of admissions points to the possibilities of concealment; and secondly, the fact of the men not being in hospital proves that they

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were not sufficiently ill to prevent their doing their duty, and confirms the belief that such diminution is generally one in local venereal sores of a comparatively trivial nature. Secondly, secondary syphilis was higher in 1872 than in 1866. But the fall previous to 1866 was a continuous one; it is therefore correct to take the amount of reduction reached in 1866 as the basis of comparison, and not an average of several years. Thirdly, a steady fall having taken place year by year up to 1866, the inevitable inference seems to be that such diminution would continue afterwards. But the Acts having been introduced at this time, it is assumed by those who support them that the subsequent results were due to the Acts. Fourthly, it is admitted by Mr. Lawson that the risks of acquiring syphilis compared with the risks of acquiring simple venereal sore are greater in the subjected than in the unsubjected districts. It is clear from other evidence that these districts, owing to the assumed protection afforded by the Acts, are visited more than they were formerly. Here we have, then, a greater liability to disease if a man runs the risk; a greater disposition to run the risk; and yet we are asked to conclude from the Registrar General's Returns (which the first Report of the Society for extending these Acts say cannot be relied on for this purpose) that there is a diminution of syphilis in the civil population among which they prevail.

Surgeon General MOUAT, V.C., C.B., London: As one of the oldest administrative medical officers in the Army, I feel I cannot remain silent under the turn this discussion has taken. I have listened to an amount of declamation and special pleading foreign to a scientific discussion, and which reduces it to a sentimental question, and even a political one, which I maintain the opponents of the Act have made it. I am here reminded by the President to keep to the statistical question; but how can I do so when our opponents impeach their honesty, interpretation, and accuracy, and term our figures mere official data, and therefore unreliable. There is no such thing as cooking returns in the army, the officers of which are honourable men and incapable of such a thing; in fact, the mere figures are compiled by clerks, and the authors of the returns have no prejudices or theories to maintain. I cannot understand any one who has seen the working of these Acts, professing to speak of common sense and humanity, disputing the benefits of these Acts—Acts that have benefited both the men and unfortunates alike. Who are the people who complain? Not those directly interested, but a truly sentimental party. I felt struck with the benefit of inspection on first visiting Paris in 1836, observing the marked difference in the character of venereal disease in England both in the army and civil population. I asked an explanation of Ricord, Larrey, and Duchatelet. They all agreed in attributing it to registration and inspection, and therefore early treatment of the disease. On the introduction of the Acts into the army in this country I was principal medical officer of a large military district, Aldershot. When the officer charged with this duty came to me for instructions, I told him to treat these women as virtuous. The change in their conduct, general appearance, and manner, were as conspicuous as the effect in diminishing the severity of disease. I was afterwards stationed at Portsmouth, one of our largest military and naval garrisons, as principal medical officer for three years, when I had ample opportunities of judging of the benefits of these Acts. They were so marked and obvious to any unprejudiced observer, as to require little or no demonstration. I visited on one occasion the Lock Hospital, and each of the military hospitals, in conjunction with the Adjutant General of the Army, Lord Airey; he was particularly struck with the clean, quiet, and orderly demeanour of the most notorious class of public women in this country, and I was able to point out to him, not only a small percentage of venereal diseases in the garrison, but one regiment actually without a single case of it in hospital.

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I maintain this is a question of prophylactic hygiene—as such to be judged solely in the interests of humanity, into which neither sentiment nor any other crotchet should be allowed to intrude—and it is mere mockery and waste of time to attempt to convince men who are blinded by prejudice, and will not believe what is clear to every officer, almost without exception, in the army and navy, respecting the unqualified benefit of the Acts.

Dr. CHAPMAN, Paris: referring to the charge made by the previous speaker, Mr. Mouat, that the moral opponents of the Contagious Diseases Acts were sentimentalists, said that, at all events, he had not treated it sentimentally. His first act in the matter was to describe the baneful effects and extent of venereal disease generally, and to insist on the importance of adopting measures to lessen it. He admitted the truth of Surgeon General Mouat's statement that the women subject to the Acts approve of them—the reason being that a vast number of prostitutes leave the "protected" districts, and thus leave those who remain in the enjoyment of a virtual monopoly of the soldiers' visits. Dr. Chapman assumed the statistics given prior to 1874 to be trustworthy; but insisted that they can only be rightly interpreted by comparing the ratios of admissions at the stations now under the Acts with the ratios at the same stations before the Acts were introduced; and he maintained that such a comparison proved that venereal disease lessened at nearly all the stations more rapidly before the Acts were applied than it did afterwards. He asserted that the experience of the Acts, as now applied only to a small number of districts, is likely to be much more favourable than the results would be of an application of the Acts to the whole country. Referring to Paris, he stated that the system of the *Police des Mœurs* is officially admitted to be a total failure, and that venereal disease is steadily increasing in that city.

Surgeon General Dr. ROTH, Dresden: Gentlemen, will you allow some words to a foreigner, who will explain the immediate impression made upon him by this discussion. A foreigner, coming into England, will find a country advanced in every question of hygiene. In one thing only he finds her surpassed by other countries, to wit, the protection of human health against disease, particularly against syphilis. Being a guest of my friend Mouat at Aldershot, in 1867, I saw that such a large amount of syphilis existed in the army that it was as if every soldier in the army had been treated in the hospital for it for seven days in the year. When I asked why the women were not subjected to examination, and sent to the hospital if found ill, I was told that no such thing could be done in England. The question under discussion having been long ago brought to a conclusion in foreign countries, seems to me merely a domestic, but not a medical one. Finally, I have to reply to that gentleman who spoke about an "official science," and say that English official science is highly estimated in foreign countries, and looked on as a basis for scientific investigation, and that we should feel proud to possess this official science. I therefore refrain from further comment.

Surgeon General C. A. GORDON, C.B., Q.H.P., London: supported the Contagious Diseases Acts in relation to British troops. He did so as a result of great experience with soldiers during thirty-nine years. He contrasted the severity of syphilitic disease primary and consecutive, before the introduction of the Acts, and subsequently to that date, and indicated the great decrease which has taken place during the latter period. Seeing the beneficial results which have followed the Act, as applied to soldiers, he would be glad were they extended beyond that class.

Inspector General LAWSON, London: in replying, stated most of the criticisms of the

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various speakers have been refuted more than once, but they are still put forward as if this never had been done. The stations never under the Acts have not been compared with those under them; those never under the Acts are only employed to indicate the variation of incidence of disease over the country, and in the paper this is expressly stated. Had men on arriving at stations under the Acts not been examined, the responsible medical officer would have been open to serious reprehension for overlooking an important detail of his duty. The medical returns merely give the numbers of cases of disease which come under the medical officer's notice in a certain time; and to apply the facts they contain to elucidate the relative prevalence of disease under different circumstances of the service, it is necessary to have a knowledge of these circumstances, and to employ the facts with such precautions as these require to avoid erroneous conclusions; the remarks of several of the speakers to-day show that from want of this practical knowledge they have fallen into serious error. Secondary syphilis has fallen proportionately with primary sores. In the paper read the actual numbers of admissions for primary sores and secondary syphilis from 1860 to 1872, the period for which this test can be applied, are given. These figures show that for these twelve years secondary syphilis did decrease regularly as primary sores did. Secondary syphilis it is said has not been reduced in the army at large since 1866. The secondary syphilis at the stations not under the Acts will fluctuate as the incidence of the disease over the country varies, and has kept high, but at the stations under the Acts there has been a reduction from 1860–63 to 1870–73 of 39 per cent. The Return 6 B laid before the Committee of the House of Commons showed the percentage of the secondary syphilis on the primary, not only for the periods before the pay was stopped under Lord Cardwell's order, but after that came into force, when, owing to the great reduction in the primary sores, the percentage of secondary seemed to rise very much, but the Committee was warned of the fallacy that lay under this, and Dr. Nevins has disregarded the warning and misinterpreted the figures, endeavouring to show there has been no reduction of secondary syphilis at the stations under the Acts, while there has really been the large one of 39 per cent. The statement regarding the more frequent occurrence of secondary at stations under the Acts was, that at these for every 100 primary sores there seemed to occur about 36 secondary cases, while at the stations not under them, for every 100 primary sores there were about 33 secondary cases. Now, as the stations under the Acts are most of them large places, while many of those not under them are small country towns, it is quite possible that there may be a few more syphilitic sores met with in the former than in the latter, but, as was shown in the paper, the total number of primary sores and secondary syphilis was much reduced where the Acts were in force. As to the return placed before the Committee of the House of Commons and subsequently withdrawn, certain information received subsequently showed there was a very small element of uncertainty in it. This was explained to the Committee, and their permission to withdraw it requested, which was the proper course to adopt under the circumstances. As to gonorrhœa at the stations under the Acts, it has fallen 7 per cent. more than at those not under them. It was said that from 1861 to 1866 the course of syphilis, as indicated by the deaths in the Registrar-General's Reports, did not agree with its course in the army. For years past the opinion has been becoming more and more general that many deaths previously put down to other diseases were really due to syphilis, and under this impression the numbers returned to the Registrar-General up to 1865 increased so largely from year to year as to obscure fluctuations. In 1866 the deaths from syphilis in England and Wales were 79 per million living, in 1878 they were 88, and in 1871 they had fallen to 77, from which they rose to 90 in 1875, and 89 in 1876; these periods of minima and maxima corresponding to similar minima and maxima in the military returns.

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Dr. Nevins's theory that the extended means of ablution, &c., introduced on the recommendation of Lord Herbert's Committee, led to a reduction of primary sores before 1866, and that a continuance of the same measures led to their further reduction, is untenable. These means of ablution were not for removing infective matters after connection, but for ordinary personal cleanliness, and were applied, along with the other measures for the improvement and occupation of the men, about the same time and to about the same extent at the stations never under the Acts, as at those which came under them, and had they reduced the disease at the latter they should have reduced it at the former also, but this did not take place. Mr. Carson drew attention to a return from the Army Medical Department, in which there was an error of 600 in primary sores as originally put in. A very abrupt deviation of the curve of admissions at the stations which came under the Act, led to the working sheets at the Medical Department being re-examined, when it was found that the figure 1 in the sum 8174 had been so formed as to bear an obscure resemblance to 7, and had been reckoned up as 7, giving a number 600 in excess of the correct one. This, of course, was corrected. Dr. Carter has given no authority for his remark that Lord Cardwell's order was issued because the authorities were dissatisfied with the working of the Acts. Lord Cardwell's reason for issuing this order has not been published; but there is no doubt that the medical authorities at the time made the strongest representations in their power as to the injurious effects it would produce.

On Insolation or Sunstroke.

By Surgeon General Sir JOSEPH FAYRER, K.C.S.I., LL.D., F.R.S., Q.H.P., London.

The Sixteenth Annual Report of the Sanitary Commissioner with the Government of India being that for 1879, tells us that out of a force of 57,810 European soldiers in India in that year, there were admitted—

274 from sunstroke,
58 from apoplexy,

they are not bracketed in the returns as the cases of natives, but they may fairly be considered as etiologically linked, and due to the same cause—heat! Of these 116 died of sunstroke, and 12 died of apoplexy. It appears that out of a force in the Bengal Presidency of 37,337 men, there were 202 cases of sunstroke and 27 of apoplexy, with 93 deaths from the former and 8 deaths from the latter. In the Madras Presidency, out of a force of 10,574 men, there were 36 cases of sunstroke, and 14 of apoplexy, with 3 and 1 deaths respectively. In Bombay, out of 8,899 men, there were 36 cases of sunstroke, and 7 of apoplexy, with 9 and 3 deaths respectively. The Native Army of 130,011 men, had 40 cases of sunstroke and 26 of apoplexy, with 33 deaths from both combined. The jail population of 117,680 persons, had 102 cases of sunstroke and 44 of apoplexy, with 78 deaths from both combined. There were 4,640 European women belonging to the European regiments in India, and they had 12 cases of sunstroke and apoplexy, of these 10 died. There were 8,993 children of the European regiments, and they had 7 cases of sunstroke with 4 deaths, they suffered much less than adults. This disease, therefore, forms an important item in the general death rate of India, as it does, no doubt, wherever great heat is combined with certain other climatic peculiarities. These figures, representing the incidence of the disease among a certain

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class of persons, whose condition, history and circumstances are the subject of accurate observation, are valuable, as far as they go, but cannot be supposed to represent more than a very small part of the mischief and mortality caused by great heat. The returns probably represent chiefly, if not entirely, the cases of the more severe forms, in which the cerebro-spinal system was seriously compromised; but to these might be added cases of so-called ardent fever, a form of disease due to the immediate effects of heat, rather than to any other climatic influence; though, doubtless, malaria is frequently associated with high temperature. The conditions to which I would call attention are as follows: under the designation of sunstroke, coup-de-soleil, heat apoplexy, heat asphyxia, thermic fever, ardent fever, ictus solis, insolation, calenture, and erythismus tropicus, certain pathological conditions are included, which differ materially, though they are not unfrequently confounded with each other.

1st. There is syncope from exhaustion caused by heat.

2nd. A condition resembling shock caused by the action of the direct rays of a powerful sun on the head and spine. The nerve centres, especially the respiratory, may be affected; respiration and circulation fail, and death may, and often does, result!

3rd. Over-heating of the whole body, blood and tissues, either by direct exposure to the solar rays, or to high temperature in the shade; vaso-motor paralysis and intense pyrexia cause failure of respiration and circulation, and result in asphyxia.

Recovery is often incomplete, owing to molecular changes, which cause a variety of symptoms indicative of grave lesions of the nerve centres.

The first form, that of exhaustion, occurs during fatigue or over exertion, especially when the physical or mental powers are depressed. As in the case of stokers and engine-room men in steamers in the Red Sea, or other tropical regions, where the temperature of the air rises to 120° or upwards; also in that of men—especially Europeans—who are exposed to intense heat and light, in the *direct solar* rays.

Death may occur in this state from failure of the heart's action!—syncope. Again, during exposure to the direct action of a tropical sun, when the atmospheric temperature is high, and especially when the air is loaded with moisture, when the nervous energy is depressed by previous illness, over-fatigue, or dissipation, a condition of shock may supervene, the respiratory and circulatory centres fail through inhibition by the vagus—and death results. When in such case death occurs suddenly, it has been ascribed to rapid coagulation of cardiac-myosin; this, however, though it may occur, is perhaps more frequently a post-mortem change; the heart's action being really brought to a close by tetanic contraction of the ventricles, as shewn by Claude Bernard and Lauder Brunton, may be in the case of animals exposed to great heat.

I have seen, and have recently heard of a case in which evidence of cerebral irritation did not supervene until a day or two after exposure to the sun, when very grave symptoms developed, but happily passed away after prolonged illness.

Recovery in ordinary cases is frequently complete, but may be tedious and imperfect.

The symptoms of this form of sunstroke are in the outset like those of simple heat-exhaustion, only more pronounced.

The person becomes unconscious, or nearly so; there is a cold skin, feeble pulse, sickness, and all the symptoms of collapse and exhaustion; and in this condition, if reaction is not established, he may die.

As I have said, recovery most frequently occurs, though in some cases it is rendered incomplete by lesions of the nerve centres.

The third condition, or that which may occur quite independently of the direct rays—though it may happen also during exposure to great solar heat—is more severe and dangerous.

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It comes on frequently at night, in a room or tent, especially in persons who are debilitated, or who are suffering from exhaustion or fatigue, dissipation or illness, or from over-indulgence in food or alcohol, or who have weak or fatty hearts, and notably when the air is impure from crowding or insufficiency of cubic space.

Such form a large proportion of the fatal attacks of so-called heat apoplexy. In India many die; more who recover, often do so only partially from secondary consequences—the result of tissue changes, destroying life or impairing health and intellect at a later period.

The premonitory symptoms may appear some hours, even days, before the dangerous condition sets in. There is general malaise and restlessness, insomnia, apprehension of impending evil, hurried, shallow or gasping, sighing respiration, precordial anxiety, giddiness, headache, occasionally nausea or vomiting, thirst, anorexia, fervent heat of skin, a disposition to frequent micturition and disordered secretions.

The symptoms becoming aggravated, the temperature rises from 106° to 110°; dyspnœa increases; great restlessness; the head, face, neck, and skin generally become livid; pulse full and labouring; carotid pulsation very perceptible; pupils contracted—dilate widely before death. Coma, stertor, convulsion, often epileptiform, relaxation of sphincters, and suppression of urine are the precursors of death.

These symptoms indicate a profoundly disturbed state of the cerebro-spinal centres, whose functions have been so seriously interfered with. Death is caused by asphyxia and apnœa, it may be, complicated with cerebral hæmorrhage and effusion.

Recovery is often, indeed, one may say always, in this form, incomplete. The general health, even if there be no obvious cerebral lesions, remains impaired, the memory defective, and there is complete intolerance of the sun's rays or of great heat in any form.

To these may be added certain cases of fever—due to exposure to great heat—in which the temperature rises very high, and in some cases proves rapidly fatal. These are the ardent fevers, which are liable to be complicated with symptoms of malarial poisoning, and to have the remissions peculiar to that disease.

Intense heat is the cause of the conditions just described;-and the degree in which it operates is more or less influenced by the hygrometric state of the atmosphere.

A dry hot air is better tolerated than a damp though cooler atmosphere. The dry hot winds of Upper India are less trying than the moist atmosphere of Bengal, or some parts of Southern India.

Dry heat favours perspiration and keeps the body cool; damp air, though cooler, represses perspiration, and thus diminishes the natural cooling powers of the body.

The state of health and acquired power of toleration and idiosyncracy influence the susceptibility to, and power of, resisting heat. Vigorous healthy Europeans, of spare frame, temperate habits, and who have sound viscera, can sustain a great amount of heat. Acclimatization has also some influence in conferring toleration; new arrivals in the tropics are more prone to suffer than those who have become accustomed to the climate, and have learned how to protect themselves.

A native can bear an amount of sun on his bare head and body with impunity—nay, with pleasure—that would prostrate a European. But when the temperature rises above a certain standard, all succumb, and natives suffer and die like others; numbers perish every year from "Loo Marna"—hot wind stroke.

Toleration of heat depends greatly on vigour of constitution, state of health, and mode of living; especially in freedom from excess in food and alcohol.

The refrigerating powers of the body in health render it able to support a very high temperature, provided transpiration is unimpeded by atmospheric moisture or other causes, and the air is pure. In the dry hot winds little inconvenience is felt so long as

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perspiration is free: but when from any cause that fails, suffering soon ensues, and the danger is great. The danger to life is great when the temperature of damp air rises above the normal body temperature.

The hot months of April, May, June and July are those in which the disease most prevails, and those who are most exposed suffer most. But, as I have already said, some of the worst cases are due to heat quite out of the sun's rays, for they occur at night, and in the close atmosphere of barracks, tents, houses, hospitals; and especially to those who are predisposed by debility, disease, or intemperate habits.

Men on parade, or when marching, if oppressed with clothing or accoutrements, are apt to suffer, and fall out fainting and exhausted. Soldiers, labourers, artificers, and others, in heated rooms, hospitals, barracks, tents, or ships, may suffer in this way, and in some cases pass into the dangerous state of asphyxia I have described, especially if they have indulged in excess of food or drink.

The more serious cases where cerebro-spinal mischief takes place, may also happen in these circumstances, though the greater part of these occur, as I have described, without the direct intervention of the solar rays. I find by reference to the Sanitary Commission reports that the sunstroke is pretty widely spread; that in the months of April, May, June, and July, it prevails most, and that there is no remarkable number of attacks at any one place, except at Mean-Meer, where among 899 men, there were 13 cases in July; at Nowshera, strength 630, there were 23 cases in June and July. In the Peshawur valley out of a force of 3,431, there were 28 cases in May; whilst in Malliapoorum, in Madras, there were 13 cases among 93 men, in May, 1879.

The deaths per 1,000 strength were,* in men of between one and four years service, 1·48; five to seven years, 1·50; at ages under 25 years, death rate 65 per 1,000; from 25 to 29, 99 per 1000. From 30 upwards, 2·33 per 100.

Though many who are attacked recover, or rather do not die, a large proportion are permanently injured, and become invalids for life, which is frequently shortened by obscure cerebral or meningeal changes that affect the sufferer in various degrees of intensity, irritability, impaired memory, headache, mania or dementia, partial or complete paraplegia, partial or complete blindness, intolerance of the sun's rays, rendering a person, otherwise fairly healthy, incapable of serving in hot climates or often during exposure to the sun; it may be, gradually ending in fatuity, dementia, or epilepsy, perhaps both; chronic meningitis, with thickening of calvaria and intense cephalalgia, or in a lesser degree in disordered innervation and general functional derangement, which seriously compromise health.

Morbid Anatomy.

When death has occurred suddenly, as from syncope, or shock, there is no very obvious morbid change, but the heart may be found firmly contracted. In animals dead from exposure to a high temperature—*i.e.*, when the blood and tissue have been heated to 110° or 112° or higher—the heart has been found tetanically contracted in some cases, though in others it remained flaccid.

The brain and membranes and the lungs are sometimes, not always, congested. As

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* Bengal . . .	2·61	per 1,000	died.
Madras . . .	1·44	,,	,,
Bombay . . .	1·21	,,	,,
Average	2·17		

Table XII. European Troops, 1879.

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in cases of shock from violence, the venous trunks, especially those of the abdomen, and the right cavities of the heart, may be full of blood which is dark, grumous, often imperfectly coagulated and effused in patches of ecchymosis, rendering the body rapidly livid. The coagulability of the blood is impaired, and it is deficient in oxygen.

In death from the more severe form of insolation, the lungs and pulmonic circulation are often deeply congested; the heart is firmly contracted, it may be, by coagulation of myosin, and the whole venous system is engorged.

The body, even before death, may be marked by petechial patches, or extensive livid ecchymosis. The blood is generally dark and grumous, and may be acid in reaction.

The globules are crenated and do not readily form into rouleaux. The body for some time after death retains a high temperature. When the body is opened, the viscera and interior feel intensely warm, hot and dark blood drips from the incisions.

Rigor mortis comes on rapidly. The brain and membranes may be congested; there may be cerebral hæmorrhage or effusion of serum.

Treatment.

Prevention: The great object is to prevent the occurrence of the disease. Careful protection of the head and spine from the direct rays of the sun by proper solah (pith) hats; light clothing, fine woollen is the best, should be worn.

Precaution against over-fatigue or exertion, either physical or mental, and freedom from excitement on the one hand or depression by fatigue or want of food on the other, is most desirable. Rooms should be well ventilated and the atmosphere kept as pure as possible by constant ingress and egress of air.

The punkah, thermantidote, and tattie should be freely used; and as much as possible men should avoid exposure to the sun, and fatigue or work during the hotter parts of the day; during the hot still nights, a most dangerous time, punkahs should be kept freely going, windows open, and the body should be protected by light woollen covering to protect it against sudden alterations of temperature.

Moderation in diet and drink is essential. Excess of animal food and alcoholic drinks should be scrupulously avoided; depression from want of food equally to be deprecated. A moderate amount of physical exercise and mental occupation should be encouraged; regularity in living should be enforced. No one more likely to suffer from insolation in any of its forms than he who has suffered from mental or physical exhaustion, or who has become habituated to the abuse of alcohol or of excess of food.

Men who are healthy and who lead regular lives will withstand heat, and preserve health in a measure that would be regarded as remarkable if contrasted with its effects on those who do not observe these precautions.

Having said so much in regard to prevention, I would make a few remarks on treatment.

In simple heat exhaustion, remove the patient to a cool place, in the shade if possible.

Douche with cold water on the head and chest, not too prolonged, so as to over depress. Remove all tight and oppressive clothing, apply ammonia to the nostrils, and give a slight stimulant; if depression be prolonged or profound, treat as for an ordinary faint.

Place the person in a quiet place, and let him avoid fatigue or exposure to more heat as much as possible.

When a man is struck down by the hot sun, use the cold douche affusion freely; the object is two-fold—to rouse by reflex action and to reduce temperature.

During the assault on the White House Picket at the capture of Rangoon in 1853, in the month of April, numbers of men were struck down by the fierce sun. They

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were brought to the field hospital, and laid out in rows, quite unconscious; they were clad in thick red coats with leathern stocks (they wore them in those days).

They were treated with free douching with the Bhisty's mussuck, and all recovered except one or two who had been bled when they were struck down.

In one case, that of a general officer, flagellation with a sweeper's broom was effective in rousing. Mustard poultices to the body and legs, and stimulating enemata, with a diffusible stimulant by the mouth, in some cases were useful.

When I say such cases recovered, I refer to the reaction at the time. In some there were consecutive symptoms of fever, cephalalgia, &c. And, could we trace their subsequent history, it would probably be found that complete recovery never took place, but that some chronic intra-cranial mischief was established.

Exposure to the sun should be carefully guarded against in future; and, unless recovery has been complete and rapid, the sufferer should be removed to a colder climate, where he must be protected from all excitement, mental or physical, and continued care must be taken to prevent excess of every kind. In severe cases of thermic fever, the object is to reduce temperature as speedily as possible, before tissue changes have taken place. The hyper-pyrexia is due not only to the direct action of heat on the tissues, but to pyrexia caused by vaso-motor disturbance, and therefore quinine and morphia used hypodermically have been thought to produce good results by their power of reducing blood pressure. The quinine probably may do so, the advantage of morphia I should think questionable.

Bleeding has been abandoned, except in rare and exceptional cases. The congested vivid skin, the coma and stertor which suggested it, do so no longer, as a general rule, though there are cases where it may be necessary to relieve a labouring right heart, and prevent suffocation. In ordinary cases, though temporary relief may have been afforded by it, the improvement has been transient, and followed by relapse into a more fatal condition.

No absolute rule should be laid down in this or any other disease; it may, for reasons above mentioned, be the lesser evil, and therefore adopted.

Each case must be treated on its own merits. Treatment generally consists in cold affusion and the application of ice, care being observed not to reduce the body temperature too low. A thermometer will guide in this respect.

Depression of the body temperature below the normal standard for too long would be dangerous, and therefore should be avoided. The bowels should be relieved by purgatives or enemata.

Sinapisms to chest may be useful.

Blisters are sometimes applied to the neck, but rarely, I should think, with any benefit. In the epileptiform convulsions, inhalation of chloroform may be useful, but it must be carefully watched.

The earliest and most severe symptoms having subsided, the fever which follows must be treated on ordinary principles. As the case progresses, symptoms of intra-cranial mischief may supervene, indicating meningeal inflammation. Iodide of potassium and counter-irritation may be of service. As a rule it is most desirable that one who has so suffered should not return—at least for a considerable period after he has apparently recovered—to a hot climate, and that he should always be guarded against heat, overwork, exhaustion, anxiety, and errors in diet.

The sequelæ of sunstroke are often very distressing, rendering the patient a source of much anxiety to himself and to his friends.

The less severe symptoms, such as those of the slighter form of meningitis, or of cerebral change, occasionally pass away after protracted residence in a cold climate.

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They are, however, frequently the cause of suffering, danger, and shortening of life, and form some of the saddest proofs of the danger of residence in tropical countries.

The time at my disposal is too short to allow of justice being done to the various authorities on this subject. Had it been otherwise, and these remarks had extended beyond a few practical observations, the outcome of personal observation in India and Burmah, I should have referred to your own valuable contributions; and to those of the late Mr. M. Hill, and of Dr. Barclay, to the valuable description by our own and highest authority on all tropical disease, Professor Maclean; but especially to the thoughtful, scientific, and most valuable work by Dr. H. C. Wood, of Philadelphia, a monograph which contains a most exhaustive description of the etiology, pathology, and treatment of insolation in all its forms or phases.

DISCUSSION.

Inspector General MACLEAN, Netley: objected to the use of the term apoplexy applied to this disease, as wrong in pathology and leading to grave errors in practice. Dr. Maclean adverted to the danger of tight clothing and accoutrements, and gave a striking example of this in the case of the 98th Regiment, where the men being dressed, when called on for exertion at a military operation in China, in tight clothing and accoutrements, had great mortality from sunstroke; while two other regiments, dressed in a more easy manner, did not suffer at all. Dr. Maclean touched on the effect of exhaustion after long marches leading to suppression of the action of the skin and kidneys, and of alcohol in increasing the liability to insolation. Dr. Maclean then gave some interesting historical examples of the disease in European armies, particularly in the case of a famous march of Frederick the Great, who lost 150 men in one day from this disease, Field-Marshal Dawn on the same day having also lost 200 men from the same cause.

Dr. NORMAN CHEVERS, London: referred to the prevalence of sunstroke in India and America. It is most important that we should observe and define individual constitution in sunstroke. A surgeon who has served in the tropics is warned by his own sensations that on certain hot days cases of sunstroke will occur; but, on sunstroke days, only those who suffer from some constitutional fault are liable to be attacked. Many cases of so-called insolation are really cases of malarious fever, the hot stage happening to occur on a sunstroke day. Many cases of sunstroke are really cases of alcoholic poisoning. Some are cholæmia or uræmia, or both. Some are cholera. I have never seen extravasation of blood, or true apoplexy, in these cases. It is a prophylactic necessity to prevent cutaneous action from being checked by any cause except cold. Are the Mongolian people of India liable to sunstroke, or are they exempt as negroes are?

Dr. CHRISTIE, late of Zanzibar, during some years' experience of the sequelæ of sunstroke, is satisfied that its after-effects are not sufficiently understood. In a large number of cases, after an attack, the mental capacity is so far interfered with as to render a man quite unfit ever to return to a hot climate. The pathological changes fully bear out the views of the author of the paper. In three or four cases I have met with a remarkable growth of spiculæ of bone from the skull, with thickening and opacity of the membranes. In one case epileptiform convulsions, with complications of mania, resulted.

Dr. YANDELL, Louisville, Kentucky: I quite agree with my learned friend as to the several points so well put in his very excellent paper on sunstroke. In

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America, the white race suffers much more than the black from sunstroke. The negro bears the sun's heat better than the white man. Whether this is due to what is supposed to be the greater activity of the negro's skin, or not, I am not prepared to say. The negro who works in the harvest field and in the cotton field very rarely suffers from sunstroke. When he removes to the city, and there lives in crowded houses and close rooms, and is subjected to the same influences which demoralize the white man, and becomes loose and irregular in his habits, committing excesses in eating, but more especially in drinking, he then becomes liable to sunstroke. Yet the negro remains under even these circumstances better able to withstand the evil effects of heat than the white man working by his side. I do not remember to have seen a genuine sunstroke from which the individual ever perfectly recovered—some twist, some evil remained. His physical strength, his mental force, his moral powers, were lessened or altered. As to treatment, the profession in America was fairly agreed that cold, applied so as to reduce the temperature in the quickest possible manner, was in the main most likely to be successful.

Surgeon Major STAPLES, A.M.D.: I served at Nowshera, one of the stations mentioned by Sir Joseph Fayrer as having a bad reputation for heat apoplexy, or insolation, and I witnessed there in 1867 an epidemic of the disease. A history of that outbreak was published in the *Army Medical Reports* for the year 1868. Before I refer to it, however, I should like to say a word concerning the other conditions under which the author of the paper has considered the effects of heat in the tropics; and firstly, as regards heat exhaustion. Instances of this, with insensibility, are less frequent in India than might be supposed, but I can recollect a very characteristic case. In the hottest part of the year 1875, I, with two other officers, accompanied the then Lieutenant-Governor of Bengal in a ride from Shillong to Gowhatti, in Assam, a distance of over sixty miles. We started after early breakfast, and the journey was completed in about eight hours. All was well while we galloped through the Shillong uplands, but when we descended into the lower ranges bordering the valley the heat became intense, and one of the party (a very stout officer of police) soon showed signs of exhaustion. He persevered, however, but after some miles in a narrow and close valley he succumbed, and fell from his horse insensible. I came up in three or four minutes, and found him quite unconscious, very pale, and breathing heavily. Fortunately a stream ran close by, and taking his Ellwood helmet, I filled it with water and douched him freely over the head and chest. After a few such applications he became completely conscious, and after a little rest was sufficiently well to be brought into Gowhatti in a dog-cart. The above is a good example of exhaustion from great heat. Clinically, it is of value, as pointing to the condition of the heart—viz., loaded with fat, judging from the rest of the body. Death by syncope from heat would appear to be but an aggravated form of the foregoing. In Bareilly (Rohilkund), in the hot season of 1871, on a very close morning, a soldier of the 25th Regiment, while employed in the regimental workshop, becoming overpowered by the heat, suddenly fell down and expired. I saw him within a minute, but life was completely extinct. I found upon inquiry that he was not previously considered to be in ill health, but a post-mortem examination discovered a very fatty heart. There was no other lesion whatever. Sunstroke, so-called, or that condition where men are suddenly struck down insensible by the sun's rays, I have never seen in India, but I have met with cases in this country They occurred in soldiers who had previously served in India, and who had suffered from malarious fevers in that country. In one case which occurred on parade at Woolwich in 1877, the return to consciousness was very slow, and was accompanied by insomnia and by

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loss of recent memory. The man could give an account of himself up to the time of being struck down, but of what happened to him afterwards his mind was a complete blank. This lasted for a couple of days, and ultimately yielded to full doses of potassium bromide. The pathology of this condition may, perhaps, be likened to shock or concussion from other causes. Insolation has appeared to me to depend not so much upon an accidental rise of temperature as upon a prolongation of a high temperature. This was the case at Nowshera, in 1867, when the hot winds blew without intermission, day or night, from the 19th to the 30th of June, the thermometer remaining for the greater part of that time above blood heat. I thought I could recognize premonitory symptoms in nearly, if not all, the cases, and my conclusion was that the surgeon's best efforts should be directed to their treatment, and thus to the prevention of the disease, for the latter appeared, when fully developed, to be little amenable to treatment. I have only further to call attention to the remarks of some previous speakers, who have laid stress on the fact that no blood is found in the brain in this disease, post mortem. No doubt this is accurate; but yet it should not be forgotten that blood serum is usually present in the brain ventricles. If the case soon terminates fatally, the quantity is small; but if, on the other hand, the case is a prolonged one, the amount will be considerable.

Deputy Surgeon General EWART, Brighton: wished to call the attention of the meeting to the importance of employing cold iced baths in the treatment of heat stroke, for the purpose of reducing the temperature of the body. At the general hospital in Calcutta he had thus seen patients restored from profound insensibility to consciousness in a few minutes. The bath is only applicable where there is augmented temperature. It is useless in *coup-de-soleil*, where at the beginning there is a collapse and reduction of temperature from shock and paralysis of the great nerve centres.

Sir JOSEPH FAYRER, London: thanked the meeting for the manner in which they had received his paper. All confirmed what he had said. There was nothing to reply to, excepting that probably Mongolians, if exposed to the same amount of heat in the hot winds, would suffer like other natives of India. Dark races bear the direct rays of the sun better than Europeans, but, if exposed to a high enough air temperature, suffer like others.

The PRESIDENT: Dr. Chevers has asked me to make some observations on the cases of sunstroke respecting which I published a paper while in India. My observations on sunstroke in India were made only in one locality—viz., in Barrackpore. On the other hand, the results of experience in this trying affection, under almost every kind of exposure and in all the chief parts of India, have been brought before the meeting by surgeons of the highest eminence and of the greatest experience in tropical diseases, and I feel I should be taking up the time of the meeting with little advantage if I were to attempt to prolong the discussion which has been so ably conducted by others who have had a far wider field of observation of sunstroke than I have had.

Medical Preparation for Action, and the Management of the Sick and Wounded on Board Ship.

Inspector-General JOHN D. MACDONALD, F.R.S., R.N., Netley.

MR. PRESIDENT, &c.,—The task which I have set myself for the present occasion is to open up the seventh or last subject on the list for discussion in this section of the Inter-

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national Medical Congress, and which is stated to be, "*On the best system of meeting the needs of men wounded in action on board modern ships of war.*"

Notwithstanding all that has been done in the military service, of late years, to perfect the principles and means of dealing with the sick and wounded in time of action, so as to mitigate the immediate evils of warfare as much as possible, very little has been done to give system and uniformity to the efforts of naval medical officers to fulfil the same humane and important purpose during action at sea.

It may be said that the conditions of naval and military warfare are so essentially different as to have but little in common. Thus, the ship is at once the *encampment*, the *fighting ground*, and the *hospital*; all of which are, more or less, widely separated in operations ashore. Also a much larger provision for casualties is usually required in the military, than in the naval service.

In all our past naval history infinitely more loss has been occasioned by preventible disease than by the shot and shell of the enemy. It is with this latter loss, however, that we have most to do at the present time, and, in particular, the best means of meeting all contingencies connected with it.

I am quite aware that many naval medical officers, who have seen much service, and consequently ought to be more competent than myself to give an opinion on the subject, think that no hard and fast rules can be laid down for general adoption. Thus, it will be found that even ships differ in form and in their whole organization and internal resources so that rules that might be suitable to one, would be quite inapplicable to another.

It seems to me, however, that this argument does not afford sufficient grounds for supposing that no rules are at all necessary, and that no practical benefit can arise from the attempt to establish a more methodical system than the merely traditional and capricious one at present in existence. On the contrary, I would anticipate the most happy results from the issue of more definite instructions in relation to the preparation for action, the duties of the medical officers themselves, and the staff of the "sick bay," as well as the duties of non-combatant officers and men, whose most useful employment during action would be to afford all the assistance they could give to the medical officers, not out of compliment, but in the interests of humanity, by a prescribed law.

If, however, a good reason for the increase of the medical staff by the appointment of trained nurses, and bearers should be wanting, it is the old standing rule in the Navy that the duty of all persons borne on the ship's books should be strictly confined to those of their special ranks and ratings.

Moreover, if fighting is to be vigorously maintained, in action provision must be made for the replacement of "gun numbers" killed or disabled; for if two men at least are required to carry off one, the loss of one third of a gun's crew would, in effect, silence the gun.

It has usually been found in naval engagements that, although efficient means may be provided for the transport of wounded from one deck to another, the seamen will naturally take the management into their own hands; and thus patients have been passed below in the ordinary unaided way, almost like magic, and apparently without having sustained any further injury. This is certainly an important fact; and it is satisfactory for naval medical officers to know that they are surrounded by men whose nautical training gives them activity of frame and readiness of hand, guided by a knowledge of the mechanical powers, which is scarcely to be found in any other class whatever. Perhaps it is on this very account that the necessity for the establishment of a "Naval Hospital Corps" has been so little felt in the past, while such an auxiliary as the "Army Hospital Corps" had long been needed before that body was first organized in the sister service.

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Manning and arming boats constitutes an evolution of much importance, as in actual warfare it is usually called into requisition. In the "Boat Signal Book," p. xix., it is provided that the senior officer's ship of the boats summoned by signal, and the captain of each ship, whose boats contain more than eighty men, is to send a fast pulling boat in addition, having a medical officer on board, to attend in rear of the line; and this is repeated at p. xxv. article 3 thus:—"A fast pulling boat with the medical officers will attend in rear of the line."

At p. xxvi. article 8, *op. cit.*, it is specified that, according to the exercise or service on which men may be ordered to proceed, additional men will be told off to carry provisions, spare ammunition, and stretchers for the wounded; but no particulars are given as to the nature of these stretchers.

From the preliminary chapter of the "Manual of Gunnery for the Fleet" we learn that a case for small stores is to be carried by boom boats and smaller boats, containing, amongst other things, two tourniquets, and that each division of boats will have a medical officer.

The quotations just given embrace perhaps all the written law on the subject; and, as a matter of fact, up to the present, medical officers engaged in such service have followed their own views and instincts, in making provision for the exigencies of each particular case.

The conventional instructions that I have received and compiled for my own guidance are the following, and, with some slight modifications in different ships, are those which are generally adopted in the naval service:—

In Case of Action or Fire the medical officer should immediately proceed to the sick bay, and, after mustering the sick, send such as are at all fit for duty in such heavy emergency to their quarters, reporting his having done so to the commanding officer. He should then select the absolutely helpless, and have them conveyed to a place of safety appointed by the Executive. Or taking charge of them to their boats, in case of fire necessitating such a proceeding, he is to see that the sick are so distributed as to be under medical supervision, when two or more medical officers are borne.

In case of action, men, not absolutely helpless, should be utilised to sling hammocks where they can be safely placed, provide fresh and salt water, sand and swabs in the cockpit, and afford assistance to the inferior sick bay men.

For General Quarters, a drawer should be set apart in the dispensary to contain a number of bandages of different kinds, calico, strapping, towels, tourniquets, waxed ligatures, &c., and it may remain always in readiness. This drawer, together with a tray containing carbolic acid, tincture of opium, chloral, chloroform, olive oil, &c., and sick bay beds, with clean sheets, should be conveyed to the cockpit. The operating table is then to be set up, and a large tray, covered with a white cloth, placed conveniently for instruments, &c.

Drinking water and vessels are to be provided by the convalescent sick told off for the purpose.

The captain's clerk is usually employed taking notes, but the chaplain, naval instructor, assistant paymaster, and the clerks of the ship are supposed to render any assistance they can afford to the medical staff.

To recapitulate briefly what has been said, and as a guide to any discussion that may follow, I would specify the five following particulars, as demanding special attention, viz.:—

1.—The selection of a suitable place for the reception of wounded.

2.—The means of conveying wounded men safely from one part of a ship to another, from aloft, or from deck to deck.

3.—The necessity of dealing immediately with cases brought under the care of the medical staff.

4.—The final disposal of cases after operation or the dressing of wounds.

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5. The organization of the medical staff, trained nurses and bearers, and the assignment of definite duties to non-combatant officers, &c.

We have to consider further, under the second head, the *means of transporting the wounded*—

1.—From aloft.

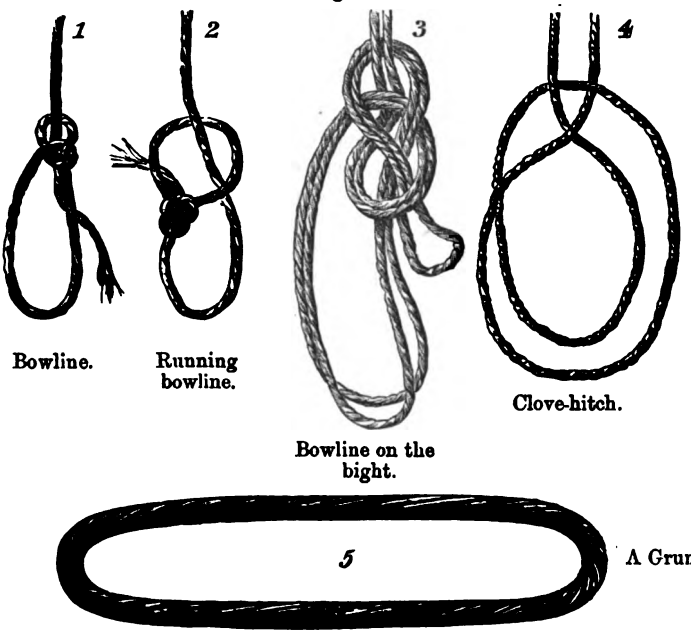
2.—From deck to deck.

3.—From place to place on the same deck.

Introductory Remarks.—"The greater number of wounds inflicted by contending armies," says Mr. Hutchinson, "are those arising from grape shot and musket balls; whereas in naval engagements the very reverse of this is the fact in an increased ratio; for the wounds received in ships of war are generally produced, either by the direct stroke of a cannon shot of large dimensions, or, what is still more lamentable, by ragged fragments of timber violently rent from the planks or beams of the ship, impelled by balls infinitely larger than any ever employed in fields of battle." If, then, means of ambulance are necessary in the military service, they would be doubly so in the naval, in which at all times, as well as in action, wounds and injuries are usually of a more severe and formidable character.

At the battle of Algiers,* under Lord Exmouth, our losses were admitted to be relatively greater than in other previous naval engagements, and therefore the medical records of that action are of much importance in the statistical way. Exclusive of the killed, the number of wounded was about 600, of whom 55, or just under 10 per cent., were cases for amputation.

Fig. 52.



Ambulance, or apparatus for transport, of any definite kind was only said to have been employed in some of the ships. It was simply in the form of the service cot, slung in the ordinary way. It was, however, admitted to have been of very great utility

* The ships engaged were five ships of the line, and six frigates and smaller vessels.

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and deserving of general adoption, instead of the rude mode of conveyance hitherto practised.

1.—In many instances, men severely wounded in the tops have succeeded in reaching the deck with little or no assistance, but it is obvious that efficient means of slinging or lowering badly wounded men from aloft is an important desideratum.

With the ropes ready to hand in the tops, the top men are never at a loss how to extemporize a sling to lower a disabled comrade to the deck in safety.

The more important knots in use for this purpose are (1) the "*simple bowline*," (2) the "*running bowline*," (3) the "*bowline on the bight*" and, (4) the "*clove-hitch*"; illustrations of which are given in the diagram (Fig. 52).

A grummet (5), or continuous ring of left handed* yarns, of reasonable thickness, and about a yard and a half in length, when extended in one direction, makes an excellent sling, or soft chain of rope, by which, with a rope's end passed through the loops, as shown in the drawing (Fig. 53), and then secured above with a bowline knot, a wounded man may be safely lowered to the deck from aloft.

Fig. 53.

2.—The term "*Ambulance lift*" was suggested to me by Professor Longmore, C.B., the President of the Section, as an appropriate name for any apparatus by which

* To obviate the tendency to twist or kink. Gunners' rope is always left-handed for the same reason.

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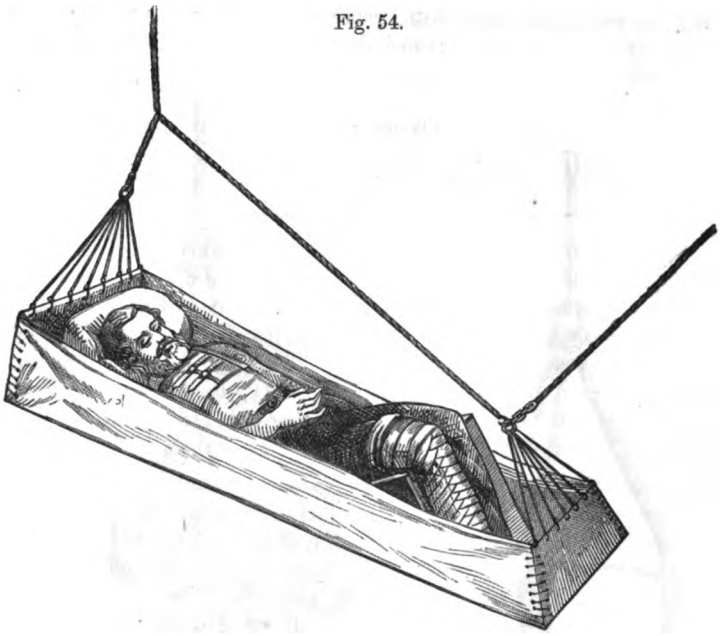
the wounded may be conveniently either raised or lowered from one deck to another. The earliest appliance for this purpose was the "service cot," slung with a pole and span, or by other simple means; but it will be at once seen that the usually small size of the hatches and the large size of the cot must render it a matter of great difficulty to pass it from deck to deck without so altering its level as to endanger the patient and cause him much pain that might otherwise be avoided.

The service cot is at least six feet long by twenty-eight inches wide, so, if inclined in any direction from the horizontal position, the patient is liable to slide within it by gravitation, and thus suffer all the inconvenience and injury that may be easily imagined to occur under particular conditions. To obviate the evil alluded to, Dr. Albert C. Gorgas, of the United States Navy, has invented a small cot with a double inclined plane, and a breast band for further security (Fig. 54).

Fig. 54.



Ambulance Cot, invented by Dr. Gorgas.

The flexion of the legs over the double inclined plane permits of the shortening of the cot to five feet eight inches, and, as it is an advantage to have the canvas sides embrace the patient closely, it is narrowed so as to be but twenty-one inches wide. Two loops of canvas, fourteen inches long, are nailed to the under surface of the upper cross-piece, and are brought over the top. Through these loops the breast-band is passed, and, the loops being movable, the band accommodates itself to all sizes of men.

The patient being placed in the cot, the upper end of the latter is raised to the proper angle by a whip; the foot of the cot, by a line attached to it, in the hands of a man, is swung over the hatch, and allowed to pass through it, and the whole apparatus is lowered together at the proper inclination.

In this ambulance cot the sliding down of the occupant is arrested by the buttocks and thighs being supported by the upper surface of the double-inclined plane, and his body is still further held by the chest band.

"In the common cots used on board the *Juanita*, this arrangement," says the

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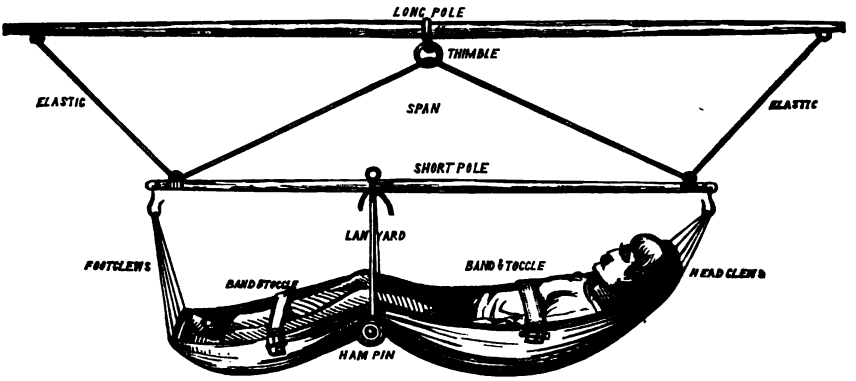
inventor, "was a complete success. In the first day's bombardment, when thirteen men were wounded by the bursting of a gun, much delay was avoided in getting them below, by the excellent working of the ambulance cot; and the propriety of having two, so that the whip had only to be unhooked from the empty one on its return to the upper deck, and applied to the other, in which a wounded man was already to be sent below, was clearly shown.

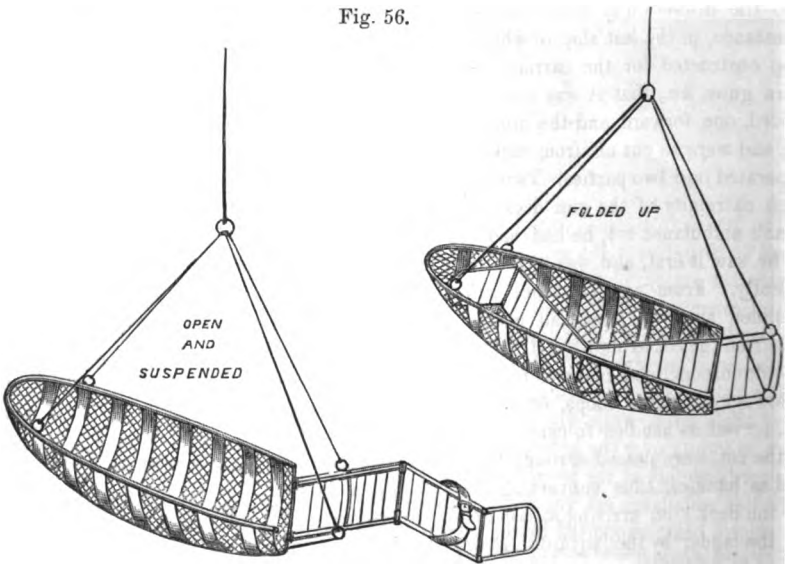
Fig. 55.



The "*Ambulance Lift*" (Fig. 55) I have now to describe was devised by myself, to supersede the use of the service cot at general quarters in H.M.S. *Lord Warden*, and it has been found very useful in transferring patients from one ship to another while at sea.

An ordinary hammock is utilized for the purpose, the clews and lanyards remaining intact. A short and rounded piece of wood, or "*ham pin*," so called, having a lanyard secured to each end, is placed transversely beneath the hammock, so as to correspond with the bend of the knee of the occupant. We thus obtain three points of suspension from a short pole, furnished with a span, having a thimble in the middle, to be connected

Fig. 56.



Ambulance Lift, or Lowering Apparatus, for the Tops.

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with the lower block of a double whip, which should be furnished with a hook at both ends.

The "*ham pin*," answers the purpose of the double-inclined plane noticed in the ambulance cot.

A long journey might be undertaken with great comfort in an apparatus of this description, attached to a pole seven or eight feet long.

At the Paris Exhibition of 1878 (Group VI., Class XIV.) a new method of transport for the wounded on board ship during action was exhibited by Dr. J. Maréchal, principal medical officer of the Port of Brest.

A wood engraving, illustrating this method, I have unfortunately mislaid, but the scheme is simply, first, to secure the patient in a hammock, rendered unyielding by splints and straps, and then to slide it down from deck to deck by means of a kind of shoot, arranged so as to hold an oblique position, extending from the fore-combing of the hatchway downwards and backwards to the deck below, where it is received by persons appointed for the purpose.

3.—The different kinds of stretchers, either recommended or in use, need scarcely be spoken of here; sufficient to say that the simpler they are the better. The supply will be more liberal, and they will be more easily replaced, as well as less likely to get out of order.

With the arrangement Fig. 56, which should be always at hand in the tops, a man six feet high may be easily lowered through a hatchway only four feet in diameter, without altering his original position, the longitudinal axis being at an angle of 30° with the horizon.

DISCUSSION.

Fleet Surgeon Lloyd, R.N., London: while entirely agreeing with Dr. Macdonald in most of his suggestions, expressed his belief in the impracticability of carrying out fixed rules for the locality for the first treatment of the wounded, or for their transport thither during action, in modern ships of war. The types and internal arrangements of these ships now differ so widely, and are changing so rapidly, that these matters must be left to the discretion of the commanding and medical officers in each individual case. For instance, in the last ship in which he had served the gun deck was long and narrow, and so obstructed for the carriage of wounded during action, by the machinery of the modern guns, &c., that it was necessary to have two stations for first treatment of the wounded, one forward and the other aft; these were in the lower sick berth and ward-room, and were so cut off from each other that the medical officers and assistants had to be separated into two parties. Two sets of bearers and ambulances had to be provided, one at each extremity of the gun deck, each working over half the space. As regards Dr. Gorgas's ambulance cot, he had had the description of it by him for about twelve years since he saw it first, and was glad at last to have had the opportunity lately of trying it practically. From an experience extending over a year, he could give it the most unqualified approval, and had made some small improvements or changes in minor details from the cot as shown in Dr. Macdonald's drawing exhibited. For instance, hinges on the double-inclined plane and a ratchet on the lower part of the frame made it better for stowage. Canvas loops, or beckets, five inches long, fixed to each corner of the frame, served as handles to carry it by, and when two stretcher poles, three feet longer than the cot, were passed through these, they extended eighteen inches at each end, and served as handles, thus converting the cot into a stretcher for carrying the wounded along the deck; on arriving at a hatchway these poles were withdrawn and the cot slid down the ladder by the lanyard. It was found the patient was safely fixed at any angle, The breast straps in two parts, appearing in the sketch, had also been simplified. A plain

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canvas band, two and a half inches broad, was attached inside to one of the angles at the head of the frame, and, passing round the man's body under his arms, was fastened by a toggle to a loop at the other angle. The toggle shipped in three holes in the strap, to suit different sized men; it could thus be fastened almost instantaneously.

Medical Director J. M. BROWNE, U.S. Navy: in endorsing the suggestions of Inspector General Macdonald, and wishing for the adoption of the same, recognized the impracticability of a fixed system of transportation of the wounded during battle, especially in vessels of the smaller class, as experience exhibits that too frequently in the excitement of battle the wounded are left to the resources of their comrades, notwithstanding the ample provisions made prior to the engagement. He mentioned among the ambulance lifts the one invented by Medical Inspector A. C. Gorgas, U.S. Navy, with subsequent modifications, which lift is employed in the U.S. Navy, and fulfils its purpose. Allusion was made to a recent invention of Medical Inspector B. F. Gibbs, U.S. Navy, which consists of an ambulance lift, a stretcher, or invalid chair, as desired.

Surgeon General MOUAT, V.C., C.B., London: I did not hear the whole of Inspector-General J. D. Macdonald's paper, and have not had time to make myself acquainted with the different mechanical contrivances he has brought before us. In my varied services on shore and board ship, it was my fate, when in medical charge of a regiment proceeding to the Crimea, to find myself in a storm in the Bay of Biscay, with several fractures to treat. The carcase of a bullock slung in the rigging broke loose, and caused something like the effects of a shell on a crowded deck. Had I possessed any of the contrivances mentioned by Dr. Macdonald, I could have moved and treated these men with greater advantage.

The PRESIDENT referred to the defective appliances in use for the carriage of the wounded on board ship after the battle of the Alma, and regretted that some of those now described were not then in use, as much suffering would have been saved.

Dr. Macdonald, Netley: read the following portion of a letter from Dr. DAVIS, Fleet Surgeon, R.N.:—

"When I fitted out the sick berth of the *Audacious* for service on the China Station in 1874, I had made for me an ambulance cot, which was used whenever we went to general quarters. The cot is the invention of Dr. Gorgas, surgeon of the United States' Navy. I enclose a drawing of it, and a copy of his descriptive letter to the Chief of the Bureau of Medicine and Surgery, Washington, dated December 6, 1869. I first saw it in U.S. S. *Brooklyn*, at Lisbon, and was struck with its advantages over appliances of the like nature, and convinced of its usefulness. A hinge on the double inclined plane, and a ratchet on the cot frame, so that the plane can be made level with the frame, and the angle of the plane altered to suit particular cases, are obvious improvements."

Fleet Surgeon LLOYD, London: could not agree with the opinion expressed in the letter just read, that "the power of altering the angle of the double-inclined plane to suit particular cases" was an improvement. The most minute directions as to height and inclination of the plane were given in Dr. Gorgas's description; and this appeared to be almost the most essential part of the cot. The angle given by Dr. Gorgas, while fitted to support any case of injury to the lower extremities, contributed chiefly to the fixation of the injured man in this cot, when inclined at high angles. He might add that Dr. Browne (Dr. Gorgas's colleague), had just informed him that Dr. Gorgas had not made any change in this particular.

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On the Special Training of Subordinates—whether Civil or Military—for Service with the Medical Department of an Army in the Field; and the Question of bringing Supplies to the Ground during, or immediately after, an Engagement.

Surgeon Major SANDFORD MOORE, A.M.D.

I may perhaps, be permitted to preface my remarks by saying how this paper originated. It arose out of a request on the part of no less a distinguished lady than Miss Nightingale to have the question discussed at one of the meetings of this Section, and the President's subsequent invitation to me to consent to put together a short paper, as he was good enough to say he considered this would be the best way of leading to a practically useful result.

A single glance at one of our mobilization tables will show the strength of the medical service of an army on a war footing. To an army corps of 36,805 men of all ranks, a medical department of 2,516 officers and men is allowed by regulation—for the purpose of succouring the wounded during action, removing them to the field hospitals, feeding, nursing, clothing, sheltering them, carrying out their transport to the base of operations, and, lastly, embarking them on board hospital ships or despatch vessels for conveyance to England. This number is made up of about 280 officers of different kinds; train officers, officers in charge of stores, but principally surgeons, and about 2,250 subordinates. Of these subordinates some are employed as clerks, some as packers, some as compounders, and some as cooks and stewards, and military nurses; while, as to the remainder, some are employed as officers' servants; 380 are stretcher bearers, and 440 are muleteers, drivers, and artificers of the train of the army.

Many of these subordinates require a previous course of special training—experience having shown that it is not possible to improvise an efficient staff of medical subordinates; and for this training the military surgeon is mainly held responsible.

In the various administrative offices in peace time the clerks acquire a knowledge of their special clerical duties. In the medical stores other subordinates learn how to pack drugs and instruments for transport. In the surgeries of the various military hospitals others are taught the uses and doses of medicines, the mode of dispensing them, and the best methods of preserving them; this instruction is spread over a period of at least six months; when reported competent by the medical officer in charge of the hospital, a board of medical officers examines the candidate as to his fitness for the appointment of compounder, and reports if he is duly qualified, which he must be found to be before he can be employed in that responsible capacity.

In the kitchens of the various military hospitals, other subordinates receive special instruction in cooking diets for the sick, and the preparation of drinks and extras; sergeant cooks, in addition, passing through the two months' course at the instructional kitchen at Aldershot camp.

At the training school of the Army Hospital Corps, stretcher bearers are put through their special course of instruction. The course seldom extends over one month. The training is partly theoretical and partly practical—a slight knowledge of such subjects as human anatomy, the early treatment of gunshot wounds, arresting hæmorrhage, &c., exercises in loading and unloading waggons, stretchers, mountain equipment and handling the wounded and their sick-transport conveyances generally, as well as pitching and striking hospital marquees and tents. After such a systematic course as

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this the soldier is well qualified to act as bearer, either with his regiment or in the ranks of the Bearer Company.

The hospital orderly or military nurse, having a wider range of responsibilities than the bearer, is naturally subjected to a more prolonged course of special training. The military nurse remains under training for at least two months with the medical officer instructor at the training school, prior to being sent for further training to the wards of a hospital. In addition to receiving the special training of the bearer, the nurse is put through a thorough course of bandaging, of instruction in the management of wards, administration of medicines, observation of the sick, nursing of helpless patients, and the use of field hospital equipment, before he is considered to be duly qualified.

Whilst the supply of bearers for our army is practically unlimited (the militia reserve furnishing to the regular army all that are likely to be required on mobilization) the same, unfortunately, cannot be said of the supply of military nurses. The A.H.C. and its slender reserve forms the sole source of supply. The present strength of the corps is not 2,000 men, and as about 1,000 nurses are required for each army corps on taking the field, the A.H.C. collected from every foreign station, and from every military hospital in the United Kingdom, supposing such were possible, would not prove more than sufficient in number to supply two army corps on mobilization; and, of course, were the eight army corps mobilized at the same time, there would be none at all for the last six.

In the presence of such a dilemma, civilian nurses must be taken to supply the places of the non-existing military nurses; but the War Office even then should not accept the services of civilians at random, but only those who can be satisfactorily shown to have received sufficient ward training, and who are properly qualified skilled nurses. As civilians they will, on arriving at the seat of war, be placed at the disposal of the surgeon general at the base of operations, by whom, as vacancies occur, they will be appointed to hospitals at the base or along the line of communications. They will never, of course, be employed in the front with field hospitals.

Such persons should be selected from the educated classes only, have undergone a six months' course of training in the wards of some surgical hospital; two of the six, if possible, being spent in a military hospital—Netley or Woolwich—so that these persons may not only be familiarized with field hospital equipment, but with military habits and discipline. As every one who has been called upon to exercise disciplinary control over subordinates knows, the habit of obedience to orders is a plant of slow growth, and requires inculcation, and the most serious impediment to the employment of otherwise efficient civilians is the want of military habits and training. It is to be hoped that the War Office will never, under any circumstances, be induced to commit the egregious blunder of accepting the services of sham nurses who have not undergone a sufficient period of ward training.

Measures for promoting the timely arrival of supplies for the use of the wounded.

In all battles of moment, the difficulty of obtaining for the field hospitals food, or what may be called subsistence supplies, and hospital supplies, is a matter of the greatest concern to the chiefs of the medical service of an army.

Vast numbers of men may be suddenly placed hors-de-combat, as was the case at Solferino (22,000), Gettysburg (28,000), or Gravelotte (30,000). Hours may elapse before the commissariat ration reaches the field hospitals, and days may elapse before the wounded can be removed from the field hospitals to points along the line of communication nearer to the base. At such times as these the supplies of the field

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hospitals, and the timely arrival of reserve supplies, are matters of primary importance to the wounded.

In smaller engagements, difficulties are seldom found to arise, for then the field hospitals, which are never beyond easy reach in case of an engagement happening, have stores in greater abundance than the emergency requires.

The causes of this difficulty are numerous:—

Sometimes it is partly due to the incomplete state of the army on starting—destitute or deficient of its proper amount of hospital stores and supplies.

At other times there is want of co-operation between the medical and other departments. For example, supply waggons standing idle in the front, which might with advantage be employed in bringing up supplies for the wounded in times of emergency, cannot be obtained for this purpose, in spite of the earnest appeals and remonstrances of the medical department.

Then, again, the reserve depôt of supplies may be situated at too great a distance from the front, and perhaps with an incompetent medical officer in charge. The stores may have been wasted, or the roads in bad order, or the wounded detained in the field hospitals for a longer period than was calculated upon. But perhaps the most frequent cause of difficulty arises from the powerlessness of the medical officer to have his orders enforced; many instances of these causes of difficulty are recorded in the medical and surgical history of the war of the rebellion in America.

There we find medical officers on many occasions forced to abandon their hospital supply waggons on one plea or another; sometimes, for want of transport, the commanding officers seized upon the supply waggons for the purpose of carrying ammunition; sometimes, and often, the hospital supply waggons were regarded as an incumbrance to the fighting force, and as soon as it became an object to reduce the amount of transport when moving near the enemy, the hospital supply waggons were the first to be ordered to be left behind.

The battle of Gettysburg was fought on the 1st, 2nd, and 3rd of July, 1863. On the 25th of June, the medical director with the army sent an officer back to Washington to bring up twenty-five waggon loads of such supplies as would be most required in case of a battle—believed to be then impending. On the 28th of June, this officer, with these supplies, reached Frederick—a place about thirty miles distant from the battle-field—and continued to move with the head-quarter train in the direction of Gettysburg until the 1st of July: on that day these supplies were not permitted to go forward any further, and on the 2nd of July were ordered to the rear, to Westminster, nearly twenty-five miles distant from the battle-field. The effect of this was to deprive the department almost wholly of the means for taking care of the wounded until the result of the engagement of the 2nd and 3rd was fully known.

The twenty-five waggon loads of hospital supplies did not, in consequence, reach the field until the evening of the 4th of July—twenty-four hours after the battle.

As regards the various causes of this difficulty, it is not unreasonable to require that any that admit of removal should not be permitted to continue to exist, for the troops have a right to the best precautions that can be taken for their protection, as well as for the safety of their lives. Firstly, then—

No army should be permitted to take the field unless complete in every respect as regards its medical service, and particularly as regards its supply and store waggons fo the movable field hospitals.

Secondly.—Instructions should be issued to the medical officers, with a view to stringent precautionary measures being taken to prevent waste or loss of any of the supplies.

Thirdly.—Upon the force moving away from its base, an advanced magazine of hospital

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supplies should be established at an accessible point on the line of communications, from whence the stores will be kept ready to be despatched to any place where needed.

The stores at the advanced depôt should be capable of supplying a large number of men, and include every kind of battle-field supply—tents, blankets, sheets, lint, medicines, cooking utensils, and concentrated food, milk and beef—and placed in charge of a medical officer, who should be specially selected for his known qualities as a trustworthy business man, and with energy in overcoming difficulties, especially those connected with transport. If this officer is held responsible for a sufficient supply being kept up at the advanced depôt, he should be allowed considerable freedom of action. He should have power to requisition on the base of operations for supplies, to arrange with the commissary department for transport to take his supplies to the front, or, if need be, with the approval of the surgeon-general in charge of the line of communications, to hire the necessary transport; and, of course, for the increased risk he will incur in giving receipts for public property, he should be compensated.

The complete chain of responsibility for supplying the field hospitals would include:—

An officer at the base responsible for the sufficiency of the supply of stores at the base, and for making the necessary arrangements for forwarding them, on demand, to the hospitals on the line of communications, and to the advanced depôt.

An officer at the advanced depôt, acting under the surgeon-general on the line of communications, responsible for a sufficient supply of stores at the advanced depôt, and for making arrangements for forwarding them to the bearer companies and field hospitals.

A field director, to requisition on the medical officer in charge of the advanced depôt, for stores required by the field hospitals.

Fourthly.—Measures for rapidly dispersing the wounded after a great battle will, of course, also, *pro tanto*, diminish the supply difficulty. An illustration may be given by an occurrence which took place at Metz, in 1870. Reserves from Wurtemburg were immediately brought into action round Metz. They came into action, were wounded, and conveyed by railway to the Stuttgart hospitals after a journey of less than twenty-four hours—actually only thirty-six from the time they came before the French, until they found themselves again in the heart of Germany, lying in their own town hospital.

Fifthly.—Of course, when an army is moving independently—maintaining no communication with its base—reserve stores cannot be counted on, unless the army is moving along a coast or river line, when supplies may be received from the ships or despatch vessels.

Lastly.—The most stringent orders should be issued by the highest authorities, and steps taken to ensure implicit obedience thereto, that, on the line of march, hospital supplies must not be left behind on any plea, nor the hospital vehicles used for any other than the original purpose for which they were intended.

For my own part I have but little faith in measures purely tentative. Till the medical officer is clothed with sufficient authority to carry out and enforce the instructions of the surgeon-general of the army—and which instructions have, in the first instance, received the sanction and approval of the general officer commanding—and till the transport is medical transport, not belonging to the train of the army—that is to say, its officers, medical officers, and the drivers and muleteers, Army Hospital Corps men—which will alone secure the services of the horses, waggons, and men to the army medical department in times of emergency; it is vain, in my opinion, in all cases to expect a timely arrival of supplies for the use of the wounded during, or immediately after, an engagement.

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DISCUSSION.

Surgeon General ROTH, of Saxon Army Corps: Gentlemen, will you allow me to make some remarks on bearer companies in different armies. The best principle is that which gives to the army *medical* officer a body of men who are under his command only, and who can, after the fight, be employed for clearing the battle-field, and giving other assistance, independently of the movements of the army. In the *English* army such a troop is already under the entire command of medical officers, and this organization is to be preferred to the bearers being taken out of the fighting troops, who are not always at the disposition of the medical authorities. In *Germany*, we have the command over the whole army medical service, except the ambulances (*Sanitäts-Detachements*), which, meanwhile, are at the disposal of the medical officer of division. That is the best organization which puts the command in the hands of those responsible for results. Of course this principle led to the general system of medical service which in England took the place of the regimental system. That was a great success, although I will not deny the new system has cost many sacrifices.

Surgeon General MOUAT, V.C., C.B., London: I have listened with much interest to the paper of Surgeon Major Moore on the training of subordinates for field service—in other words, the Army Hospital Corps. I am of opinion that in this, and many other matters connected with the "rough service of war," we are attempting too much—are harassing rather than assisting the military authorities. The medical service of the army, with its increased medical staff, its bearers, and band of trained nurses, male and female, is a small army corps in itself, adding to the difficulties of transport and supplies, and impeding the movements of the troops in the field.

Surgeon Major S. MOORE, Aldershot: said he wished to correct a misapprehension under which Surgeon General Roth appeared to labour. An English bearer company is not a homogeneous body of soldiers. A portion of the *personnel* is furnished by the medical department, whereas the transport, and the remainder of the *personnel*, is furnished by the commissariat department. The surgeon-major in charge of the bearer company exercises a general power of direction over the whole, it is true, but commands only the *personnel* of the military department; while the commissariat officer commands the *personnel* of the commissariat department, whom, consequently, the surgeon-major has no power to punish or control; in this respect differing materially from the surgeon-major in command of a Prussian field hospital. In this respect the organization of an English bearer company is still imperfect, and, in point of fact, it would be probably still more satisfactory if the transport of a bearer company were medical transport—that is to say, the drivers, Army Hospital Corps men—and not found by the commissariat department at all—a point which has been dwelt upon at some length in the paper.

Dr. TSCHUDI, Banjaluka: In the Austrian army the medical officers have the absolute command, and the right of punishment only in the sanitary railway trains and in the ship ambulances. In the bearer companies, whose service is from the skirmishing line, and in the sanitary companies, whose work extends from the dressing stations to the field hospitals, &c., the medical officers have no command at all. In the sanitary companies the sanitary officer has the command and right of punishment, and the train soldiers are under the train officer. The command of the medical officer of the sanitary railway trains and the ship ambulances is, so to say, the first step toward obtaining the same power in the field ambulances, hospitals, &c.; the desire of the Austrian medical officers is to get the command of the sanitary service in order to ensure its efficient working.

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Surgeon General Roth, Dresden: I have always read that the surgeon-major is in command of the bearer company, but feel satisfied with the answer of Surgeon Major Moore. If the principle is settled, the arrangement of details will certainly follow. I beg you to bear in mind that we are a young body, and our goal, that of being brought upon the same footing as the engineers, is not to be reached at once; but all organizations are following the same plan—*e.g.*, in the German regulations for the Army Medical Service many things are to be found resembling the British arrangements during the Afghan War.

The President: There can be no doubt that the whole bearer company in time of war is placed under the direct command of the medical officer in charge, by existing regulations. The surgeon-major commanding the bearer company commands every one in it; the transport staff, and all belonging to the movements of the transport, being included. He could not discharge the duties for which he is held responsible were it not so; for the Regulations hold the medical officer in charge of the bearer company responsible for its efficiency in all respects. I do not mean to say that the medical officer in charge of the company is to interfere with the technical details of the duties of the transport officer. But it will be on the medical officer that the duty will devolve of ordering this or that collection of wounded to be picked up and carried to the help stations, or a waggon or stretcher to be sent to this or that place, according to circumstances; and the transport officer, by the regulations, is required to carry out the directions so received from the medical officer. The command of the medical officer in charge is a general command over the whole company, very similar to that of a general officer over the different arms and departments of the service under his command.

On Gunshot Fractures of the Femur, based mainly on the results in 414 *cases treated at the same time in Letterman, U.S.A., General Hospital, on the battle-field of Gettysburgh, in the year* 1863.

Surgeon Henry Janes, United States Army.

The location and circumstances were so exceptionally favourable, that, from about 14,000 Union, and 8,000 Confederate wounded left under my command, I had some 1,800 or 1,900 of the most severe cases selected and put into one hospital, of which I took personal charge, with the view of keeping them under observation, and together as long as possible. The hospital was broken up after about 4½ months, and the cases were scattered among many distant permanent hospitals, but the histories were continued for about a year, so far as they could be followed up in the records of the Surgeon General's Office at Washington.

The results obtained seem to justify the conclusion that the rule, as laid down by the majority of writers on military surgery, to amputate in gunshot fractures of the middle and lower thirds of the femur, may be disregarded with advantage in a large proportion of the cases not involving the knee-joint, provided the patient can be treated near the battle-field.

The following table gives the results so far as are known at present:—

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Tabular Statement of the Results in 414 *cases of Gunshot Fractures of the Femur treated in Lettermant, U.S.A., General Hospital, July to Nov.* 1863. Henry Janes, Surg., U.S.A., in charge.

UPPER THIRD.

Treated Conservatively.				Treated by Amputation.			
		Discharged				Discharged	
Cases	Died	Cured	Not Cured	Cases	Died	Cured	Not Cured
90	43	19	28	1	...	...	1

MIDDLE THIRD.

103	35	29	39	20	7	12	1

LOWER THIRD.

66	13	23	30	134	47	62	25
259	91	71	97	155	54	74	27

Results in 98 fractures involving knee-joint:—

TREATED CONSERVATIVELY.

Cases.		Deaths.		Discharged not Cured.
32	...	28	...	4

TREATED BY AMPUTATION.

	Deaths.				Discharged.					
	Point of Amputation.				Cured.			Not Cured.		
	Cases.	U. 3d	M. 3d	L. 3d	U. 3d	M. 3d	L. 3d	U. 3d	M. 3d	L. 3d
Primary . . .	28	...	1	1	2	8	10	...	...	6
Intermediate . .	14	1	1	2	...	4	3	...	2	1
Secondary . . .	7	...	2	2	1	...	1	...	...	1
Date not stated .	7	...	6	1	...	5	1	...	3	1
Total amputated	66	1	10	6	3	17	15		5	9

Summary of Treatment:—If the knee-joint is implicated in the fracture, and *generally*, if the femur is comminuted more than six inches, it is best to amputate, primarily, if possible; but an intermediate amputation is better than to risk waiting for the secondary period.

If conservation is decided upon, remove at once all foreign bodies, and loose or *slightly* attached fragments of bone, but do not tear off pieces firmly attached to the periosteum, nor saw off the ends of the bones, nor probe unnecessarily, nor bleed to prevent inflammation. Remove necrosed fragments as soon as they are loosened from the periosteum. Use simple and comfortable retentive apparatus; generally, moderate extension with a weight and pulley, with sand-bags at the sides of the limbs to prevent rotation, is the best.

Do not attempt to entirely prevent shortening, unless where there is no comminution. If the bone is much shattered, let it shorten as much as it is likely to necrose. Death is more apt to occur than the regeneration of three or four inches of firm bone between the fractured ends.

Keep the wounds clean, using antiseptics freely. Prevent burrowing of pus by counter-opening, if necessary. Give plenty of good fresh air, the best of food, and, if there is much debility, stimulants in moderation. Do not move the patients more than

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is absolutely necessary; they will do better in tents, especially in the warmer parts of the year, than in permanent hospitals, and they never bear moving to distant hospitals without injury, unless tolerably firm union has taken place. If removal is absolutely necessary, keep the patient on the same stretcher from the beginning to the end of his journey.

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*A comparison of the Cylindro-Conoidal and Round Bullet Wounds.**

Surgeon GILBERT KIRKER, R.N.

From the knowledge, which, in the late war between the Russians and Turks, I gained of rifle gunshot wounds, the belief arose in my mind that the nature of these wounds was not fully understood, and that the opinion which held them to represent more tissue injury than round bullet wounds was erroneous.

The case, whose consideration called forth the elemental ideas of my belief, I shall at first refer to. In the latter end of July, 1877, was fought the battle of Yenisaghra. Many of the Turkish wounded were carried down to Adrianople, and early in August two hundred of them came under the care of the Stafford House surgeons. Among the patients who came under my care was a young man, about twenty-five years of age, with a compound fracture of the left thigh. The bullet had passed through from before, backwards, a little higher up than midway between the knee and the hip.

His treatment up to that time had been of the most meagre kind. Probably he had been carried from the battle-field to Adrianople without any fixing appliance to his fractured thigh, for, when I received him, he was wearing a splint from the Stafford House stores. That was a posterior wire splint, with foot-piece; too short, however, to properly support the fracture. I removed it, and placed his limb in M'Intyre's apparatus, applying short wooden splints at the sides and front of the thigh. The openings of the wound were dressed with lint dipped in carbolic lotion. This was all the treatment he received until the bone had united. Then, for a short time before he was allowed to move about, in order to remove, if possible, a little outward bend at the seat of fracture, I applied a Liston's splint. When he was discharged from the hospital there was a little deformity of the thigh, a little eversion of the foot, and an inch and a half shortening of the limb.

Among this man's two hundred wounded companions, there were many, who, like him, had compound fractures of the thigh; but in their wounds suppuration established itself, extended, and, I believe in every case, only stopped when it had drained away its victim's life. The happy issue, then, of my patient's case was due immediately to suppuration not taking place in his wound. A little bloody fluid—less from the opening of entrance than of exit—was the only discharge I saw; and it is not possible that suppuration had established itself, and again subsided, before he came under my observation. The small openings in the skin healed by suppuration and granulation, but the deep track of the wound healed by adhesive inflammation alone. I had been taught that no gunshot wound, especially one with fracture of bone, ever healed without suppuration of its track and sloughing of the bruised tissue.

Having from the foregoing case gained the knowledge that it is possible for a gunshot wound to heal by adhesive inflammation, as corroborative examples, other cases arose

* In the absence of the author, and owing to want of time, the abstract of this paper in the programme was all that was read to the meeting.

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and arrayed themselves before my mind. I shall mention two. In the first there was injury of bone, and in the second the bullet traversed the chest.

A soldier was admitted into hospital who had been shot through the right buttock at Plevna. His wound was healed, but he was suffering from paralysis of the corresponding lower extremity. The bullet entered behind, close to the hip-joint, and emerged in front, in the outer half of Poupart's ligament. He said that at the time of the accident a little blood flowed from his wound, but that it healed quickly, without afterwards discharging any matter. There was no doubt as to bone injury, for a displaced fragment of the acetabulum made a prominence beside the wound of entrance, and I believe, by pressing on the sciatic nerve, caused the paralysis of the leg.

In the second case, the bullet entered the patient's back, at the posterior edge of the right scapula, an inch and a half above the inferior angle, passed forward and came out in front, about two inches below the right clavicle. He made a quick recovery. No pus came from his wound, and he never exhibited symptoms of injury to his lung. That the lung must have been penetrated, however, was evident from the position of the entrance and exit openings of the wound, and from the frequent appearance of little bubbles of air under the granulations in front. There was too, probably, injury of bone.

After considering these cases, it seemed to me certain that I had observed gunshot wounds to heal in a different manner from that in which I had been taught they always healed. To what was that difference due? To imperfect observation, or to a difference in the nature of the wounds observed. I thought the latter theory was the true explanation. For the wounds which I had the opportunity of observing were made by the modern rifle bullet, whereas the wounds on whose observation were founded the doctrines I was taught, were made by the old round ball; and it appeared to me that the two bullets, reasoning from the different conditions appertaining to them as projectiles, would produce wounds with the required difference in nature. The obvious points wherein the two bullets differ, as regards the conditions appertaining to them as projectiles, are their shape and dimensions, their rate and mode of motion. Their difference in shape is expressed by their names; from their differences in dimensions the wounding area of the cylindro-conoidal is less than that of the round; the former wounds at a much higher rate of speed than the latter; and while the cylindro-conoidal rotates on an axis parallel, the round rotates on an axis transverse to the line of flight.

Owing to the differences in shape and dimensions of the bullets, the cylindro-conoidal affects a narrower area of the tissues, and makes its passage through them more by splitting, pushing aside, and lacerating, than the round does. Depending on these differences, too, there is less likelihood of foreign substances being carried into wounds of the former than the latter. The quicker a bullet travels in wounding, the more intense is its action, and the more limited and sharply defined is the area which it laterally affects. From this influence, the wound of the cylindro-conoidal bullet will be narrower, have more clearly cut walls, and contain more thoroughly disorganized tissue debris than that of the round. Again, owing to the difference in the mode of rotation, the cylindro-conoidal penetrates the tissues with greater facility, and consequently destroys less of them than the round. If then the wound of the cylindro-conoidal bullet, compared with that of the round, is narrower, less often contains foreign substances, and has a sharper line of demarcation between the injured and uninjured tissues, it has by nature a greater readiness to heal.

I shall now describe the experiments which I made to test the conclusion stated above.

One fore leg and one hind leg of a horse were the material on which I operated. I fastened them consecutively on a board, which I placed against a tree. At the distance of eight or nine yards I fired two round bullets from a fowling-piece into the fore-leg.

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Each bullet weighed 435 grs., and was ⅝ inch in diameter. At about the same distance I shot five cylindro-conoidal bullets into the hind leg. Each of these bullets weighed 198 grs., and was ¾ inch in its greatest transverse diameter.

In dealing with the comparison of the wounds made by the two bullets, I shall take up, in order, the openings of entrance and exit, the tracks through the soft parts, and the fractures of the bones.

The entrance and exit openings made by the cylindro-conoidal bullet are smaller than those made by the round, to a less degree than is accounted for by the smaller wounding area of the former. This is owing, no doubt, to the differences in shape of the wounding surface, and the modes of rotation of the two bullets.

The distinctive characters of the entrance and exit openings are similar in the wounds of both bullets, but are more apparent in the wounds of the round. The wound of entrance is circular, and has its margins inverted; the wound of exit is of irregular shape, is often the centre of radiating fissures, and from it subcutaneous tissue protrudes. As regards the wounds of cylindro-conoidal bullets, though immediately after their production it is always possible to tell the opening of entrance from that of exit, very soon changes take place, and if the bullet in its passage from one to the other has not been altered in shape, or has not carried a piece of bone before it, it becomes impossible to say at which wound it entered, or from which it emerged. The wound of exit, unless when pieces of bone have been carried into it or its vicinity, as a rule heals before the wound of entrance.

I come now to compare the tracks of the two bullets in the soft parts. The differences as to size and loss of substance which exist between the openings they make in the skins also exist between their tracks in the soft parts; and in addition, the track of the cylindro-conoidal, owing to quicker wounding action, has more cleanly cut walls than that of the round. The track of the round is often patent owing to its greater size, while, on the other hand, the track of the cylindro-conoidal is so small that it is often closed by approximation of its sides. An occurrence, which no doubt has an influence on the closure of the cylindro-conoidal bullet track is, that in traversing a substance such as muscle or wood, in which there is a parallelism of the constituent fibres, this bullet sometimes slips through between, and does not break across the fibres. Thus, the track through the soft parts is different in the subcutaneous cellular tissue from what it is in the muscle. In the former tissue the constituent fibres are reticulated; in the latter, they are parallel. In the former, the track is circular, and, if not actually patent, is easily opened; in the latter, the track is slit-like, and its edges come closely together.

In the last place, I have to compare the fractures produced by the two bullets. In the second of the two wounds made by the round bullet, the entire thickness of the bone was smashed, and in the first, only one unmoved splinter preserved the continuity of the bone. In both, numerous free and displaced splinters were produced; many of the former were carried into the tracks of the wounds, and some beyond, into the board and tree. Only a few unmoved splinters were left; and of these, only that one above referred to served in any way to bridge across the fracture. In addition, the bony margins bounding the broken gaps, on the sides which the bullets struck, were stripped of periosteum from half-an-inch to two inches in width. The cylindro-conoidal bullet produced free and displaced splinters, few in number and small in size; but by extensive fissuring it gave rise to many and long unmoved splinters, held together by periosteum.

It is thus, I believe, evident that fractures caused by the cylindro-conoidal bullet will be less likely to excite suppuration than those produced by the round bullet. In the fractures of the former there is little comminution from the crushing of the bullet,

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but a good deal of fissuring from its wedge-like action. In the fractures of the latter there is a great deal of comminution and little fissuring. Splinters caused by fissuring, if not subjected to rough usage, and if not steeped in the fluids of foul wounds, preserve their connections with the parent bone and soft parts, and live; but comminuted splinters are mostly crushed, and have their vital connections severed at the time of the accident.

Because the cylindro-conoidal bullet, as a rule, wounds at a greater velocity than the round, it is, therefore, by most authorities, said that, *cæteris paribus*, it produces a greater amount of tissue-injury. I not only hold, however, that this opinion is incorrect but that reason will build, on the same fact, just the opposite conclusion. The disturbance or injury to a body penetrated by a projectile is in proportion to the loss of momentum the projectile suffers; the resistance of the one is overcome by a proportionate expenditure of force by the other. The great characteristic of a rifle projectile is that, owing to its shape and mode of rotation, it penetrates substances with the minimum expenditure of force. It therefore conserves its momentum, and also the varying factor of its momentum, its velocity.

That the greater wounding velocity of the cylindro-conoidal bullet is due to the characteristic facility with which it penetrates, and not to greater initial velocity, is proved by the fact that the modern rifle expels its projectile with less initial velocity than some of the old smooth-bore firearms. Therefore, since the cylindro-conoidal bullet retains a greater wounding velocity than the round, it penetrates with less expenditure of force, and in a given wound produces less tissue injury.

Again, speaking with special reference to injury of bones, the notable peculiarity in the wounding action of the cylindro-conoidal bullet is the great amount of splitting and fissuring produced; and my experiments showed that the resulting unmoved splinters have intact their periosteal and other soft tissue connections. Under careful treatment these splinters will reunite; therefore, part of the force expended by the cylindro-conoidal bullet in wounding has been rendered comparatively harmless.

As a rule, gunshot fractures are produced by bullets clearing for themselves a passage through the bones; and it is evident that, in a fracture in which there are both comminution and fissuring, part of the force expended has produced one, and the remaining part the other variety of injury. Now, supposing the force necessary to fracture a certain bone constant, the more a bullet, in perforating it, fissures, the less it comminutes, and *vice versâ*. This conclusion is a truism, but I have introduced it because I have been able, almost as near as it is possible, to demonstrate it in gunshot fractures of the vault of the skull.

While experimenting with reference to the relative amounts of splintering at the entrance and exit apertures of penetrating fractures of the skull, I noticed that when the force was applied to the inner surface of the bone, fissures were more apt to be produced than when it was applied to the opposite side. Subsequently, I made this accidental result the subject of examination. I fired a small rifle bullet, such as I used in the experiments on the horse's legs, through a number of calvaria, aiming sometimes at the inner side and sometimes at the outer. These experiments not only showed that fissures were oftener produced under the former circumstances than the latter—being the rule in one, in the other the exception; but, again by accident, I observed that the hole with fissures which the bullet made in passing from the inside was less in size than the hole without fissures, which it made while passing in the opposite direction.

The occurrence and non-occurrence of fissures in the above instances are owing, I believe, to the vault of the skull having the construction of a dome. Its shape is irregularly dome-like; and like a dome, too, it is completed at the top by the closing of the

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fontanelles. Clearly then, it will longer maintain its integrity against external than internal violence. When a bullet penetrates the vault of the skull from the outside, the structure does not yield widely to the concentric force, and no fissures are produced; but when it perforates from the inside, the dome cracks under the excentric force. It may be for the same reason that, in fractures of the base of the skull, where the force is applied to the concavity of the basilar dome, fissures are so common.

Having compared the fractures of the cylindro-conoidal and the round bullets, I wish to refer to the action of the former on the different kinds of bone. When a cylindro-conoidal bullet strikes cancellous bone, such as the upper or lower end of the femur, the fragile structure yields and breaks before it, is partly carried forward, and partly crushed against the walls of the track. A moiety of what is crushed laterally is destroyed, while the remainder is saved by the elasticity of the tissue. The latter portion returns to its place, and a tunnel remains, less in diameter than the diameter of the bullet which formed it. There is no fissuring, but the exit opening has a bevelled margin. The bullet expended just so much force as cleared its way, and this it did partly by breaking down, and partly by exercising the elasticity of the bone which came under its influence. As regards the amount of injury, the elasticity neutralized that force which brought it into action, and hence the narrow diameter of the passage formed.

Immediately under the articular cartilage, there is a layer of dense, granular-looking bone, not unlike imperfectly dried putty. When this is struck by a cylindro-conoidal bullet it breaks up into cuboid splinters, or becomes traversed by wide, gaping fissures. These fractures completely destroy the joints. I have seen one thus involving the astragalus.

When a cylindro-conoidal bullet strikes the compact shaft of a long bone, the force it expends in making its passage through produces extensive fissuring and little comminution. Many and large unmoved splinters; but few and small displaced and free ones result. So much force has been spent in fissuring that, I believe, the area of the tunnel formed by comminution is less than that of the posterior end of the bullet.

In the fractures of the shafts of long bones, as a pretty constant occurrence, four fissures radiate from the entrance and exit openings. Two run to opposite sides, obliquely upwards, and, similarly, two downwards. Fissures are due to masses of bone yielding to a slight degree their original positions, under the influence of the fracturing force; and therefore, in every case, their direction depends on the manner in which the bone is struck and the nature of its resistance. The not uncommon occurrence of four oblique fissures evidently depends on two lateral pieces of the shaft yielding.

Fissures from factures of the shafts of bones frequently run into the neighbouring joints. In my experiments they did so in two out of three cases. Their courses may be, either direct from the point struck, or from the place of junction of two or more intervening fissures.

It may be here stated, as a generalization regarding the invasion of joints by fissures, that a joint is much less likely to be involved when the bullet passes close to it through the true cancellous bone, than when, at a greater distance, it fractures the partly or completely compact bone. I have mentioned a healed wound of the thigh, close to the knee, in which the position of the cicatrices showed that the bullet must have passed right through the bone; yet, at the time, I believed the course of the bullet was circuitous through the flesh, for I knew not that the lower end of the femur could be perforated by a bullet without some injury to the knee-joint. Again, in Turkey, I saw numerous cases where the lower ends of the tibia and fibula had been shot through without injury to the ankle-joint; and at Netley, there were, among soldiers wounded in the Zulu war, one or two such injuries of the lower end of the femur.

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From what I have said of the wounding action and resulting tissue-injury of the cylindro-conoidal and round bullets, it is evident that a given wound of the former will heal more readily than one of the latter. It must be pointed out, however, that in this comparison there is no pretension to taking account of all the results of the wounding power and action of the two bullets; only so far as these are represented in any given wound have they been compared. No notice has been taken of the cylindro-conoidal bullets wounding at greater distances, nor of their frequently wounding more than one soldier, nor of their frequent penetration of cavities. A comparison in reference to these results would show that the cylindro-conoidal produces a greater number of wounds, and more of a direct and fatal character than the round; and on the complete comparison could be founded the statement that, though the cylindro-conoidal bullets produce a greater number of wounds, and more of a grave and fatal nature than the round, yet, other things being equal, wounds of the former heal more readily than wounds of the latter.

In conclusion, if the nature of cylindro-conoidal bullet-wounds is such as I have represented it to be, from it important indications may be drawn as to their early treatment. For, though there may be openings in the skin, if the tracks in the soft parts are closed up; if it is probable there is no lodgment of foreign substances carried by the bullets; and if, when there is fracture of bone, almost all the splinters have their soft tissue connections intact, no wound should be disturbed by exploring it with a finger or probe, and a wound involving bone should be made immovable at the earliest opportunity; or, above all, it should be treated at the nearest possible place to where it was nflicted.

Again, I believe, that much of the late success in the antiseptic treatment of gunshot wounds is due to the wounds having been produced by cylindro-conoidal bullets, and to their receiving the diligent care and freedom from meddling characteristic of the antiseptic method.

Some Observations on Wounds Inflicted by the Bullets of the Martini-Henry Rifle.

Surgeon General Professor T. Longmore, H.P., C.B.

In the summer of 1879, Dr. Kirker, R.N., brought to my notice some observations he had made experimentally on certain differences in the characters of wounds inflicted by spherical bullets, and those by conoidal bullets. These differences were quite reconcilable with the conditions of the experiments instituted, especially as regards the nature of the objects fired at, the relative dimensions and weights of the projectiles employed, and the distances at which the objects were hit by them.

One of the observations made by Dr. Kirker was, that though bones penetrated by the conoidal bullets were extensively fissured, the periosteal investment of the fragments was not much disturbed; and Dr. Kirker was led to surmise that, in some instances, the cures without suppuration of fractures of bones caused by conoidal bullets, which had been attributed to special modes of treatment, might have been chiefly due to the fragments having been maintained *in situ* in the manner just mentioned.

That fractures of long bones by conoidal bullets are usually accompanied by extensive fissures, and that some of the fissured fragments are held *in situ* by the periosteum, are acknowledged facts; but that the maintenance of the fragments by their periosteal

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covering should occur so largely as to enable the wound to become healed without suppuration was new to me.

It occurred to me that it might be useful to carry out a few experiments with the hardened bullet of the Martini-Henry rifle in present use, for the purpose of comparing its effects with those of the bullet used by Dr. Kirker, as described by him, and also with the effects previously observed by myself of the Enfield rifle bullet. It was particularly as to the amount of local comminution of the bone, and the extent to which the fissured fragments were kept *in situ* by the periosteum, that I thought observations might be useful.

The results of the trials I accordingly made were briefly as follows:—

1.—The Martini-Henry bullet causes considerably more laceration and contusion of the soft tissues, with more comminution and displacement of bone at the site of impact, than were observed in the experiments described by Dr. Kirker.

2.—The area of comminution is not so extensive, and generally the fragments are not so widely scattered, with the Martini-Henry bullet, as occurred in corresponding experiments made by me with the Enfield rifle bullet, ·55 inch in diameter, of soft lead.

3.—A larger proportion of fragments at the part of the bone struck by the Martini-Henry projectile, retain their periosteal connections than when the softer Enfield bullet was used; while long fragments, above and below the site of impact, though detached by fissuring from the adjoining bone surfaces, are maintained in close apposition by the periosteal investment.

4.—It was noticed, in one experiment, that though fissures extended completely through the apophysis of the bone, the synovial membrane, covering the joint surface, remained intact. The fissures extended to the joint, but the joint was not opened.

Conclusions:—The state of the bone wounded by the hardened Martini-Henry projectile appears to offer more opportunities of repair than if the fracture had been caused by an Enfield rifle soft leaden bullet.

When the grave importance of the long fissured fragments being retained in juxtaposition, and in their normal relations to the shaft of the fractured bone is remembered, and the further importance of the periosteum covering them being kept intact; these experiments point very strongly to the urgent necessity of completely *immobilizing* such a fracture from the onset.

The special importance of immobilizing gun-shot fractures, which are supposed to be accompanied with fissures extending into a neighbouring joint, is also rendered very manifest; for in any case where the extremity of the bone is split, but the synovial capsule of the articulation happens to be still complete, it is obvious that a small amount of rough movement may cause the synovial membrane to be torn along the line of fissure in the bone, and a communication to be established between the seat of fracture and the interior of the joint. The gravity of the injury would be very largely increased should this happen.

It may be useful to furnish a description of the injuries done to the soft tissues and bones in the experimental trials which led to the conclusions embodied in the paper. The rifle and projectiles used were of regulation pattern. The trials were five in number, and the results of them were as follows:—

Experiment 1.—Martini-Henry bullet, fired from a distance of 12 yards, through lower part of thigh. Full charge of powder.

Wound of Entrance.—On inner side of thigh, about 6 inches above the upper margin of head of tibia. Opening a little less than ·45 of an inch in size. Complete loss of substance of skin at the opening.

Wound of Exit.—One inch and a half in length and lacerated. Muscular tissue pro-

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truding from it. A second opening, smaller in size, appears near to it, where the skin has been pierced by a spicula of bone.

Track of Bullet.—On incising the parts so as to expose the wound, the soft tissues in the track of the bullet are seen to be much contused and lacerated. A cavernous hollow space in the muscular tissues contains small fragments of bone sticking in its walls.

Condition of Femur.—Complete fracture, about the junction of the middle and lower thirds of the bone. Part of the shaft, about two inches in length, is broken into fragments. Eight of these fragments in the direct line of passage of the bullet are still held together by periosteum. The chief fragment forms a bridge across a gap beneath, and is attached by periosteum to the shaft above and below. On bringing the eight fragments into their original position, the cylinder of the bone is nearly completed by them. Small fragments, equalling together the size of a florin, are completely detached. Three fissured fragments above, and two below, remain *in situ* in the remaining parts of the shaft, and are invested by periosteum.

Experiment 2.—Bullet fired at a distance of 6 yards, through middle of leg. Full charge of powder.

Wound of Entrance.—Outer side of limb. Small punched out vacant opening; about ·45 of an inch in diameter.

Wound of Exit.—Enormous lacerated wound, with muscles and fragments of bone protruding.

Condition of Bones.—Tibia and fibula completely fractured, rather above their centres. Extent of complete fracture about 2⅓ inches. Fragments very numerous.—*Tibia:* A strip of periosteum connects the upper and lower fragments of the tibia, but no bone. Many of these fragments, though loose, are connected, more or less completely, by periosteum, and some of these fragments are of considerable size. When replaced in position, they occupy about half the cylinder of the bone, the other half being vacant—*Fibula:* Fractured completely across at level of passage of bullet. The bone is split down into two nearly equal parts for 3⅓ inches. These two fragments are still held *in situ* by periosteum. One small fragment is loosely attached to the upper part of the fibula.

Experiment 3.—Bullet from 6 yards off, through lower part of thigh. Charge reduced to one-half.

Wound of Entrance.—On outer side of limb, about 2⅓ inches above line of junction of end of condyle and upper edge of tibia. Opening in skin about ·45 inch; substance lost, as if punched out; edge cleanly cut, and darkened by powder.

Wound of Exit.—Over centre of inner condyle. About equal in size to wound of entrance. Edges starred and lacerated. On subsequently removing the skin around the wound of exit, and bringing the edges together, there is found to be scarcely any, if any, loss of substance in it.

Condition of Bone.—The bullet has entered near the junction of the shaft and the cancellated structure of the condyle, and has left a punched, tunnel-like opening, extending downwards and inwards. The bullet has made its exit at the anterior part of the inner aspect of the inner condyle. The opening here is more elongated, and has its fibrous investment lacerated, its edges presenting a fringed appearance.

Both condyles are separated from the shaft, but are held closely together by periosteum. Line of separation very irregular, and embracing several fragments. Fissures are seen extending into the inter-condylar surface, to which the patella is applied, *but the synovial surface is not opened.* Patella not injured.

Experiment 4.—Bullet fired from 6 yards off, through top of leg. Charge of powder reduced to one-half.

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Wound of Entrance.—Over outer aspect of head of tibia, 1½ inch below level of joint. Small cleanly punched out opening.

Wound of Exit.—At upper part of calf, about 1 inch lower in level than wound of entrance. Opening similar in size to that of entrance, but the edges more lacerated, and some areolar tissue protruding through it.

Condition of Bone.—Bone struck on a level with the tubercle, and near it, on its outer aspect. The bone is ploughed near its surface, a rough groove being made, having a depth of about half an inch, passing backwards and slightly downwards, and terminating at the posterior aspect. A fissure extends from the grooved fracture down the shaft to the middle of the bone.

Experiment 5.—Bullet fired 6 yards off, at middle of leg. Charge reduced to one-half.

Wound of Entrance.—Anterior and inner aspect of leg, 5 inches below level of knee-joint. Sharply defined, punched hole, about ·45 of an inch in diameter.

Wound of Exit.—On outside of limb, nearly on same level as wound of entrance, 2½ inches distant from it. The opening of exit is 2¼ inches long, with lacerated edges. There are two other openings caused by spicula of bone, one lacerated, the other punctured.

Condition of Bones.—Complete fracture of tibia. Fibula entire.—*Tibia:* a semicircular gap marks the entrance of the bullet near the spine of the tibia. The fragment that would bridge this gap across has been carried away. The lower margin of the gap is formed by part of the edge of an elongated fragment, three inches in length, completely detached from its bony connections, but held firmly *in situ* by periosteum. A few fragments of bone, seven in number, entirely detached and carried away from their normal positions, were collected.

DISCUSSION.

Dr. Reyher, St. Petersburg: expressed his thanks to the President for pointing out that in the treatment of gunshot fractures so much depends on careful immobilization, and especially so because there is the danger of this point being possibly overlooked from an analogy being instituted between them and fractures occurring in civil practice, where immobilization, in our antiseptic era, need not be so strictly insisted upon. But fractures in civil practice *never* exhibit such extensive comminutions as those met with in war; and for the latter, therefore, even when their course remains aseptic, there is much greater danger that, through insufficient immobilization, many of the numerous fragments may become necrotic, because the periosteum bridging them over and holding them together may get torn. Dr. Reyher begs permission to point out that, in his opinion, based on the experiments of Dr. Bornhaupt, and the examination of his collection of preparations from the late Russo-Turkish war, the degree of comminution and its extent depend much less on the physical properties of the projectiles, than on the configuration of the injured bone. As proof thereof may serve the extensive splintering of *tubular* bones, and the frequency of canal-like perforations of their epiphyses by bullets, whereas similar perforations of the shafts of long tubular bones, that is, *simple perforations, uncomplicated by fissures*, are hardly ever observed.

Brig. Surg. Becher, London: points out the occurrence, during the Franco-German war, of perforating bullet wounds of bones—cleanly cut holes without fracture or fissure of the adjacent parts of the bone, in German described as "Lochschüsse." He has observed these almost in every bone of the body—most notably the tibia, in which he saw it in the upper, in the middle, in the lower third, from front to back, from side to side, likewise in

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the humerus, upper and lower portion; of flat bones he has seen perforations of the pelvis as cleanly cut as a bullet-hole in a window-pane. The importance of this kind of bone wound, as regards diagnosis, prognosis, and treatment, is self-evident, and not to be further dilated upon here. As far as Dr. Becher's experience goes, he believes the occurrence of "Lochschüsse" was *confined to the Chassepot bullets;* whereas the German needle-gun appeared to have shattered the bones more or less injuriously in every instance.

Mr. J. H. Casson, London: The bullets employed by the Turkish Army were of soft lead, and of an expansive character—possibly this accounts for the great amount of comminution of fractures observed by Dr. Reyher in Russian soldiers. Perforation without comminution of all bones was very common among Turkish wounded. One case was observed of knee-joint perforated in this way, which did well under modified antiseptic treatment.

Surgeon General Mouat, V.C., C.B., London: We are all much indebted to our President for the interesting paper on fissure in long bones by the modern rifle and conical bullet. I well remember the first case of this nature in the Crimea. I was puzzled how to act, whether to amputate higher or leave the case to the efforts of Nature; where I followed the latter course, it too often led to secondary amputation, necrosis, or pyæmia. This is a point on which the author of the paper has not touched, but I quite agree with him, so far as my experience in the Crimea goes, while in medical charge of the principal field hospital for wounded, that no well-established case of recovery of injuries of large joints, when fissured into the synovial capsule, can be produced from the record of our cases.

Dr. B. Howard, New York, expressed his appreciation of the paper of Professor Longmore, in that our knowledge of the peculiarities of fractures by the respective missiles, always vague, has thereby been rendered more accurate—next, because of the further evidence his experiments afforded, that fracture might be quite extensive, and yet the periosteum covering them remain intact; chiefly, however, the value of this paper was its encouragement to conservative treatment, and to make us content with less extensive removal of undetached fragments.

Surgeon Major Starcke, Berlin: I should like to direct the attention of the army surgeons present to some dangers to which soldiers are exposed during peace. In Germany, during the shooting exercises, soldiers use for practice very small bullets, often without knowing that this small mass of lead is so very effectual for harm. One of my friends made experiments to elucidate ten sad cases which he had the misfortune to meet with, and he found that these little bullets can perforate at a distance of twenty paces the metallic parts of the helmets, and not only the soft parts of the body, but also the bones.

Ueber primäres Debridement der Schusswunden.

Dr. Carl Reyher, St. Petersburg.

Die Frage, ob die Behandlung der Schusswunden, insbesondere der durch Kleingewehrprojectile erzeugten Schusscanalwunden mit dem Debridement einzuleiten ist, scheint mir der Discussion werth, weil, wie es scheint, die Ansichten über diesen Punkt auseinander gehen. Professor Gurlt spricht sich in seinem neuesten Werk: "die Gelenk-Resectionen nach Schussverletzungen," pag. 1225 u. ff., entschieden für die so

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früh als möglich unter antiseptischen Cautelen vorzunehmende ausgiebige Erweiterung aller Schusswunden aus. Ausnahmslos soll jede Schusswunde in ihren Oeffnungen gespalten, und ihr Canal bis an die Grenzen anatomischer Möglichkeit freigelegt werden. Derselben Richtung scheint Prof. Volkmann zu huldigen; wenigstens wird jeder, der sich an seine Lehren über die Behandlung der Civil-Frakturen hält, geneigt sein, jede Schussfraktur der Extremitäten in gleicher Weise zu behandeln.

Anders Prof. Esmarch, von dem der Vorschlag ausgeht, zuerst die Schorfheilung zu versuchen, und erst bei eintretender Reaction das Debridement vorzunehmen.

Die ersteren gehen von der Annahme aus, dass in jeden Schusscanal septische Stoffe hineingetragen sein müssen, letzterer stützt sich auf die Thatsache gelungener Schorfheilung nicht erweiterter, nicht drainirter Schussfrakturen; erstere fürchten, dass das Debridement bei schon eingetretener septischer Reaction häufig erfolglos sein wird, letzterer wiederum ist in seinen Hoffnungen kühner.

Gestatten Sie nun mir, aus den Beobachtungen und Erfahrungen, welche ich während des letzten russisch-türkischen Krieges sammeln konnte, das vorzulegen, was für die in Rede stehende Frage von Werth sein kann.

Wenn wir zunächst von der praktischen Seite dieser Frage, d. h. der Durchführbarkeit des primären Debridement in genere auf Verbandplatz und erster Lazarethlinie, absehen, und die Frage ganz prinzipiell behandeln, so haben wir zu allererst mit folgender Thatsache zu rechnen: eine ganze Reihe von Schussverletzungen der Extremitäten heilt ohne Debridement, ohne Drainage—unter dem trocknen Schorf, und zwar von diesen ein Theil ohne jegliche local oder allgemein septische Reaction, und ein anderer Theil mit nur ganz geringen, weder Leben noch Glied des Verletzten in Gefahr bringenden, meist erst spät eintretenden Nachstörungen (Circumscripte Eiterungen, Exfoliationen von kleinen Sequestern, Elimination von temporär eingekapselten Fremdkörpern, &c.). Die Häufigkeit solcher Schorfheilungen scheint nicht gering zu sein, jedenfalls nicht so gering, dass man dieselben in die Kategorie der Ausnahmen stellen könnte. Wenigstens habe ich unter 376 Fällen von Schussfrakturen der langen Röhrenknochen und penetrirenden Gelenkschüssen der sechs Hauptgelenke der Extremitäten—48 Fälle von Schorfheilungen beobachtet, oder, wenn ich correcter Weise einen Fall von Fussgelenkverletzung noch in Abzug bringe, weil derselbe späterhin doch noch zur Resection und Amputation kam—47 Mal, d. h. genau in ein Achtel aller Fälle.

Noch vielmehr würde das Verhältniss zu Gunsten der Schorfheilung sprechen, wenn ich die zahlreichen Schorfheilungen der Weichtheilschüsse der Extremitäten hinzugezogen hätte; ich möchte jedoch heute nur bei den Schussfrakturen und Gelenkschüssen der Extremitäten bleiben, und lege in beifolgenden Tafeln Tabellen vor, zu denen die detaillirten Krankengeschichten demnächst in einem Artikel der "Revue de la médecine et chirurgie militaire (Prof. Delorme), Paris," erscheinen werden.

Tafel II. enthält die in Rede stehenden Fälle von Gelenkschüssen und Schussfrakturen, welche unter dem Schorf ausgetheilt sind, und bedarf kaum einer Erklärung. Ich füge nur hinzu, dass von den 48 Geheilten, nur 7 Patienten mit Temperaturen über 38·5 C. gefiebert haben, und 3 von diesen ohne Localstörungen (kaukasisches Malariafieber?). Ebenso ist der Erwähnung werth, dass von diesen 47 unter den schorfgeheilten Fällen, keiner vor Ablauf von vier Wochen als geheilt entlassen ist, dass überhaupt nur wenige vor Ablauf der zwölften Woche (nämlich nur 16 Fälle), und alle übrigen, also die Mehrzahl, drei bis neun Monate in Beobachtung gewesen sind, ja einige derselben von mir noch später und noch in jüngster Zeit in Petersburg wieder untersucht worden sind.

Mögen durch Fehlerquellen, auf die hier einzugehen zu weit führen würde, und welche zu beseitigen, mir leider unmöglich ist, auf Tabelle II. die Fälle misslungener Schorf-

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TABELLE I.

Generalsumme aller 376 *von mir im russisch-türkischen Kriege beobachteten Schussfrakturen* (105) *und Gelenkschüsse* (271) *der Extremitäten.*

	Summa.	Geheilt.	Gestorben.	Letalitäts per Cent.
Schultergelenk	33 { 24	19 { 15	14 { 9	42.4 { 37.5
Oberarmhals	9	4	5	55.5
Oberarmdiaphyse	11	6	5	45.4
Oberarm dicht am Ellbogengelenk	59 { 12	36 { 9	23 { 3	38.9 { 25.0
Ellbogengelenk	39	22	17	43.5
Vorderarm dicht am Ellbogengelenk	8	5	3	37.5
Vorderarmdiaphysen	4	2	2	50.0
Handgelenk	21	11	10	47.6
Hüftgelenk	13	0	13	100.0
Oberschenkel	44	13	31	70.4
Kniegelenk	96	33	63	65.6
Unterschenkel	46	34	12	26.0
Fussgelenk	49	26	23	46.9
105 Schussfrakturen 271 Gelenkschüsse	376	180	196	52.1

TABELLE II.

Alle penetrirenden Gelenkschüsse und Schussfrakturen der Extremitäten, bei denen (57) *die Behandlung im Sinne der Schorfheilung nicht nur auf dem Verbandplatz eingeleitet, sondern auch in den Feldlazarethen fortgesetzt wurde und* 47 *Mal bis zu definitiver Heilung durchgeführt werden konnte.*

Summa.		Geheilt.					Gestorben.	
		Complet unter der trocknen Schorf ausgeheilt oder mit nur oberflächlicher Secretion der Haut-Schuss-Oeffnungen.	Localisirte Eiterung im Schuss-Canal; aber extraarticulär und nicht bis an die verletzten Knochen sich erstreckend.	Canaleiterung bis in's Gelenk und die Bruchspalten des verletzten Knochens hinein.	Spätabscedirung mit Elimination von Fremdkörpern und kleinen Sequestern.	Summa.	Summa.	Letalitäts per Cent.
0	Schultergelenk	0	0	0	0	0	0	0
3	Oberarm	2	1	0	0	3	0	0.0
4	Ellbogengelenk	2	0	0	1	3	1[3]	25.0
1	Vorderarm	1	0	0	0	1	0	0.0
0	Handgelenk	0	0	0	0	0	0	0
2	Hüftgelenk	0	0	0	0	0	2[4]	100.0
13	Oberschenkel	10	1	0	0	11	2[5]	15.4
18	Kniegelenk	15	1	1	0	17[2]	1[6]	5.5
8	Unterschenkel	3	1	0	2	6	2[7]	25.0
8	Fussgelenk	6	0	1[1]	0	7	1[8]	12.5
57		39	4	2	3	48	9	15.7

[1]) In der 5ten Woche Fieber, Phlegmone, Gelenkresection und später noch Amputation.

[2]) Von diesen ist bei einigen das Hämarthron pungirt und antiseptisch ausgewaschen worden.

[3]) Septische Reaction, Intermediär-Resection, danach Transport, acut progrediente Phlegmone-Amputation.

[4]) Todesursache beide Male: acut progrediente Phlegmone.

[5]) Todesursache in einem Fall: acut progrediente Phlegmone nach erfolglosem Debridement im Intermediär-Stadium (Zeugstück im Schusscanal); im anderen Fall: Pyämie im 5ten Monat nach vollständig aseptischem Verlauf während der ersten zwei Monate, später progressive Eiterung.

[6]) Acute Anämie, Blutung aus Arteria und Vena poplitea.

[7]) Todesursache im einen Fall: Pyämie in der 10ten Woche (blinder Schusscanal, Projectil, Zeugstück, progr. Eiterung); im anderen Fall: acut progrediente Phlegmone, im Anschluss an progressive Eiterung, welche sich nach 2 Monate aseptischem Verlauf entwickelt. Amputation.

[8]) Fussgelenkwunde complet unter dem Schorf geheilt. Todesursache: Acut progr. Phlegmone eines Oberarmschusses.

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behandlung, zu niedrig beziffert sein, und in Folge dessen auch die Totalsummen der einzelnen Verletzungsgruppen, und daher auch die zu gehörigen Letalitätschiffren zu klein ausgefallen sein, so ist die Differenz zwischen diesen Letalitätschiffren (Tabelle II.) und denen der Tabelle I. doch durchgehend so enorm (man berücksichtige, dass für die Oberschenkelfrakturen die Differenz zwischen 15·4 per Cent und 70·4 per Cent, und für die Kniegelenkschüsse zwischen 5·5 per Cent und 65·6 per Cent liegt)—dass, wenn die Fehlerquellen nicht gar zu beträchtlich gedacht werden, die Zahl der Schorfheilungen immer noch durch einen ansehnlichen Posten repräsentirt bleibt, und ihre Häufigkeit daher keine geringe sein kann.

Das wird zur unumstösslichen Thatsache, wenn wir in Betracht ziehen, dass (cf. Tabelle I. und II.) von den 33 Heilungen, welche überhaupt auf die notirten 96 Kniegelenkschüsse fallen, 17, also beinahe die Hälfte, durch Heilung unter dem trockenen Schorf zu Stande gekommen sind, und von den 13 Heilungen, welche auf 44 Oberschenkelschussfrakturen fallen—11, sage, also fast alle Heilungen—ebenso unter dem trocknen Schorf zu Stande gekommen sind. Das ist der Hauptgrund, auf den ich mich stütze, wenn ich meine, dass man unmöglich die gelungenen Schorfheilungen in die Kategorie der Ausnahmsfälle stellen darf.

Wenn also, gleichviel, ob mit oder ohne Recht, behauptet wird, dass in jeden Schusscanal septische Infectionsstoffe mit dem Projectil hineingetragen werden, so kann mit fraglosem Recht dagegen constatirt werden, dass dieselben keineswegs immer, wenigstens bei den Verletzungen mit unseren modernen Kleingewehr-Projectilen, ihre phlogo- und pyrogonen Eigenschaften zur Entfaltung bringen.

Immerhin aber entwickeln sich doch in einer gewissen Zahl der Fälle septische Störungen, und sogar von den mit besonderer Auswahl in Schorfbehandlung genommenen Fällen, stirbt ein Theil in Folge septischer Complicationen. Freilich ist die Letalitätschiffer (Tabelle II., 15·7 per Cent) nicht hoch. Würde dieselbe aber auch factisch noch niedriger sein, und thatsächlich ist sie wohl höher, so würden wir dennoch dem Prinzip des primären Debridements unweigerlich den Vorrang geben müssen, wenn dieses zu noch besseren Resultaten führte.

Um die Resultate dieser primären Wundreinigung beprüfen zu können, habe ich in der ersten Columne der Tabelle III. alle Schussfrakturen und Gelenkschüsse zusammengestellt, in welchen auf dem Verbandplatz oder im Feldlazareth, jedenfalls vor Eintritt septischer Reaction, also im primären Wundstadium unter allen antiseptischen Cautelen die Schusscanäle gespalten, per Debridement die Blutextravasate entfernt und die Wunden bis in die Bruchspalten und Gelenkhöhlen hinein drainirt wurden.

In dieselbe Columne sind, weil im Wesentlichen mit dem Debridement gleichwerthig, auch die partiellen und totalen Gelenksectionen gestellt worden. Von 55 Fällen haben 13 letalen Ausgang genommen. Hier 23·6 per Cent Letalität dort, bei den unter den Schorfbehandelten (Tafel II.), 15·7 per Cent. Die Differenz ist so gering, die Anzahl der Fälle so klein, dass mit Rücksicht auf obengedachte Fehlerquellen wohl nur der Schluss erlaubt ist, dass die Erfolge bei primären Debridement jedenfalls nicht auffallend günstiger sind, als bei Behandlung unter dem Schorf. Mit allem Vorbehalt dürfen wir daher wohl sagen: ein Theil der Fälle heilt sicher unter dem Schorf aus, ein anderer aber verlangt entschieden das Debridement. Ganz ohne Vorbehalt lässt sich aber aus einem Vergleich aller Columnen in Tabelle III., IV. und V., der Schluss ziehen, dass wirklich eclatante Resultate vom Debridement nur dann erwartet werden können, wenn dasselbe vor dem Eintritt septischer Reaction, also so früh als möglich, im Primärstadium vorgenommen wird.

* In Bezug auf die Sanitätsverhältnisse auf der Lazarethlinie des "Rothen Kreuzes," und der Militärlazarethe, bitte ich auf meinen Vortrag 142 und 143 in Volkmann's "Sammlung klinischer Vorträge" (1878) hinweissen zu dürfen.

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TABELLE III.

Alle, d. h. 175 *Schussfrakturen und Gelenkschüsse der Extremitäten, die auf der Lazareth-Linie des "Rothen Kreuzes" unter antiseptischen Cautelen entweder mit Debridement und Drainage der Bruchspalte* (51 *Fälle*) *oder mit Debridement und Drainage des verletzten Gelenkes* (57 *Fälle*) *oder mit partieller oder totaler Resection* (67 *Fälle*) *desselben behandelt wurden.*

			Im Primär-Stadium, d.h. vor Eintritt entzündlicher Wundreaction in antiseptische Behandlung gekommene und auch gleich auf Verbandplatz oder Feldlazareth mit Debridement, Drainage resp. Resection behandelte Fälle.				Im Intermediär-Stadium in antiseptische Behandlung gekommene und mit Debridement und Drainage resp. Resection behandelte Fälle.				Im Secundär-Stadium in antiseptische Behandlung gekommene und mit Debridement, Drainage resp. Resection behandelte Fälle.				TOTALSUMME.			
			Sa.	Geb.	Gest.	Let. p.Ct	Sa.	Geb.	Gest.	Let. p.Ct	Sa.	Geb.	Gest.	Let. p.Ct	Sa.	Geb.	Gest.	Let. p.Ct
17	Schultergelenk	Resection	6	6	0	0·0	7	3	4	57·1	1	0	1	100·0	14	9	5	35·7
		Debridement und Drainage	2	2	0	0·0	1	0	1	100·0	—	—	—	—	3	2	1	33·3
6	Oberarm	Debridement	1	1	0	0·0	5	3	2	40·0	—	—	—	—	6	4	2	33·3
31	Ellbogengelenk	Resection	9	8	1	11·1	3	3	0	0·0	8 (5)	2	3	60·0	20 (17)	13	4	23·5
		Debridement und Drainage	3	3	0	0·0	8	7	1	12·5	—	—	—	—	11	10	1	9·0
2	Vorderarm	Debridement	2	1	1	50·0	—	—	—	—	—	—	—	—	2	1	1	50·0
13	Handgelenk	Resection	2	2	0	0·0	4	3	1	25·0	5 (1)	1	0	0·0	11 (7)	6	1	14·2
		Debridement und Drainage	2	2	0	0·0	—	—	—	—	—	—	—	—	2	2	0	0·0
7	Hüftgelenk	Resection	—	—	—	—	3	0	3	100·0	—	—	—	—	3	0	3	100·0
		Debridement und Drainage	2	0	2	100·0	2	0	2	100·0	—	—	—	—	4	0	4	100·0
14	Oberschenkel	Debridement	2	0	2	100·0	8	0	8	100·0	4	2	2	50·0	14	2	12	85·7
40	Kniegelenk	Resection	—	—	—	—	8	0	8	100·0	1	1	0	0·0	9	1	8	88·8
		Debridement und Drainage	7	3	4	57·1	22	6	16	72·7	2	0	2	100·0	31	9	22	70·9
29	Unterschenkel	Debridement	12	11	1	8·3	15	11	4	26·6	2	2	0	0·0	29	24	5	17·2
16	Fussgelenk	Resection	3	2	1	33·3	3	3	0	0·0	4	1	3	75·0	10	6	4	40·0
		Debridement und Drainage	2	1	1	50·0	4	3	1	25·0	—	—	—	—	6	4	2	33·3
175			55	42	13*	23·6	93	42	51	53·7	27 (20)	9	11	55·0	175 (168)	93	75	44·6

* Todesursachen dieser 13 letal verlaufenen Fälle: 1 Mal acut progrediente Phlegmone.
4 Mal Pyämie.
2 Mal Fettembolie der Lungencapillaren.
4 Mal Erschöpfungseiterung (2 Mal mit Dysenterie.)
1 Mal Entkräftung und Inanition.
1 Mal Tetanus.

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Die Letalitätsziffer von 23·6 per Cent steigt im Intermediärstadium auf 53·7 per Cent, und im Secundärstadium auf 55·0 per Cent. Wollte man also alle Fälle prinzipiell in Schorfbehandlung nehmen, und als Indication für das Debridement nur den Eintritt septischer Reaction gelten lassen, so würden von den unter dieser Indication zum Debridement kommenden Fällen gewiss nicht so viele ausheilen, als von ihnen ausheilen könnten, wenn bei denselben oder auch nur einen Theil derselben das Debridement schon früher im Primärstadium, womöglich auf dem Verbandplatz vorgenommen worden wäre. Um günstigste Resultate zu erzielen, müssen wir also nach Zeichen suchen, welche uns schon früher, als das klinische Symptom der Intermediärschwellung es erlaubt, die Indication des Debridements aufnöthigen, und es fragt sich daher, ob solche existiren, oder, mit anderen Worten, ob wir Mittel besitzen, schon im Frühstadium von jeder Schussverletzung vorauszubestimmen, ob es bei ihr zu septischer Infiltration kommen wird oder nicht. Da nun diese Frage nicht mit einem entschiedenen "Ja" beantwortet werden kann, so wird die Frage doch wieder nahe gelegt, ob es nicht besser ist prinzipiell alle Schusswunden mit dem primären Debridement zu behandeln. Gewiss dürfte die Antwort affirmativ ausfallen, sobald wir im Stande wären, jeder gespaltenen und gereinigten Wunde einen aseptischen Wundverlauf zu garantiren. So gross die Wahrscheinlichkeit ist, dass das für die Wundflächen geringerer Ausdehnung, insbesondere die der oberen Extremitäten gelingen würde, so fraglich möchte dieser Erfolg doch für Wundflächen grösserer Ausdehnung sein. Die Schwierigkeiten für die Sicherung eines aseptischen Wundverlaufs wachsen eben auch mit der Grösse der Wunde. Und wenn es schon unter der Voraussetzung vollen klinischen Comforts, ja unter der Voraussetzung, dass dieselben 13 Oberschenkelfrakturen (Tabelle II.), welche unter Schorfbehandlung auf Verbandplatz und Feldlazareth ein Let. per Cent von 15·4 gaben, gleich, ohne Feldtransport, &c., in einem klinischen Institut mit allen Mitteln der Kunst zur Behandlung, aber mit dem primären Debridement zur Behandlung gekommen wären, fraglich ist, ob das Resultat ein so günstiges gewesen wäre, wie bei der nun factisch eingeschlagenen Behandlung unter dem Schorf; wie viel zweifelhafter müsste der Erfolg unter den Verhältnissen von Kriegslazareth und Feldtransport sein! Ich würde an demselben nicht so zweifeln, wenn es sich in allen Fällen um Querschüsse des Oberschenkels handelte; ich glaube aber den Erfolg für ganz entschieden zweifelhaft halten zu müssen, wenn zu diesen Fällen auch Schusscanäle zählen würden, welche bis zu zwei Fuss Länge, vom Knie durch die ganze Länge des Oberschenkels hin bis auf die Höhe der Glutäen reichten und Blutextravasate die Intermuscularien bis in die Wade erfüllten. Schon die Spaltung eines solchen Schusscanals selbst setzt eine ganz ungeheuerliche Wunde, zu dieser aber addirt sich noch die Wundfläche, welche die Ausräumung der Blutextravasate nöthig macht. Und auf eine sorgfältige Entfernung aller Blutextravasate scheint es wohl anzukommen, wenn ein Mal das Messer angesetzt wird. Halbe Massregeln scheinen hier nur zu schaden. Je peinlicher die Ausräumung, desto sicherer und besser das Resultat. Weil an den oberen Extremitäten dieses leichter gelingt, auch deshalb sind die Resultate primären Debridements und primärer Resection an ihnen so unvergleichlich viel besser, als an den unteren Extremitäten (Tabelle III.). Gilt überhaupt die Anschauung, dass die entzündliche Intermediärschwellung durch septische Stoffe bedingt ist, die von aussen in den Schusscanal mit dem Projectil importirt sind, so ist es nur folgerichtig, auch vorauszusetzen, dass eine gewisse Quantität derselben mit dem sich aus dem Schusscanal in die Intermuscularien ergiessenden Blut auch in diese hineingetragen wird, und daher in den Blutextravasaten selbst ihren Sitz hat. Einen Theil solcher Blutextravasate zurücklassen, heisst deshalb fast den Kranken in eine noch grössere Gefahr zu bringen, als die, in welcher er sich schon ohne Debridement befand. Das ist der Grund warum einfaches Ausspritzen der Schusscanäle mit antiseptischer

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TABELLE IV.

Quoad Vitam-Resultate aller (146) *mit Debridement und Drainage behandelter Gelenkschüsse und Schusefrakturen, d. h.* 108 *Fälle, auf der Lazarethlinie des "Rothen Kreuzes" mit antiseptischen Cautelen behandelt* (*cf. Tabelle III.:* 51 *Frakturen und* 57 *Gelenkschüsse*) 38 *Fälle, in Militärlazarethen derselben Evacuationslinie ohne antiseptische Cautelen behandelt.*

		Im Primär-Stadium Debridement und Drainage ausgeführt.				Im Intermediär-Stadium Debridement und Drainage ausgeführt.				Im Secundär-Stadium Debridement und Drainage ausgeführt.				TOTALSUMME.			
		Sa.	Geb.	Gest.	Let. per Cent	Sa.	Geb.	Gest.	Let. per Cent	Sa.	Geb.	Gest.	Let. per Cent	Sa.	Geb.	Gest.	Let. per Cent
Schultergelenk ...	Antiseptisch	2	2	0	0·0	1	0	1	100·0	—	—	—	—	3	2	1	33·3
	Nicht antiseptisch	—	—	—	—	1	0	1	100·0	—	—	—	—	1	0	1	100·0
Oberarm	Antiseptisch	1	1	0	0·0	5	3	2	40·0	—	—	—	—	6	4	2	33·3
	Nicht antiseptisch	—	—	—	—	2	0	2	100·0	4	2	2	50·0	6	2	4	66·6
Ellbogengelenk ...	Antiseptisch	3	3	0	0·0	8	7	1	12·5	—	—	—	—	11	10	1	9·0
	Nicht antiseptisch	—	—	—	—	4	0	4	100·0	3	2	1	33·3	7	2	5	71·4
Vorderarm	Antiseptisch	2	1	1	50·0	—	—	—	—	—	—	—	—	2	1	1	50·0
	Nicht antiseptisch	—	—	—	—	—	—	—	—	1	0	1	100·0	1	0	1	100·0
Handgelenk	Antiseptisch	2	2	0	0·0	—	—	—	—	—	—	—	—	2	2	0	0·0
	Nicht antiseptisch	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—
Hüftgelenk	Antiseptisch	2	0	2	100·0	2	0	2	100·0	0	0	0	—	4	0	4	100·0
	Nicht antiseptisch	—	—	—	—	—	—	—	—	3	0	3	100·0	3	0	3	100·0
Oberschenkel... ...	Antiseptisch	2	0	2	100·0	8	0	8	100·0	4	2	2	50·0	14	2	12	85·7
	Nicht antiseptisch	—	—	—	—	—	—	—	—	8	0	8	100·0	8	0	8	100·0
Kniegelenk	Antiseptisch	7	3	4	57·1	22	6	16	72·7	2	0	2	100·0	31	9	22	70·9
	Nicht antiseptisch	—	—	—	—	2	0	2	100·0	6	0	6	100·0	8	0	8	100·0
Unterschenkel ...	Antiseptisch	12	11	1	8·3	15	11	4	26·6	2	2	0	0·0	29	24	5	17·2
	Nicht antiseptisch	—	—	—	—	—	—	—	—	3	2	1	33·3	3	2	1	33·3
Fussgelenk	Antiseptisch	2	1	1	50·0	4	3	1	25·0	—	—	—	—	6	4	2	33·3
	Nicht antiseptisch	—	—	—	—	—	—	—	—	1	0	1	100·0	1	0	1	100·0

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Lösungen und Drainiren derselben—was meist übrigens ohne Durchschneiden von Muskelschichten, die sich vorgelagert haben, gar nicht möglich ist—keinen Erfolg giebt. Ist der Schusscanal inficirt, so sind auch die Blutextravasate inficirt. Ist die Reinigung des ersteren indicirt, so ist auch die Entfernung der letzteren indicirt, und deshalb hängt die Ausdehnung der von einem correcten Debridement zu erwartenden Wunde nicht nur von der Länge und Tiefe des Schusscanals, sondern auch von der Grösse und Ausdehnung der Blutextravasate ab.*

Im gegebenen Falle heisst es daher fast alle Intermuscularien des Oberschenkels spalten und drainiren, und ferner die grossen Wundflächen durch correct schliessende Verbände vor Infection von aussen schützen, nicht nur auf dem Verbandplatz oder im Divisions- und Feldlazareth, sondern auch auf den Transporten von einer zur anderen Sanitäts-Station. An und für sich unter günstigen Hospitalverhältnissen möchte es schon ein riskirtes Unternehmen sein, so grossen und ausgedehnten Wundflächen aseptischen Wundverlauf zu garantiren, wie viel gewagter muss dieses Unternehmen in Verhältnissen erscheinen, die nicht gestatten den Kranken vom Verbandtisch direct in's Bett zu tragen, und hier in passender Ruhelage sorgfältig zu pflegen, sondern mit zwingender Nothwendigkeit demselben Transporte vorschreiben und denselben in Situationen der Hülflosigkeit bringen, welche Jedem bekannt sind, der einen Eil-Transport beobachtet hat. Vis-à-vis zu den gelungenen Schorfheilungen endlich giebt man, mit dem prinzipiellen primären Debridement für die entsprechende Zahl von Fällen, Vortheile auf, welche schwerlich immer durch aseptische Heilungen einzuholen sein werden. Deshalb glaube ich, dass ein prinzipielles primäres Debridement aller und jeder Schusswunden nicht zu acceptiren ist, so lange unsere Schussverletzungen den Character bewahren, welchen sie in den letzten Kriegen gezeigt haben, und welcher sie zur Schorfbehandlung geeignet macht—und so lange wir nicht in den Besitz von Wundbehandlungsverfahren kommen, die bedingungslos und ohne Rücksicht auf Grösse und Ausdehnung der Wunde solide Heilungen sichern. Es ist das primäre Debridement also nur bedingt zulässig, das heisst, in Fällen, welche keine Chanzen für die Schorfheilung zu besitzen scheinen, oder, mit anderen Worten: ein Theil der Fälle verlangt entschieden die Schorfbehandlung, ein anderer Theil unbedingt das primäre Debridement. Und factisch liegen nun in Praxis die Verhältnisse meist so, dass, mit Rücksicht auf Mangel an Zeit, Kräften und Mitteln an ein primäres Debridement für alle Schussverletzungen gar nicht gedacht werden, dass nur einer beschränkten Zahl von Fällen dieser Modus der Hülfeleistung zugedacht werden kann. Die Fälle, welche am dringendsten dieser Hülfe bedürfen, welche für die Heilung unter dem Schorf keine Chanzen haben, finden und zu erkennen, ist daher auch eine unserer ersten Aufgaben. Eine präcise Erfüllung dieser Ausgabe ist, wie schon oben besprochen, nicht möglich, weil auf das einzige sichere Zeichen des Import's septischer Stoffe, auf den Eintritt der Intermediärschwellung nicht gewartet werden darf. Die Erkennungszeichen, deren wir uns bedienen dürfen, sind nur äusserliche, nicht absolut zuverlässige. Dieselben sind mehr positiver Natur für die Fälle, welche das primäre Debridement verlangen, und sind daher weniger unzuverlässig, als die Zeichen rein negativer Natur, deren wir uns bedienen müssen, um die Fälle zu erkennen, welche die meisten Chanzen für die Schorfheilung besitzen. Dort besitzen wir sichere Hinweise dafür, dass die Wunde ohne Entzündung und Eiterung und dess-

* Der Gedanke ist gewiss nahegelegt, durch häufigeren, frühzeitigen prophylactischen Gebrauch des Esmarch'schen Stranges den Bluterguss auch bei diesen Verletzungen zu beschränken; nur müssten die Blessirtenträger in der Handhabung desselben unterrichtet sein und eine grössere Zahl derselben zur Hand haben. Prof. Esmarch schlägt zu diesem Zweck bestimmte Hosenträger vor; ich liess meine Leute den Gummischlauch als Hosengurt benutzen.

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TABELLE V.

Quoad Vitam-Resultate aller (114) *Gelenkresectionen, d. h.* 67 *Fälle* (*cf. Tabelle III.*) *auf der Lazarethlinie des "Rothen Kreuzes" mit antiseptischen Cautelen behandelt;* 47 *Fälle in Militärlazarethen derselben Evacuationslinie ohne antiseptische Cautelen behandelt mit den Letalitätschiffren der aus dem deutsch-französischen Kriege bekannt gewordenen Gelenkresectionen* (*Gurlt. die Gelenkresectionen nach Schussverletzungen* 1879 *Seite* 1216).

		Im Primär-Stadium ausgeführte Resectionen.				Im Intermediär-Stadium ausgeführte Resectionen.				Im Secundär-Stadium ausgeführte Resectionen.				Totalsumme.			
		Sa.	Geh.	Gest.	Let. per Cent.	Sa.	Geh.	Gest.	Let. per Cent.	Sa.	Geh.	Gest.	Let. per Cent.	Sa.	Geh.	Gest.	Let. per Cent.
Schultergelenk	Antiseptisch	6	6	0	0.0	7	3	4	57.1	1	0	1	100.0	14	9	5	35.7
	Nicht antiseptisch	1	1	0	0.0	—		—	—	6	3	3	50.0	7	4	3	42.8
	Summa beider Gruppen	7	7	0	0.0	7	3	4	57.1	7	3	4	57.1	21	13	8	38.9
	Gurlt 1870–71	31			41.9	47			50.0	291			41.1	431			36.7
Ellbogegenlenk	Antiseptisch	9	8	1	11.1	3	3	0	0.0	8(5)	2	3	60.0	20(17)	13	4	23.5
	Nicht antiseptisch	—	—	—	—	2	1	1	50.0	13	5	8	61.5	15	6	9	60.0
	Summa beider Gruppen	9	8	1	11.1	5	4	1	20.0	21(18)	7	11	61.1	35(32)	19	13	40.6
	Gurlt 1870–71	45			28.0	42			28.0	364			28.0	511			25.0
Handgelenk	Antiseptisch	2	2	0	0.0	4	3	1	25.0	5(1)	1	0	0.0	11(7)	6	1	14.2
	Nicht antiseptisch	—	—	—	—	1	1	0	0.0	7	4	3	42.8	8	5	3	37.5
	Summa beider Gruppen	2	2	0	0.0	5	4	1	20.0	12(8)	5	3	37.5	19(15)	11	4	26.6
	Gurlt 1870–71	1			0.0	4		3	75.0	25			28.0	42			23.8
Huftgelenk	Antiseptisch	—	—	—	—	3	0	3	100.0	—	—	—	—	3	0	3	100.0
	Nicht antiseptisch	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—
	Summa beider Gruppen	—	—	—	—	3	0	3	100.0	—	—	—	—	3	0	3	100.0
	Gurlt 1870–71	1			100.0	5			80.0	32			93.7	39			92.3
Kniegelenk	Antiseptisch	—	—	—	—	8	0	8	100.0	1	1	0	0.0	9	1	8	88.8
	Nicht antiseptisch	—	—	—	—	—	—	—	—	1	0	1	100.0	1	0	1	100.0
	Summa beider Gruppen	—	—	—	—	8	0	8	100.0	2	1	1	50.0	10	1	9	90.0
	Gurlt 1870–71	20			55.5	6			83.3	46			47.0	75			80.0
Fussgelenk	Antiseptisch	3	2	1	33.3	3	3	0	0.0	4	1	3	75.0	10	6	4	40.0
	Nicht antiseptisch	—	—	—	—	2	2	0	0.0	14	5	9	64.2	16	7	9	56.2
	Summa beider Gruppen	3	2	1	33.3	5	5	0	0.0	18	6	12	66.6	26	13	13	50.0
	Gurlt 1870–71	0	0	0	—	6			33.3	90			45.6	99			40.4

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halb ohne Debridement schwerlich ausheilen kann, hier ist von sicheren Hinweisen für einen reactionslosen Verlauf und eine aseptische Heilung keine Rede; dort ist die septische Reaction ohne Debridement fraglos, hier bleibt auch im scheinbar günstigsten Fall der aseptische Verlauf immer fraglich. Da jedoch der summarische Erfolg, welchen ich bei solchem mit Auswahl und nur bedingungsweise vorgenommenem primären Debridement erzielte, überaus günstig genannt wird, und, wie ich glaube, auch mit Recht günstig genannt wird (es kann hier natürlich nur von den im Primärstadium, aber allen, mit Schorf und Debridement in Behandlung gekommenen Fällen die Rede sein), so müssen die Momente, an welche ich mich bei der Wahl und Entschliessung für primäres Debridement und Schorfbehandlung hielt, so wenig dieselben absolut zuverlässig sein können, doch immerhin brauchbar sein. Die Momente, an welche ich mich im Allgemeinen hielt, und die Gesichtspunkte, von welchen aus ich handelte, sind kurz folgende: Klaffte bei einer Schussfraktur oder einem Gelenkschuss der Wundcanal so weit, dass Bruchspalte und Fragmente oder Theile der Gelenkfläche des verletzten Gelenkes offen zu Tage lagen oder bewiesen gurgitirende Geräusche die Anwesenheit von Luft in den tieferen Theilen von Schusscanal und Gelenk, oder waren die Schusscanäle sogar sichtbar durch Tuchfetzen, Stroh, Koth, &c., verunreinigt, so wurde Debridement und Drainage, wo nöthig, mit Resection des Gelenkes für indicirt gehalten.

Waren die Schussöffnungen klein und erschienen sie rein, oder waren dieselben auch gross, aber war der Wundcanal selbst nicht offen, war derselbe durch Muskelschichten verlegt und in demselben weder Luft nachweisbar, noch sonst eine Verunreinigung sichtbar oder wahrnehmbar, hatte die Verletzung eine gewisse Aehnlichkeit mit den, doch auch nicht ohne bestimmte dringende Indication dem Debridement unterworfenen, einfachen uncomplicirten Stichverletzungen, so wurde die Behandlung unter dem trocken Schorf eingeleitet. Bei so vager Indicationsstellung für die Schorfbehandlung kann es natürlich nicht ausbleiben, dass von den zur Schorfheilung bestimmten Fällen ein gewisses Contingent doch septische Reaction zeigt, und dann erst im Intermediärstadium zum Debridement kommt, selbstverständlich mit fraglichem Erfolg, wie auch aus Tafel II. ersichtlich. Wie es uns aber nicht möglich schien, unter den günstigsten klinischen Verhältnissen mit primärem Debridement allen Schussverletzungen ohne Ausnahme aseptischen Verlauf garantiren zu können, so ist es auch bei diesen Grundsätzen nicht möglich für alle Fälle der Entwicklung septischer Prozesse vorzubeugen. Der Krieg verlangt seine Opfer, und aus der Zahl der Verwundeten stellt ihm diese in grösste Ziffer der septische Prozess. Auf dem betretenen Wege scheint die Zahl dieser Opfer auf das kleinste Minimum reducirt werden zu können.

In einem nächsten Kriege würde ich übrigens mit dem primären Debridement an bestimmten Extremitäten-Abschnitten noch vorsichtiger sein.

Als oberste und erste Indication für das primäre Debridement würde mir freilich unverändert gelten: jede, vom Gesichtspunkt der Antiseptik aus, am Schusscanal wahrnehmbare Verunreinigung (zu Tage liegen der Bruchspalte, des Fragments, der Gelenkflächen, Luft im Canal, Tuchfetzen, Stroh, &c. &c.). Ich würde aber dieser Indication nur nachkommen, wenn sich noch zwei Bedingungen erfüllbar erweisen: 1, dass alle eventuell nöthig werdenden Eingriffe unter streng antiseptischen Cautelen ausgeführt werden können, und 2, dass mit einem gewissen Grade von Wahrscheinlichkeit vorausgesehen werden darf, dass auch für die fernere Pflege es nicht an der Möglichkeit fehlen wird, alle die Forderungen zu erfüllen, welche ein aseptischer Verlauf mehr oder weniger stellt. Ich denke hiebei ganz besonders an Transporte, auf welchen grosse Wundflächen unter antiseptischem Schutz zu halten, fast ganz unmöglich ist. Ein Blick auf die Tabelle III zeigt wie ungünstig das primäre Debridement bei den Frakturen des Oberschenkels verlief. Das ausgedehnteste Debridement an der oberen Extremität, ja wenn die Wunde

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sich sogar vom inneren Rande der Scapula bis in die Mitte des Oberarms erstreckt, und Theile von Scapula und Humerus resecirt sind, nimmt noch günstigen Verlauf, wenn der Patient auch gleich nach dem Eingriff vom Verbandplatz auf zehn Kilometer Distanz und mehr rückwärts in's Feldlazareth transportirt wird—weil an diesem Körperabschnitt sich die Wunden gut occludiren und auch auf dem Transport gut occludirt halten lassen, und ganz besonders, weil der Patient selbst im Stande ist, die Lage des Verbandes zu controlliren, nachzuhelfen, &c.; ebenso günstig verläuft, aus gleichen Gründen, die ausgedehnteste Verwundung am Unterschenkel;—nicht aber trifft der gewünschte Erfolg ein, wo es sich um ausgedehnte Wundflächen am Oberschenkel und Hüfte, nicht selten am Kniegelenk handelt. Derselbe kann ja auch nicht erwartet werden, wo man gleiche Wundflächen schon im klinischen Krankensaal nicht mit trocknen Verbänden aseptisch halten kann, und deshalb zu permanenter Irrigation greift, und nun gar hier der Kranke im Transportwagen hin und her geworfen wird und nicht ein Mal im Stande ist selbst seine Lage zu corrigiren. Deshalb würde ich, so sehr es mir im Allgemeinen richtig scheint, dass, wo die Wunde an und für sich das Debridement verlangt, dieses auch so früh als möglich, womöglich schon auf dem Verbandplatz (first dressing station) vorgenommen wird, bei Frakturen des Femur und Hüftgelenkschüssen nicht eher das Debridement ausführen, als bis ich annehmen darf, dass für die nächsten Tage kein Transport in Aussicht steht, und deshalb das Debridement dieser Verletzung en eo ipso vom Verbandplatz auszuschliessen.

Ganz ebenso würde ich mit den Schussfrakturen des Kniegelenks verfahren, welche voraussichtlich ein bedeutenderes Debridement verlangen; ja ich würde in solchen Fällen die primäre Amputation auf dem Verbandplatz häufiger ausführen, als ich es im letzten Kriege gethan habe.

Bei den Frakturen und Gelenkschüssen der oberen Extremität, an Fuss und Unterschenkel würde ich anstandslos das primäre Debridement auf dem Verbandplatz vornehmen, und meine, dass es, wo nur immer das Personal gut geschult ist und einheitlich geleitet wird, und die nöthigen Vorbereitungen getroffen sind—meist möglich sein muss. Gewiss können aus dem Gange einer Schlacht Schwierigkeiten für die Erfüllung dieser Aufgaben erwachsen, auf der Seite des Siegers, wie auf der des Besiegten. Gewiss können Clima, Jahreszeit, Oede des Terrains, insbesondere Frost und volle Absenz jeder Cultur auf dem bekriegten Rayon—jeden Versuch activer Therapie unmöglich machen. Ich habe selbst in solch ungünstigen Verhältnissen, in der Nacht des 9. November, bei der Erstürmung von Kars gestanden, und glaube die Bedeutung derselben nicht zu unterschätzen. Es fror 7° R. unter O.; Wasser gefror im Eimer, und, um es zu wärmen, fehlte es an Holz. Die armenische Hochebene ist auf Hunderte von Kilometern baum- und strauchlos. Vorhandenes Brennmaterial war im Laufe der sechsmonatlichen Belagerung verbrannt worden; ja zuletzt waren das Holzwerk der Erdhütten (Zakli), die das Dach haltenden Strebebalken zur Feuerung verwandt worden; damit aber auch die Erdhütten ihrer schützenden Dächer beraubt worden. Andere Wohnlichkeiten existiren in den armenischen Dörfern der unmittelbaren Umgebung von Kars nicht, und so mussten die ersten Verbände in diesen ungeschützten Erdgruben angelegt werden, ohne die Möglichkeit die Kranken zu entkleiden, die Wunden zu reinigen—geschweige denn auch nur ein radicales Debridement vornehmen zu können. Solche Schwierigkeiten, solch' fast unüberwindliche Hindernisse können sich also unserer Arbeit in den Weg stellen. So ungünstige Situationen sind aber, wie die neueren Kriege zeigen, doch nur selten. Für die gewöhnlichen, nicht ausnahmsweisen Verhältnisse, genügt es nur die einfachen Vorbereitungen zu treffen, um genügend mit Wasser, Antisepticis und Verbandzeug versorgt zu sein; und dazu haben sich sonst noch immer Mittel und Wege gefunden. Es handelt sich nur darum, dass man die Vorbereitungen trifft, und im Prinzip der Behandlung eine gewisse Einheitlichkeit herrscht.

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PROCEEDINGS ON CLOSING THE SECTION.

Dr. FERRADAS Y RODRIGUEZ, Madrid: handed in a letter, which he requested might be read by one of the secretaries, expressing his appreciation of all he had heard and seen in the Section.

Surgeon General ROTH, Dresden: Gentlemen,—Allow me to express a feeling, I am sure, common to the whole meeting. At our first meeting we were welcomed in so friendly a way by our respected President, and he has discharged his duties all the time with so much kindness and impartiality, that we feel thankful to Surgeon General Longmore for having taken the chair during our session. I propose a vote of thanks to the President, Surgeon General Longmore.

Medical Director J. M. BROWNE, United States Navy: said: Gentlemen,—I rise to perform a pleasurable duty—to second the proposition of Surgeon General Roth. I do this in all honesty, fully appreciating and gratefully recognizing the wisdom that occasioned the selection of Surgeon General Longmore to preside over the section of military surgery and medicine. How well he has performed the duties pertaining to his office, you well know. In the discharge of this duty he has exercised courtesy, urbanity, ability, and efficiency. Our thanks are due to him for the valuable paper he has presented, for the happy manner in which he has summed up and extracted from and given to us the essence of the discussions, his logical deductions, and especially for his earnestness in purpose and love for all that would promote knowledge and enhance our profession. Many of us before we came hither only knew our worthy chairman by reputation; we knew in what esteem and consideration he was held by various communities—and now, as we go hence, we can fully understand how well he has merited his honours, and how nobly entitled we believe him to be to yet future honours. As we go hence, we shall carry with us into the unknown future, among the pleasant remembrances of the International Medical Congress, that which will be ever green and fragrant in memory, our association and comradeship under the administration and direction of Surgeon General Longmore.

The PRESIDENT said: I am greatly gratified by the very kind and flattering terms in which Inspector General Dr. Ferradas, of Spain; Surgeon General Dr. Roth, of Saxony; and Medical-Director Dr. Browne, of the United States; have spoken of the manner in which I have discharged the duties of President of this Section, and I am much indebted to you all, gentlemen, for the way in which you have accepted these expressions regarding me and my office. Such service as I have been able to render in the chair has been shorn of most of its difficulties; in the first place, by your kind reception of me at the commencement of our meetings, and, in the second, by your constant assistance and support during the whole period they have been in progress. It is really yourselves who deserve thanks for the success which has attended our deliberations. You have come from all the leading countries in the world; you have freely opened to us the stores of your varied experience; and you have not merely exhibited interest, but you have taken an active part in the work of our section. If we have not been able to arrive at definite conclusions, or a universal agreement, upon some of the subjects which have been under discussion, it should be remembered how relatively short the time at our disposal has been, and how numerous the questions brought forward for consideration; and, at any rate, all will have heard facts related and opinions expressed by many of the most competent naval and military surgeons of the public services of Europe and the United States, and this experience and the conclusions based upon it by the observers concerned

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will be valuable material in our possession for purposes of study and reflection. Let us hope that the seed thus sown may acquire a vigorous growth and bear useful fruit in the future! I must also express our acknowledgments to our Honorary Secretaries, Dr. Lloyd and Mr. Myers, who have had a great deal of work thrown upon them, and who have been indefatigable in its execution. These gentlemen have been actively engaged for a long time past in conducting correspondence regarding the papers to be read and in making other preparations for our meetings, and they have given constant attention to the current business, while our meetings have been in progress. We are greatly indebted to our Secretaries for the success of our sectional meetings. Finally, in closing the Section, and bidding those who have taken part in its business "Farewell," allow me, while again thanking you for the courteous and continued support you have given me in my position as chairman, to express a hearty wish that all of you, particularly our foreign visitors, may have a prosperous return homewards, and the happiest of reunions with the friends and relatives from whom you have been parted while attending the London International Congress of 1881.

A vote of thanks to the Secretaries was proposed by Mr. Casson, and seconded by Surgeon General Mouat, V.C., C.B.

Dr. Lloyd returned thanks for his colleagues and himself.

The proceedings then terminated.

Visit to Aldershot.

On Wednesday, August 10th, a party of about forty members of the Congress, chiefly medical officers of foreign armies, who had attended the meetings of Section 14, went by an early train to Aldershot Camp to inspect the Training Depôt of the Army Hospital Corps, where also, under the new regulations, the medical officers go through a course of drill and special training after having left Netley.

The party was accompanied by Surgeon Myers, Coldstream Guards, one of the Secretaries of the Section, and on reaching the camp was taken in charge by Surgeon-Major Sandford Moore, A.M.D., commanding the Depôt, also a Secretary.

The men of the Depôt were first formed up in line, at open order, under the command of Surgeon Major Moore, and received the visitors with a general salute, after which the ranks were inspected, and a march past in column of companies took place. The class of young medical officers were then put through sword and rifle exercise. A party of men of the Army Hospital Corps satisfactorily went through stretcher, litter, and cacolet exercise. A bearer company was next formed up at the end of the parade ground for bearer company exercise, removing the supposed wounded from a supposed battlefield to the dressing stations in rear, and applying the necessary dressings.

The parade was then dismissed, and the visitors, having expressed their satisfaction at the day's proceedings, were entertained at luncheon at the officers' mess of the Army Medical Department, by Surgeon General Fasson and the officers of the Department.

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